THE

ELECTRICITY,
MAGNETISM, AND CHEMISTRY;

AND

Guardian of Experimental Science.

CONDUCTED BY

WILLIAM STURGEON,

Lecturer on Experimental Philosophy, at the Honourable East India Company's Military Seminary, Addiscombe, &c. &c.

AND

ASSISTED BY GENTLEMEN EMINENT IN THESE DEPARTMENTS OF PHILOSOPHY.

VOL. IV.—JULY, 1839, TO APRIL, 1840.

London:
Published by Sherwood, Gilbert and Piper, Paternoster Row;
and Bancks and Co., Exchange Street, Manchester.
Sold also by Messrs. Hodges and Smith, and Fannin and Co.,
Dublin; Maclachlan and Stewart, and Carfrae and Son,
Edinburgh; Mr. Robertson, Glasgow; Mr. Smith, Aberdeen;
and Mr. Hobson, No. 108, Chesnut Street, Philadelphia.

1840.

Uttarpara Jaikrishna Public Library
Accn. No. 31490 Date 03.8.04

WHITE AND CARTER, PRINTERS, MANCHESTER.

INDEX.

A.

PAGE

Alexander, Professor, on the Aurora Borealis 404
Amalgam for Electrical Machines 80
Atmospheric Electricity 248
Aurora Borealis 403, 404

B.

Barium, on the extrication of 356
Barker, Charles, Esq. on Electrical apparatus 221
Beagle, H. M. S. supposed to be struck by lightning 171
Beccaria, Father Giamsbabtista, on Vindicating Electricity 379
Becquerel, M. on the temperature of Vegetables 212
Bregnot, M. on a thunder Clap 215
Brewster, Sir David, On the colours produced by mixed plates 250

C.

Calcium, on the extrication of 356
Callan, the Rev. N. on Electro-Magnetism 333
Chemical forces 137
Chloride of Zinc and Alcohol, on the action of 205
Colours produced by mixed plates 250
Compass, on the variation of 101
Coward, B. W. Esq. on an air thermometer 402

D.

Daguerre, M. Secret of 225
Decomposition of Water, by growing plants 25
Dippingneedle 359
Doppler, Professor, on Electric tension 376
Dogs restored to life by Galvanism 481

E.

Electric Coils 256
Electric Currents 240
——— Kite Experiments 181
——— Tension 376
Electrical apparatus 221
——— Machine in Royal Victoria Gallery, Manchester, Description of 503
Electro dynamics 281
Electricity, Researches in 1
Electricity and Magnetism, Researches in 33, 137, 161
——— and Vegetation, their connexion 421
Electro air thermometer 402
——— Magnets made of iron wire 58
——— Magnetic Engines 203
——— Magnetic Forces, by J. P. Joule, Esq 474
Electrometer 8
Electrometers 145
Electro-type 258
Electro-vegetation 241
Ether, a new one, by Dr. Hare 70

PAGE
Faraday Dr. F. R. S. his Researches in Electricity 1, 81, 229
Fitzroy, Captain Robert, on Lightning 171
Formula for two Chemical bases 360
Frodsham, W. J. Esq. F. R. S. on Vibrations of Pendulums 365
Fulminating Powder 358

G.

Galvanic experiments on the body of an executed murderer 78
Galvanism applied to ignite gunpowder 359
Gas, on the illuminating power of 62
Gelatine, used as food 207
Green, Lieutenant W. Pringle, on Lighting Conductors 229
Grenville, M. on the Illumination of Gas .... [illegible]
Guggsworth, Julian, on a curious Electrical phenomena 234

H.

Halse, W. H. Esq. on secondary Electric coils 256
—————— on Voltaic action 500
—————— Restores Dogs to life by Galvanism .... 48[illegible]
Harden, Dr. J. M. B. on Chemical Formular 360
Hare, Dr. Robert, on the fusion of Platina 70
—————— on the extrication of Barium, Strontium and Calcium 356
—————— on Tornadoes 393
—————— on Fulminating Powder 358
Hare, Mr. Clarke, on the reaction of Sulphuric Acid with Essential Oil of Hemlock 362
Harper, J. Esq. on Electrical phenomena 335
—————— Spotted Jar, &c 335
Harris, W. Snow, Esq. F. R. S. on Lightning Conductors 163, 310, 481
—————— his Letter to Mr. Sturgeon 325
Hemlock, the Essential Oil of, with Sulphuric Acid 362
Henry, Professor, on Electro-dynamics 281
Hoskins, Dr. S. E. on Galvanic Batteries 61
Hydrogen Gas, on a mode of distinguishing the Arsenuretted from the Antimoniuretted 331

I.

Irradiation, note on 353

J.

Joule, J. P. Esq. on the use of Electro-magnets, made of iron wire . 58
—————— on Electro-magnetic Engines 471
—————— on the Laws of Electro-magnetic action ....
—————— Investigations in Magnetism, Electro Magnetism, &c. 131

K.

Kuhlman, Mr. Frederic, on the Reaction of Spongy Platina 157

L.

Lateral discharges 171
Lenz, M. on the Wollaston's Battery 231
Lightning Conductors 163, 310 481
—————— Expense of for Shipping 130, 187, 189
—————— Struck H.M S. Rodney 166
—————— the Effects of 166, 171, 235, 372
—————— Supposed Effects of 327, 329
Lussac, M. Guy, on Chemical Forces 137

M.

Magendie, M. on Paralysis and Nervalogie of the Face 210
Magnetic Electrical Machine 66
——————Investigations, by the Rev. W. Scoresby, F.R.S. 73
Magnetism and Electro Magnetism 131
Magnetism, Terrestrial 432

PAGE

M.

Magnetical Instruments, &c. 451
Magnetometer, Declination 452
Marine Lightning Conductors, Mr. Sturgeon's 183
Marsh, Mr. James, on a Method of Detecting Arsenic in Poisons 335
Max, Professor, do. 335
Masson, Mr. on the Action of Chloride of Lime on Alcohol 205
Medallions formed by Voltaic Action 279
Microscopic Objects Method of Illuminating them 407
Meteor [illegible] Sandwich, on a 305

N.

Naylor, Lieut Edward, on Terretsrial Magnetism 104
Noad, H. M. Esq. his Lectures 73
Neef, Dr. his Magnetic Electrical Machine 66

O.

Observations on Dr. Faraday's eleventh Series 231
Oxyhydrogen Blow Pipe, by Mr. Weekes 192

P.

Paralysis and Nervalogie of the Face 210
Parrot, M. on Electricity 214
Pendulums, on the vibrations of 365
Peltier, M. on Electricity 214
Perry Professor Thomas, on the adjustment of Dipping Needles 359
Photography 225
Pine, Thomas, Esq on Electro-vegetation 241
———— on the connexion between Electricity and Vegetation 121
Plateau, M. on Irradiation 354
Potassium obtained by Voltaic Action 80

R.

Read, the Rev. J. B. on Illuminating Microscopic Objects 107
Reviews of Books 73, 237, 503
Rich, Mr. Charles, on the Effects of Lightning 372
Roberts, Martyn Esq. his Voltameter 401

S.

Scientific Expedition to the Antarctic Regions 431
Scoresby, the Rev. W. his Magnetic Investigations 73
Strontian, the extrication of 356
Sturgeon William, his Experimental and Theoretical Researches in Electricity and Magnetism 33, 137, 161
———— his Letters to Mr. Harris 322, 414, 496
———— on the various methods of obtaining *fac simile* Medallions by Voltaic Electricity 279
———— on the Aurora Borealis 403
Sullivan Lieut, on Lightning 327

T.

Temperature of Vegetables 212
Thunder, Storms 80, 215, 217, 393, 397
Tornadoes 393
Turner, Captain, on Lightning 171

V.

Vindicating Electricity 379
Voltaic Batteries 502
Voltaic Reaction, on, by Mr. W. R. Grove 503
Voltameter, Description of a new one 401

W.

Weekes, W. H. Esq. on the decomposition of Water by growing plants. 25
———— Memoir on the Oxyhydrogen Blow Pipe 192
———— on the Analysis of different kinds of wood 391
———— on Vegetable Conductors 246

ERRATA.

Page 29, top line, for 8, A.M. *read* 8, P.M.

Page 198, line nine from bottom of text, for Pepy's *read* Pepys.

Note on same page, line fourth, for T. O. N. Rutter *read* J. O. N. &c.

Page 200, line seventeen from top, for Pepy's *read* Pepys.

Page 397, for Lieut. Sutway *read* Lieut. Sullivan.

Page 499, for Bakersian *read* Bakerian.

ANNALS OF ELECTRICITY

VOl IV Plate X

Fig 1

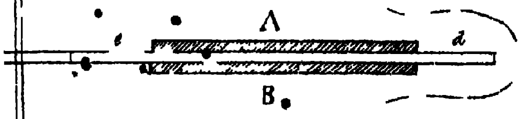


Fig 2

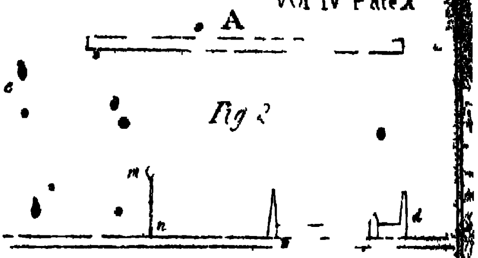


Fig 7

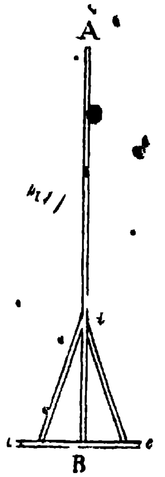


Fig 3

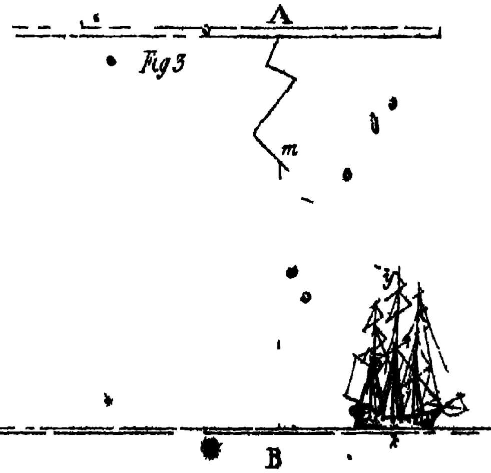


Fig 8

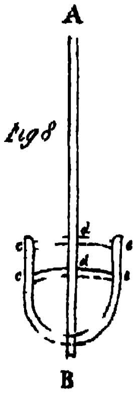


Fig 4

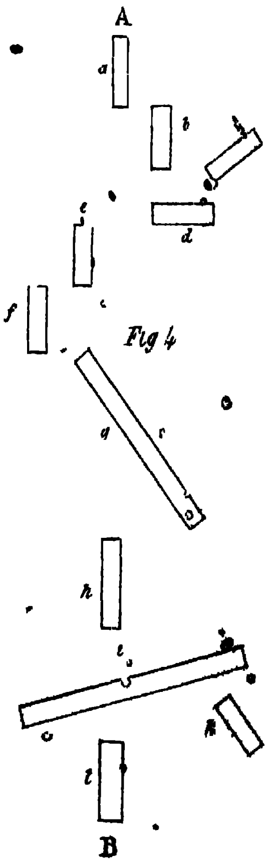


Fig 5

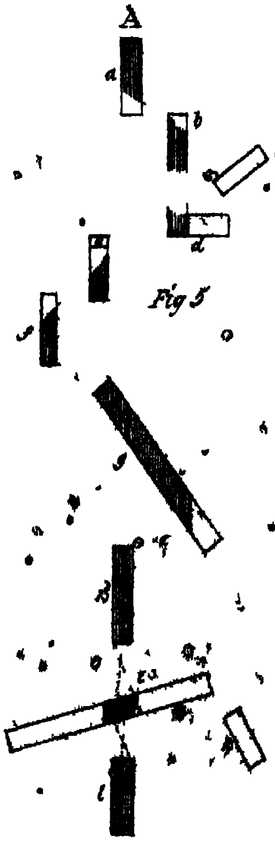
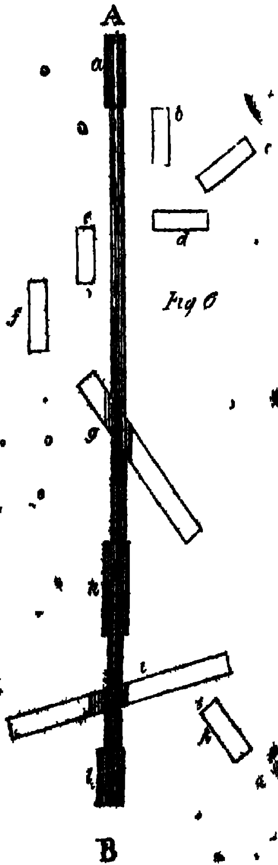
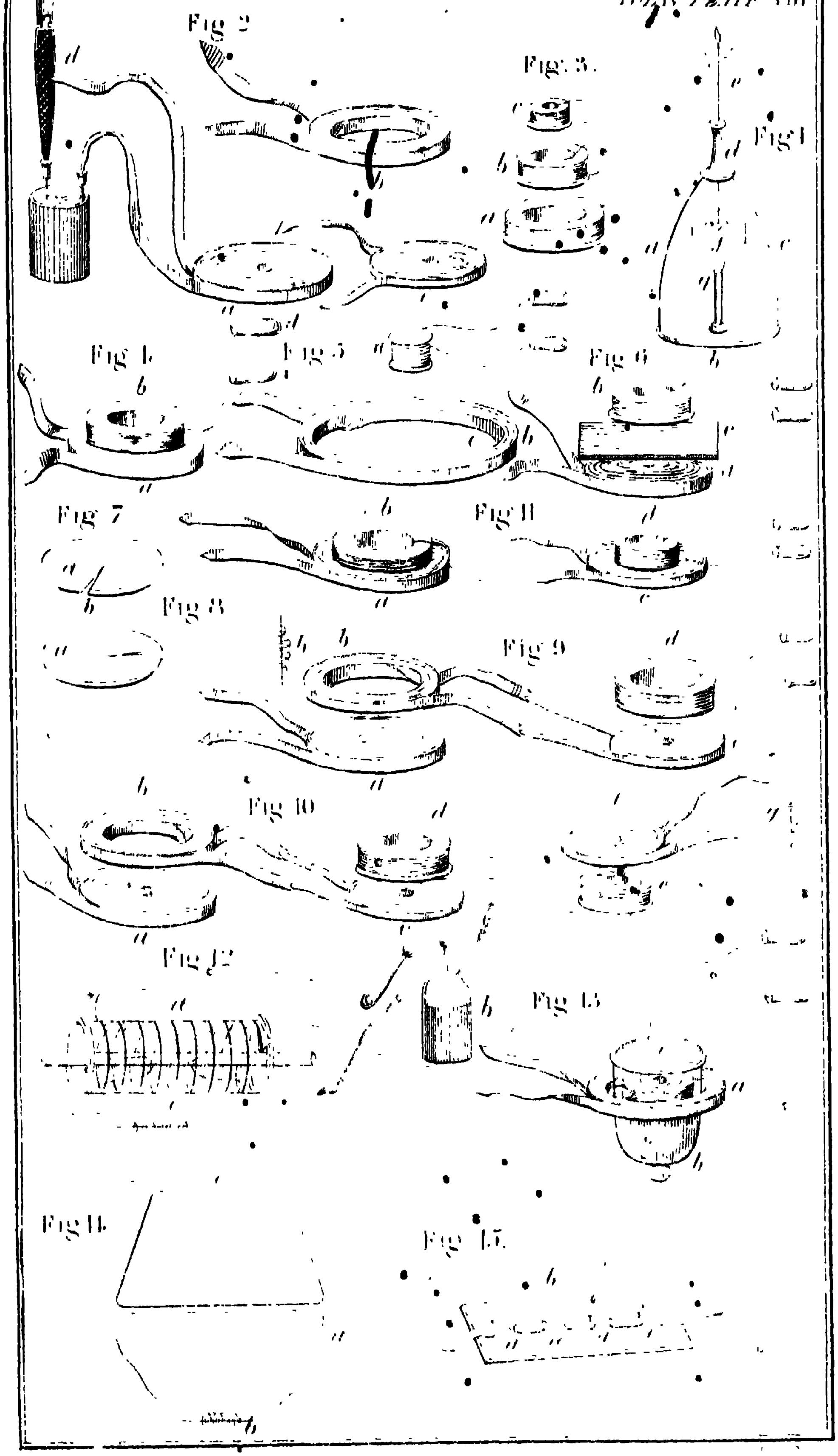


Fig 6



ANNALS OF ELECTRICITY &c. VOL IV PLATE VIII



ANNALS OF ELECTRICITY, &c.

VOL. IV. PLATE VII.

Fig 1

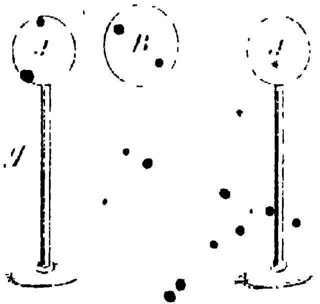


Fig 2.

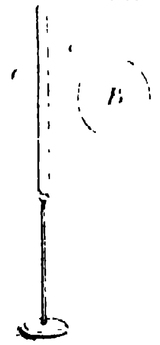


Fig 3

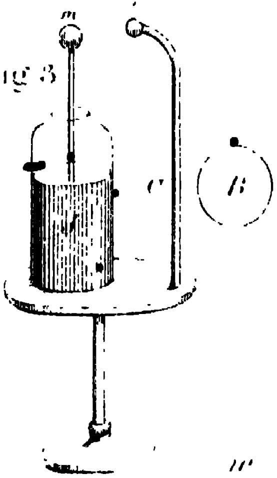


Fig 4.

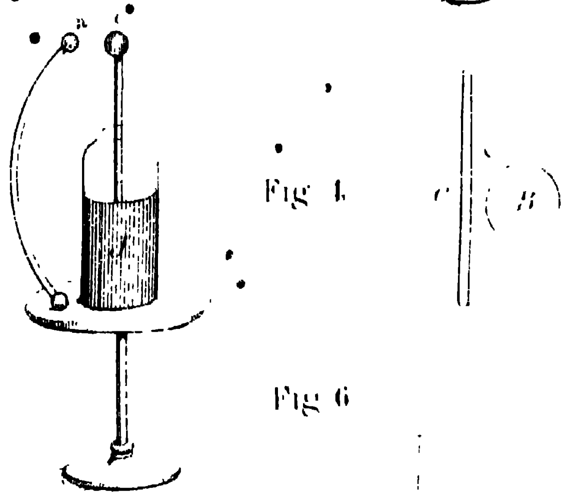


Fig 5

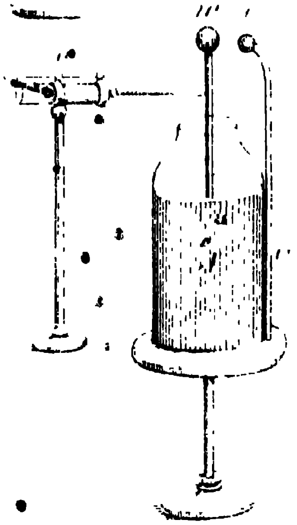


Fig 6

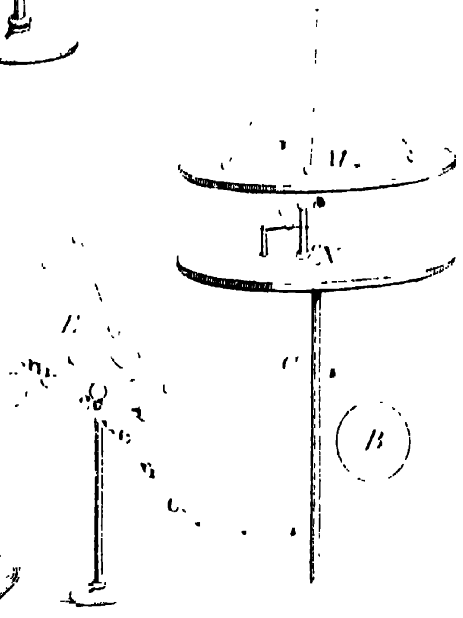
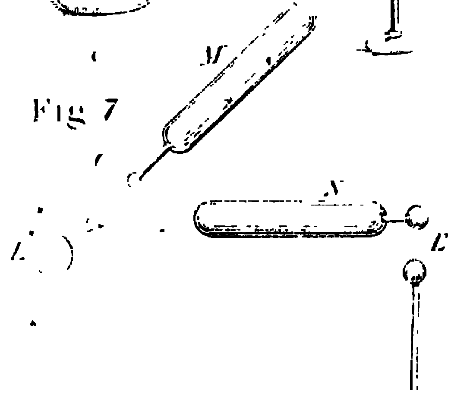


Fig 7



OOTTERPARAH PUBLIC LIBRARY 1859

THE ANNALS
OF
ELECTRICITY, MAGNETISM,
AND CHEMISTRY;
AND
Guardian of Experimental Science.

JANUARY, 1840.

XXXIII. *On the connexion between Electricity and Vegetation.* *By* Thomas Pine, Esq., *Maidstone.*[*]

Reflecting on the known properties of the electric fluid, and the suitability of plants in their relation to the surrounding elements to receive its influence, I was led to regard it as highly probable that vegetation depends much on this prinple. From the experiments of Mr. Cavallo, as recently confirmed by those of Mr. Sturgeon,[†] I had learnt that the air is in a constant state of positive electricity; hence it seemed reasonable to conclude that the acute extremities of plants in a living growing state, must be constantly imbibing some portions of the fluid, and introducing it into their substance. I expected that their attractive energy must be considerable, as the mutual arrangement has much the appearance of an immense apparatus in constant operation; and was strongly confirmed in my conclusion by observing that a common blade of grass, when presented to the prime conductor of an electrical machine, gave evident proofs of a more potent attractive and conducting power than appeared in a corresponding metallic point; the fluid appearing to flow toward it with less obstruction, with a more uniform current, exhibiting a much brighter light, and at considerably greater distances. In some experiments made in the month of June, a metallic point and a vegetable point being held equidistant from the prime conductor, the vegetable point continued to be illuminated till it had reached at least four times the distance at which the metallic point ceased to exhibit any light, that is at the distance of about fourteen feet. A corresponding effect attends the passing of the contents of a charged jar through a vegetable point; as in this case, the human body being a part of the circuit, the jar will be discharged with almost no perceptible effect on the animal frame, yet leaving hardly any residuum; whereas if a metallic point be employed, the shock will be more sensibly felt, and the residuum more considerable.

---

[*] Communicated by the Author.

[†] Mr. Sturgeon favoured me with a most obliging letter containing ample evidence from his numerous experiments both of the general fact and of many interesting particulars relating to the subject, the contents of which, I trust, will appear in confirmation of the above statement.

Hence it follows that vegetable points must be acting with a great and continued energy upon the electricity of the atmosphere, either in imparting to it the electric matter which it uniformly contains, or in imbibing the fluid from the atmosphere, which must be as constantly afforded to it from some other source. The latter conclusion appears by far the more probable; since it is impossible to account for continual supplies of electric fluid from an earth in a constant state of negation with respect to its atmosphere; whereas the atmosphere is constantly receiving solar rays which possess *some* if not *all* the properties of electric matter. There are so many points of resemblance, if not identity, between the phenomena respectively ascribed to light, caloric, and electricity, that much fewer difficulties will probably be found to attend the hypotheses that they are but different effects, arising from one common cause or source, than from the conclusion that they are produced by so many distinct, yet all-pervading, fluids. If the ball of a thermometer be electrified, by first moistening it for an exterior coating, while the interior coating is formed by the mercury, a stream of light will shoot through the vacuum to the summit of the tube, showing that the fluid is essentially luminous. Is it not then essentially the same with light, and does not the latter possess electrical properties in common with the former? If this be admitted, it appears to me that vegetation, through every stage of its progress, from the germinating seed to the full grown and perfected plant, will be found admirably to accord with such influences from the sun, whether by his direct rays, or through the instrumentality of the air and vapours.

The several varieties of form and properties to which plants are subjected in their progress, seem adapted to corresponding electric influences from the respective elements.*

---

* I venture to use the old term, not having a better, to designate the principal divisions of our atmosphere, though two of them are now well known to be compound bodies.

Thus the seed in the loosened soil, the pointed germ issuing from its surface, and the bud on the branching tree, appear peculiarly fitted by their acute rigid extremities to receive the exciting influences of an electrified air, which, by its gradual swell and agitated movements, in the early spring season, presses against them continually in successive eddies, conveying to each of them some of its electric matter. It appears to me that this simple principle, in conjunction with an occasional supply of the same fluid from vapours, together with their moisture, will in a considerable degree account for the first excitement and germination of plants; especially when it is considered that every preparation seems made for adapting the atmosphere thus to act on the embryo plants at this season. Freed from vapours by the condensing effects of cold in the preceding winter, it is now in a peculiar state of dryness; and now the glancing rays of the sun accumulate in it and render it strongly electrical. This accumulation must be much favoured by the absence of foliage in the larger and more vigorous parts of the vegetable kingdom at this crisis; for the transpiration of moisture from the expanded leaf neutralizes myriads of solar rays, and charges the atmosphere with vapours; but, in consequence of the whole class of indigenous plants presenting nothing but minute buds from their ramifying branches, no rays are neutralized, and no vapours are formed, from them. Consequently all those rays, which would otherwise have been thus neutralized, are left floating electrically in the pure air, or entering its pores in small portions, serve a little to raise its temperature and swell its volume, and so to aid the general effect.

That the rays of the sun entering the atmosphere at this season, at acute angles with the earth, must tend considerably to cause them to lodge and accumulate in the strata above in the form of electricity seems evident in itself, and to receive confirmation from the very large accumulations of it in the polar regions at the periods when the sun is at his greatest distances from the zenith of the respective poles; for to what more probable cause can these agreeable and welcome lights be ascribed, but to a very large portion of the almost parallel rays resting in the higher and more attenuated regions of the atmosphere in those quarters; and thus in some degree administering the several benefits of light, warmth, and electricity, to those otherwise deserted parts of the globe? But we are not left to conjecture as to the fact, Mr. Sturgeon having generously informed me, as the result of his numerous atmospherical experiments, that the strongest electricity exists in the air at this season, and

Uttarpara Jaikrishna Public Library
Accn. No. 31490 Date 13.08.97

under the influence of those cold drying winds by which it is distinguished: and this is in entire conformity with the observations I have occasionally made. The following experiment will tend to show its influence on vegetation.

On the 28th of April I sowed mustard seed in similar soils, contained in two jars, one electrified positively, the other negatively. The covers being removed they were both left open to the action of the atmosphere. In four days the plants appeared in both jars, but those in the negative jar were the most advanced; while no plants appeared till about two days later from a similar sowing at the same time, unelectrified. On the 12th of May the plants in the negative jar had grown to 2¾ inches, those in the positive jar to 2¼ inches, in height; those unelectrified rather remaining in the ordinary state to 1⅛ inch. The electrified plants were vigorous and flourishing in proportion to their height. This result in favour of the plants negatively electrified must have arisen chiefly from the *relative* superiority of electric matter in the atmosphere; in the case of those positively electrified the *absolute* quantity was increased, but the relative difference between those and the atmosphere somewhat reversed. Hence it appears that, while much depends on electric influence, its *positive* state in the atmosphere as contrasted with the soil operates most effectually on vegetation. The difference of more than half in height in favour of the former, above those in the natural state, strongly encourages the conclusion that the constant accessions of electric fluid in the atmosphere at the spring of vegetation constitutes its influence, and that on the degrees of that influence depends in a great measure the rapidity and vigour of its rise and progress. A medical electrician, residing in this town, acquainted me with the following particulars:—A narcissus plant when in a very weak and languishing state being placed in the room in which his powerful machine was kept in frequent action, soon began to show signs of extraordinary vigour; it grew to the height of 36 inches, and was stout and luxuriant in proportion. Some branches of the moss rose, and various other flowers in the room, retained their colours while the seeds were forming, during about five weeks, and at length dropped off without losing their freshness. These and some other particulars which he related to me, on the correctness of which I have reason to rely, show the vast advantages that might be expected to result from a continued powerful electricity in the atmosphere, accompanied with a suitable temperature and dryness. If a plant can be made to expand to thrice its ordinary dimensions, by an artificial increase of positive electricity, and that in a confined situation, where the direct rays of the sun could hardly exert their ordinary influence, how important must be the operation of this principle, in proportion as it should seem, to the degrees in which it obtains! But it must be by the proper union of its several properties of light, heat, and electricity combined, that its greatest and most salutary effects on plants are produced; and accordingly by far the richest and most copious productions of the vegetable kingdom are found in those climes in which the solar beams are most abundantly distributed. A very satisfactory proof of the electric operation of those beams appears in the following extract from the Atlas, to which I was referred by my valued friend and coadjutor, Mr. Weekes. "For the double purpose of ascertaining the power of spines in modifying the electric relation of the atmosphere and the earth, and in effecting the progress of vegetation by their electric influence, M. Astier insulated a sextuple spine of the *gleditzia triacanthos* at the top of his house, and brought a wire to it from an insulated pot, in which were planted five grains of maize: a similar sowing was made in an uninsulated pot, for the purpose of comparison. The experiment continued from the 6th to the 20th of June, including two stormy days. The electrometer gave considerable signs of electricity in the flower pot, and, by using the condensor, sparks were produced. The electrified grains were found to pass more rapidly through the first stages of vegetation. When Bengal-rose trees were submitted to the same experiment, the flowers of the electrified plant appeared more rapidly and more abundantly than in the other case."

I trust it will have sufficiently appeared from the above statements that the commencing stages of vegetation are in a great degree caused or promoted by the influence of the electric fluid which is lodged in the dry air of our atmosphere at the season of germination. Additional evidence is, no doubt, highly desirable, and experiments of a more decisive and interesting character could easily be devised were the attention of those who have good opportunities of connecting the cultivation of plants with electrical inquiries more particularly directed to the subject. With respect to my leading position of the superior conducting efficacy of vegetable points, and their extensive influence on atmospheric electricity, the most accurate scientific proof will be seen in the annexed very obliging and admirable letter of Mr. Weekes. And I have only to add, on this head, that our correspondence took its rise from some hints he had received concerning my humble, but I trust not unimportant, discovery, and the inferences I was beginning to deduce from it. The ardour with which he engaged in the inquiry will best appear from his own statements, from which I shall no longer suspend the attention of your readers; only congratulating them on his recent discovery of the decomposition of water by vegetation, as related by him in the last Number but one of these valuable Annals of Electricity; a discovery which must form a most interesting addition to general science, as well as much assist our researches into the principles of vegetation.

(To be continued.)

Sandwich, May 31, 1828.

Dear Sir,

Various circumstances have united to occasion a tedious delay in our correspondence since I last addressed you, and promised an investigation of your ingenious theory of electricity. However, I find I have by no means had too much time for a fair and impartial examination of the subject, the interest of which, to me, has been such as to excite experiment far beyond my intentions at the outset. The final result in my mind is an entire conviction that your opinions are well founded, and have stood the test of the severest trials to which they could be subjected. The vast superiority of vegetable over metallic points in the drawing off and accumulating electric matter, is, I conceive, a subject of great interest and importance. A coated jar having 46 inches of metallic surface was repeatedly discharged by the activity of a vegetable point in 4 min. 6 sec.; while the same jar charged to the same degree, required 11 min. 18 sec. to free it from its electric contents by means of a metallic point: the points in both cases being equidistant. I find also that Benett's gold leaf electroscope (a delicate instrument) is powerfully affected by a charged jar, at the distance of nearly 7 feet, when the brass cap of the instrument is furnished with a branch of the shrub called *butcher's broom*, and which I have found of great use in my experiments. The same delicate instrument when mounted with pointed metallic wires is not perceptibly affected until the charged jar approaches to within 2 feet of the cap. I must not think of troubling you with the details of all that I have been about as regards this investigation; but one circumstance has proved too pleasing to be wholly omitted.

Let *a, b, c,* fig. 1, Plate VI, represent a large *street lamp* in an inverted position, mounted with a brass cap *d,* through which passes a stout wire *e f*, having a brass knob at *f*, and a pair of small pith balls attached to the wire just above the knob. *g* is a portable stand with two metallic discs, one on each side of the wire and rising to a level with the pith balls. *h* is a small branch of the butcher's broom fixed by a twine to the upper extremity of the wire *e f*. This apparatus I have for many weeks past had in almost daily use, nor can I express the pleasure it has afforded to myself and friends by its frequent indications of atmospheric electricity; for, armed with your *vegetable detectors*, it has shown symptoms of electricity by the passing of clouds at a great altitude, and under various other circumstances in which electrometers with metallic points placed by its side gave no indications whatever. This appears to me so decided a proof of the superiority of vegetable conductors, that it admits of no contradiction.

The correctness of your opinions respecting the influence of electricity upon the growth of plants, appears to me to be sufficiently proved by the following experiments. Two small flower pots filled with rich mould were taken for the purpose in doors. A few grains of mustard seed were sowed in each; both were kept gently watered, but one pot was *insulated* and frequently electrified under circumstances which kept it, as it were, in an electrical atmosphere. The other pot had no such attention shown to it, and the result proved what you probably would anticipate. The vegetation of the electrified seeds appeared several days before the others, and continued afterwards to grow with a much greater degree of vigour. As a lover of science you can easily imagine the pleasure these pursuits have yielded, and to this has succeeded an anxious desire that you should speedily assert your claims, or I apprehend you will lose your just title to originality. I send you an extract which lately fell in my way; the perusal of which I hope will put you on the alert. The extract is from Taylor's "System of Philosophy," in which the author has written a great deal of downright nonsense; but still it appears he had somewhere obtained a *glimpse* of the same opinion by which you are animated on this subject.

"The leaves of plants act as so many spicula to attract the electricity of the air and *solar rays*; hence very high trees are so many natural conductors, attracting a vast quantity of electric fluid, and, consequently, put forth a luxuriance of foliage proportionate thereto." *Review of Books. Quarterly Journal of Science, October*, 1826.

You see by these approaches towards your theory and facts, you ought to lose no time in securing the just praise of originality, to which you are doubtless entitled. My health being now perfectly restored I am very actively engaged, or I could willingly have written at greater length, but must conclude with saying,

I am, dear sir,
Very faithfully yours,
W. H. WEEKES.

To Thomas Pine, Esq.

Woolwich, Dec. 2d, 1832.

My dear Sir,

It is with a very great deal of pleasure that I have read your letter stating your intention of publishing your views on some of the most interesting phenomena of nature, and permit me to acknowledge that I feel much honour by your selecting my humble authority in giving assistance to your efforts; and I can assure you that nothing shall be wanting on my part, as far as experience has enabled me to draw conclusions, to forward your very laudable object.

In the first place, then, I perfectly agree with you, as to the solution of the results of Sir H. Davy's experiments on corn; for the positive pole of a voltaic battery would supply the animating electric fluid to the germinating seed in precisely the same manner that nature supplies it from the atmosphere to the ground. As Sir Humphrey does not state from what "experiments made on the atmosphere" he draws his conclusions "that clouds are usually negative," I am unable to form any opinion respecting them. But I must beg permission to state, that such a conclusion is quite at variance with the results of my experiments. It is true I have obtained negative charges at the kite string, but the instances are very few indeed. Those which did occur were only whilst heavy clouds passed over the kite; the indications, both before and after the clouds' transit, being invariably positive. And even in those temporary exhibitions of negative electricity, I am very far from concluding that the clouds themselves were negatively electric. The indications were those of the kite, which was floating much lower than the clouds: and the air *vicinal* to the kite was consequently the *only part* of the atmosphere explored during each experiment, which air probably became negative or deprived of most of its natural electricity by the repulsive force of the accumulated electric matter in the positively charged clouds. This assertion can hardly be construed into "begging the question" or "strain-ing a point;" because such phenomena are easily produced by experiment and must necessarily frequently occur in nature.

To ascertain directly, the electric state of clouds, the kite ought to be immersed immediately in those we wish to explore. No such experiments have yet been made. I am of opinion that my kites have been nearer to the clouds than any hitherto employed for experiments of this kind. Many experimenters have contented themselves with 400 or 500 yards of string, and others have drawn their conclusions without employing any kite in their experiments, from experiments made with an apparatus not much longer nor very unlike an Angler's Rod!!! The results which I have obtained from about 300 experiments, at nearly all seasons of the year, and at all times of the day, and many at night, induce me to believe that the *general* electric state of the atmosphere, with its contained clouds, vapours, &c., is, with reference to the earth, *positive*. For, notwithstanding those very rare aberrations from the general results which I have noticed at the kite string during the transit of a fleeting cloud, they appear to me (in the way which I think they operate) to be *favourable* than otherwise to the conclusions at which I have arrived. Moreover, I find from experience, that bodies generally, when in, what is usually called, their natural electric state, have *not* an equable distribution of the electric matter on every part of their surfaces; but, on the contrary, that each individual body or substance, when in this, its *natural state,* exhibits different electric tensions on various parts of its surface. So it is in the atmosphere, that at different times, and at different altitudes at the same time, different electric tensions are exhibited.

All electrical phenomena are *relative*, and consequently all our calculations respecting them, have no other basis but the ever varying degrees of those relations. But, notwithstanding the variations in the extent or degrees of those relations, the *relations themselves* appear to be constant and uniform. Therefore I conclude generally (and my conclusions are from direct experiments) that the atmosphere, taken as a whole, is constantly in an electro-positive state with reference to the earth; and that in the atmosphere itself, the upper regions are constantly electro-positive with reference to all those situated nearer to the surface of the earth. The strata of air near to the earth's surface are therefore in an *intermediate* state of electricity with reference to the upper strata and the body of the earth, the earth itself being negative to the whole.

These results, my dear sir, are, in my opinion, of a very decisive character; and if you deem them of sufficient importance to be taken into consideration whilst framing your theory of electro vegetation (pardon me for inverting the term "vegetable electricity") they are quite at your service; and if you are desirous of stating the authority, you are perfectly at liberty to do so.

Whilst writing this letter (between six and seven in the evening) a tremendous thunderstorm passed over this place. Half an hour before, the sky was quite clear, the moon and stars shone with great lustre.

I am, Dear Sir,
With very great respect,
Yours very truly,
W. STURGEON.

To Thomas Pine, Esq.

XXXIV. *On the Colours of Mixed Plates.* By Sir David Brewster, *K.G.H. F.R.S.*[*]

Received October 25,—Read December 14, 1837.

The colours of mixed plates were discovered by Dr. Thomas Young,[†] and described in the Philosophical Transactions for 1802. He produced them by interposing small portions of water, or butter, or tallow between two plates of glass, or two object glasses pressed together so as to give the ordinary colours of thin plates. In this way portions or cavities of air were surrounded with water, butter, or tallow; and on looking through this combination of media he saw fringes or rings of colour six times larger than those of thin plates that would have been produced had air alone been interposed between the glasses. These fringes or rings of colour were seen by the direct light of a candle, and began from a white centre like those produced by transmission; but on the dark space next the edge of the plate, Dr. Young observed another set of fringes or rings, complementary to the first, and beginning from a black centre like those produced by reflection. This last set of colours was always brighter than the first.

The following is Dr. Young's explanation of these two series of colours.

"In order to understand," says he, "this circumstance, we must consider that where a dark object is placed behind the glasses, the whole of the light which comes to the eye is either refracted through the edges of the drops, or reflected from the internal surface; while the light which passes through those parts which are on the side opposite to the dark object consists of rays refracted as before through the edges, or simply passing through the fluid. The respective combinations of these portions of light exhibit a series of colours of different orders, since the internal reflection modifies the interference of the rays on the dark side of the object, in the same manner as in the common colours of thin plates seen by reflection. When no dark object is near, both these series of colours are produced at once; and since they are always of an opposite nature at any given thickness of a plate, they neutralize each other and constitute white light."*

---

[*] From the Transactions of the Royal Society for 1838.

[†] Since this paper was written I find that this class of colours was discovered by M. Mazeas, and that his experiments were repeated and varied by M. Dutour.

In so far as I know, these observations have not been repeated by any other philosopher; and subsequent authors have only copied Dr. Young's description of the phenomena and acquiesced in his explanation of them. In taking up this subject I never doubted the accuracy or the generality of the results obtained by so distinguished a philosopher. I was induced to study the phenomena of mixed plates as auxiliary to a more general inquiry; and having observed new phenomena of colour in mineral bodies, which have the same origin as those of mixed plates, and which lead to conclusions different from those of Dr. Young, I am anxious that they should be described in the same work which contains his original observations.

Having experienced considerable difficulty in obtaining satisfactory specimens of the colours of mixed plates by using the substances employed by Dr. Young, I sought for a method of producing them which should be at once easy and infallible in its effects. With this view I tried transparent soap, and whipped cream, which gave tolerably good results: but I obtained the best effect by using the white of an egg beat up into froth. To obtain a proper film of this substance I place a small quantity between the two glasses, and having pressed it out into a film I separate the glasses, and by holding them near the fire I drive off a little of the superfluous moisture. The two glasses are again placed in contact, and when pressed together so as to produce the coloured fringes or rings, they are then kept in their place either by screws or by wax, and may be preserved for any length of time.

---

* Philosophical Transactions, 1802. Dr. Young republished the same explanation of mixed plates in 1807 in his Elements of Natural Philosophy. See vol. i. p. 470, 787; vol ii. 635, 680.

If we now examine with a magnifier of small power the thin film of albumen, we shall find that it contains thousands of cavities exactly resembling the strata of cavities which I have described as occurring in topaz, quartz, sulphate of lime and other minerals:* and if we look through the film at the margin of the flame of a candle, we shall perceive the two sets of colours described by Dr. Young, the one upon the luminous edge of the flame, and the other on the dark space contiguous to it. The first we shall call the *direct*, and the second, which are always the brightest, the *complementary fringes*.

If we apply a higher magnifying power to the albuminous films, and bring the edge of one of the cavities to the margin of the flame, we shall perceive that both the *direct* and the *complementary* colours are formed at the very edge, the complementary ones appearing just when the direct ones have disappeared, by the withdrawal of the edge from the flame.

As the colours therefore are produced solely by the edges of the cavities, their intensity must, *cæteris paribus*, depend on the smallness of the cavities, or the number of edges which occur in a given space. When we succeed in forming an uniform film in which the cavities are like a number of minute points, the phenomena are peculiarly splendid and we are enabled to study them with greater facility. When the edges of these cavities are seen by an achromatic microscope, and in direct light, neither the direct nor the complementary colours are visible; but if we gradually withdraw the lens from the cavities a series of beautiful phenomena appear. When the vision first becomes indistinct both the direct and the complementary colours appear at the same time, specks of the *complementary red* alternating with brighter specks of the *direct green* light. By increasing the distance of the lens from the cavities, the complementary specks become less and less visible, and we see only the direct green light.

In order to study these phenomena by observing the action of a *single* edge upon light, and to ascertain the effect of an edge when there were no prismatic edges to refract, and no internal surface to reflect light, I conceived the idea of immersing thin plates of a solid substance in a fluid of such a refractive power, that the thickness of the plates should be virtually reduced to the same degree of thinness as the film of albumen between the plates of glass. The new substance described by Mr. Horner,† and which I shall call *nacrite*, furnished me with the means of performing this experiment. I accordingly inclosed the thinnest films of it between two plates of glass containing balsam of capivi; and I had the satisfaction of observing that the bounding edge of the plate and the fluid produced the identical direct and complementary colours above described.

---

* Edin. Trans. vol. x. Part I. 407.

† Philosophical Transactions, 1836, p. 49.

The bounding edge which I selected for observation gave a *bright green* for the *direct*, and a *bright red* for the *complementary* tint. This edge appeared as a narrow distinct black line, exceedingly well defined, and of a uniform breadth like the finest micrometer wire. It consequently obstructed the incident light and produced the phenomena of diffracted fringes. Those fringes, however, were modified by the peculiar circumstances under which they were produced, and exhibited in their tints both the direct and complementary colours under consideration.

When the diffracted fringes are viewed in candle-light by a lens placed at a greater distance from the diffracting edge than its principal focus, the middle of the system of fringes corresponding to the diffracted shadow of a fibre is occupied with the *direct tint*, which we shall suppose to be *green*; and on each side of this *green* shadow, as we may call it, we observe very faintly the *complementary red* tinging what are called the two first exterior fringes. This tinge of red is strongest in the first fringe within the solid edge, or within the green shadow, while it is *yellowish* in the first fringe without the green shadow. These effects are inverted if we place the lens nearer to the edge than its principal focus.

The phenomena now described appear more distinct if we take an extremely narrow piece of nacrite, having its two edges nearly in contact, and transmitting only a narrow line of light. In this case the two red fringes within the solid edge unite their tints, and become a bright red; and in like manner if we place the lens nearer the solid edges than its principal focus, the two yellow fringes will unite their tints, and become a brighter yellow band. In this last case, when the two bounding edges are still nearer each other, the united fringes, in place of being yellow, will be *green*, or the same as the direct colour.

If we bring the edges of two pieces of nacrite of equal thickness very near each other, having, as formerly, *green* for the *direct*, and *red* for the *complementary* colour, the space between the edges, or between the green bands, will be faint *red* when the lens is nearer the edges than its principal focus, and *yellow* when it is further from them; but if the edges are brought still nearer, the faint red will become brighter, and the united green bands will take the place of the yellow one.

Let us now return to our plate of *nacrite* with a single edge, having *green* and *red* for the two tints; and let us always suppose that the lens is adjusted to observe the diffracted fringes, that is, that the lens is placed at a greater distance from the diffracting edge than its principal focus. We shall also suppose that the light of the sun passing through a narrow aperture parallel to the diffracting edge is substituted for the light of a candle. Under these circumstances the central part of the system of fringes seen by light incident perpendicularly, consists of *blue**, *green*, and *yellow* light, constituting, as it were, the shadow of the edge, the blue light being on the same side as the plate of nacrite, and the yellow rays encroaching upon the exterior faint red band already described, the other red band next the blue being more distinctly seen. If we now incline the incident ray to the plate of nacrite more than 90°, the faint red band next the yellow gradually becomes brighter, while the other bands become fainter; and at the boundary of light and darkness all the other bands disappear except this *red* one, which is the *complementary* colour to the *green*, (produced by the union of the *blue*, *green*, and *yellow* bands), and the colour which is seen upon the dark space next the edge of the flame, as described by Dr. Young. If we, on the other hand, incline the incident ray in an opposite direction, so that it forms with the plane of the plate a less angle than 90°, the *red* band next the blue will now become brighter; and at the boundary of light and darkness, when all the other bands have disappeared, the *red* band will afford the complementary colour to the *green*.

As the edge of the plate of nacrite is rough and unpolished, and accurately perpendicular to the parallel faces, there are no reflected nor refracted pencils, whose combinations with one another, or with the direct rays, can be employed to account for the complementary colours. The phenomena of mixed plates, indeed, are cases of diffraction when the light is obstructed by the edge of very thin transparent plates placed in a medium of different refractive power. If the plate were opake the fringes would be exactly those which have been so often described, and explained by the principle of interference. But owing to the *transparency* of the plate, fringes are produced within its shadow; and owing to the *thinness* of the plate, the light transmitted through it and retarded, interferes with the partial waves which pass through the plate and with those which pass beyond the diffracting edge with undiminished velocity, and modifies the usual system of fringes in the manner which we have described.

---

* Owing to the small quantity of blue rays in candle-light the blue almost disappears in it.

As the plate of nacrite diminishes in thickness, or as the fluid in which it is immersed approaches to it in refractive density, the central coloured bands, whose union constitutes the *direct* tint, will diminish in number, and descending gradually in the scale will finally disappear when the retardation produced by the plate does not perceptibly alter the phase of the ray. When the plate, on the other hand, increases in thickness, or the fluid diminishes in refractive power, the central bands will become closer and more numerous, and will finally resemble the fringes within the shadow of the ordinary system.

When the plate of nacrite is thicker at one place than another by the partial removal of a parallel film, the edge where the increase of thickness takes place produces exactly the same phenomena as the edge of the film that is removed, or of the film that is elevated above the general surface, and hence we are led to look for the phenomena of mixed plates in minerals, such as *sulphate of lime* and *mica*, where a plate of two different thicknesses can be easily obtained. I have accordingly discovered the phenomena of mixed plates distinctly exhibited in sulphate of lime and mica.

A more splendid exhibition of these colours is seen when a stratum of cavities of extreme thinness occurs in sulphate of lime. I have observed such strata repeatedly in the gypsum from Mont-martre; but they are most beautiful when the stratum has a circular form. In this case the cavities are exceedingly thin at the circumference of the circle, and gradually increase in depth towards the centre, so that we have a series of edges increasing in thickness towards a centre; the very reverse of a mixed plate, such as a film of albumen pressed between two convex surfaces. The system of rings is therefore also reversed, the highest order of colours being in the centre, while the lowest are at the circumference of the circular stratum. In many strata of cavities, such as the one which I have engraven in my paper on the new fluids in minerals,* the cavities are too deep to give the colours of mixed plates.

Another example of the colours of mixed plates in natural bodies occurs in specimens of mica, through which titanium is disseminated in beautiful flat dendritic crystals of various degrees of opacity and transparency. In these specimens the titanium is often disseminated in grains, forming an irregular surface. The edges of these grains, by retarding the light which they transmit, produce the direct and complementary colours of mixed plates in the most perfect manner, the tints passing through two orders of colours, as the grains of titanium increase in size towards the interior of the irregular patch. I have observed another example of these colours in the deep cavities of topaz, from which the fluids have either escaped, leaving one or both of the surfaces covered with minute particles of transparent matter, or in which the fluids have suffered induration.

---

* Edinburgh Transactions, vol. x. Plate II. fig. 33.

Allerly by Melrose,
October 18*th*, 1837.

XXXV. *Sparks obtained from the secondary coil after the current being made to pass through water. In a letter to the Editor. By* W. H. HALSE, ESQ.

Sir,

There is a fact connected with voltaic electricity which I believe has never as yet been published, and as it proves the intensity of the secondary current in a remarkable manner perhaps you may think this letter worthy a place in your next number of "the Annals."

It is well known that if we separate the two extremities of the secondary coil at the same moment that one of the primary wires of a shock apparatus is disconnected with the battery, that a spark will be visible on the *secondary* wire as well as on the *primary* wire, but in this case *metallic contact of the secondary wires has hitherto been considered necessary to produce the effect.* During a course of experiments in which I have lately been engaged, I imagined that the secondary current was sufficiently intense to give a spark after passing through water containing a very minute portion of common salt. I accordingly put my revolving apparatus to work using only one pair of cylinders both for causing the revolutions, shocks, and sparks. I then placed one end of the *secondary* wire in a glass of water, and about one inch from it I placed a file in a perpendicular position; the other end of the *secondary* coil I drew up and down this file. The revolving apparatus was going at the rate of 900 rounds per minute and gave two shocks each revolution. *Immediately I touched the file with the secondary wire I observed a spark, and by continuing the motion of the wire on the file I obtained them by scores.* Those who are not in possession of a revolving apparatus or a contact breaker may perform the experiment in the following manner: affix to one of the plates of the battery a file, and to the other plate affix one end of the *primary* wire coil; then insert one end of the *secondary* coil in a glass of water containing a very small quantity of common salt or sulphuric acid, and place a file in the same glass about one inch distant from the immersed wire. Now let an assistant draw the other end of the *primary* wire across the file attached to the battery, whilst the operator draws the other end of the *secondary* wire across the dry part of the file in the glass. *Sparks will soon be perceived on this latter file and a portion of the water will be decomposed.* By keeping the file and the wire one inch or more distant from each other in the water, it is evident that the current has to pass through that space of water previous to obtaining the spark, thereby proving THAT METALLIC CONTACT OF THE SECONDARY WIRES NOT TO BE NECESSARY. I have no doubt that a spark could be taken after the current passing through one's body; but this experiment I do not like to try, neither can I get any of my friends to try it, my shock apparatus being very powerful. The battery I used was composed of cylinders immersed in a two quart pot, sulphate of copper being in contact with the copper, and a solution of common salt in contact with the zinc.

The size of the spark is much increased by increasing the number of cylinders which perhaps is unnecessary for me to mention. With ten pairs I have obtained very brilliant sparks. When the spark is to be produced from one pair the experiment should be performed in the dark. If these few lines should meet your approbation sufficient for their insertion you shall again hear from me, having several things to communicate which I believe would prove interesting to your readers; particularly a method how to increase the intensity of the coils, and also a new theory to explain why a small pair of plates, introduced into a circle, brings the action of the whole battery to that standard, De la Rives explanation of it being in my opinion unsatisfactory. Can you recommend me a recently published work which treats principally of the physiological effects of electricity.

I am, Sir,
Yours respectfully,
WILLIAM H. HALSE.

Brent, near Ashburton,
Nov. 27th, 1839.

XXXVI. *An Account of some experiments made for the purpose of ascertaining how far Voltaic Electricity may be usefully applied to the purpose of working in metal.* *By* Mr. THOMAS SPENCER.

Prefatory.

Having made known, about three months ago, at a meeting of the Liverpool Polytechnic Society, that I intended to have brought the subject of the following paper before the British Association at its Birmingham meeting, I deem it a duty I owe myself,—and perhaps the public,—to state the reasons why I have not done so.

About a month previous to the meeting, I wrote to Professor Phillips, the general secretary, informing him of this, and two other papers I was desirous of laying before the Association at its next meeting, and requesting to know what forms were necessary to enable me to do so.

I received a very obliging answer in return, intimating that two of my papers would be read at the Chemical Section; but the one which is the subject of the following paper would be read at the Mechanical one, as it was deemed a portion of the process related to that science; also, that in the event of my non-attendance they would be read in my absence, by forwarding them to the secretaries of the different sections, as he would make notes to that effect.

Nothing could be more satisfactory. I, however, went to Birmingham on the Monday of the week of meeting, and immediately paid my subscription for the ensuing year, which entitled me to all the privileges of a member,—including that of reading papers, if so disposed.

Having satisfactorily completed my business at the Chemical Section, I at once proceeded to the Shakspeare Rooms, where the Mechanical one held its meetings, and inquired for the secretary. I was shown to a Mr. Carpmall. After intimating my business I inquired when I should be called on, that I might be in readiness. I was told that a note had already been made of it, and to hold myself in readiness on Thursday morning, as my paper would be called on first. With this I was perfectly satisfied; but on Wednesday, when the papers for the following day, as is usual, were announced, I could not find my name or paper in the list. I at once addressed a note to the secretary, thinking he had forgotten our previous arrangement and reminding him of it. I attended next morning at the appointed time, prepared, if called on; but on entering the committee-room, I was informed by

Mr. Carpmall that my paper could not be brought forward, giving as a reason, that "I was quite unknown." On asking what this meant, I was told that I was a man of no scientific reputation, and more especially unknown to himself and the acting president of the section, Dr. Lardner; also, that there were so many important papers to be brought forward by men of acknowledged reputation, that there was no chance for me.*

When the section had closed its labours for the day, seeing Dr. Lardner in the room, I mentioned to him, as president of the section, the arrangements that had been entered into with me by the secretary respecting my paper. Before he heard more than a few words, he told me he had nothing whatever to do with it, and haughtily turned away, adding something about valuable time.

Under these circumstances I could not bring forward the paper: but, had it not been for the unavoidable absence of Professor Phillips, I am quite sure the engagements entered into with me would have been kept.

In conclusion I may add, I can find no fault with arrangements that might be made by any Society to select such subjects as a Committee might deem proper to be laid before its members and the public, as otherwise much valuable time is often likely to be lost,—it requiring no small portion of scientific learning to be acquainted with all that *has* been done on most subjects, and without this knowledge we are too apt to stumble on what may have been years before the public. But in the instance I have related, neither the Committee nor Secretary were aware of the views I had taken of the subject I was desirous of illustrating, as they never once asked to look over my paper. Had they done so, it would have been at their service.

It is two years since I began to experimentalize on this subject. I *then* made mention of it to a few friends, (some of whom are connected with the public press in Liverpool,) but strictly enjoined them not to make it public until the experiments were matured. At the same time I showed some results obtained by this process. About four months ago a paragraph appeared in the *Athenæum*, stating that Professor Jacobi, of St. Petersburgh, had received a grant of money from the Emperor, to enable him to make experiments on engraving by galvanism, as he had been enabled to preserve fine lines in relief by this principle.* I accordingly concluded that he was engaged in experiments analagous to my own: but having gone much farther than merely producing lines in relief, I at once made it public, and showed specimens of the results I had obtained in different experiments.—This was done at a meeting of the Liverpool Polytechnic Society; some of the members of which *then* spoke to their knowledge of my having been engaged on this subject for a considerable period.

---

* I may state that on the day in question, a gentleman was allowed to occupy the section by a description of the method he had adopted to cure his chimney from smoking. I mention this,—not because it was unimportant,—but because it had not even been announced. The alleged reason was that the section were on the subject of smoky chimneys. However, I concluded he was a man of known scientific reputation,—although I have since forgotten his name.

I am not aware that Professor Jacobi has made his process in any way public: but if I am to judge from some specimens I saw a few days ago in Birmingham, in the model room, produced by it, I should be inclined to think that he has made small progress in this subject,—one of the specimens being a plate of copper precipitated on another, which he has been unable to get off; the printed description stating that "through some particular circumstance they adhere together." It will be seen, in the course of the following experiments, that I early arrived at this point which has also been easily and completely surmounted.†

I entertain no very sanguine notions as to the future *general* application of this method of operating upon the metals more especially copper. This must be entirely left to the practical engraver and printer.

---

* See a notice of Jacobi's experiments at p. 507, vol. 3, of these Annals. Edit.

† I cannot help finding some fault with the mode adopted by the professor (or, it may be, injudicious friends), of announcing his discoveries. About twelve months ago, I imagined that I had discovered a method of obviating, by simple means, the great difficulty in the construction of electro-magnetic Engines; but, on seeing several paragraphs from time to time in the newspapers, professing to be copied from letters received from St. Petersburgh, stating that Dr. Jacobi *had succeeded* in constructing engines of considerable power, on this principle, I at once gave up my researches on the subject, thinking the thing already done. It is only a few weeks since a statement appeared (in a Liverpool paper,—on the authority of a letter received by Professor Wheatstone from Dr. Jacobi,) that he *had* an electro-magnetic machine, of forty-horse power, *at work* on the Neva; but, since, it appears another letter states he had *hoped* only to have such an engine by this period, and still hopes by next year to accomplish his object—but has not as yet succeeded with one of three-horse power.

The question with them will be,—Is it cheaper and better than the methods in common use? It may now be answered—Give it a fair trial: the way is pointed out—practice will no doubt enable you to improve upon the methods which suggested themselves during the experimental investigation detailed in the following pages, and most probably may realise an extended field of practical utility for the peculiar mode of operation which has been the result.

I feel assured, however, that, in the arcana of many trades and branches of art, this process will be found an important addition—supplying as it does a means of producing a cast, or a die, in hard metal, *without the agency of heat or pressure*, and in extreme perfection and well-defined sharpness. Nor, (I need hardly observe) is its application confined to copper only.

In addition to the applicability of this process, in procuring exact fac similes of *coins*, or *medals*, with all the lineal sharpness of the original, perfect copies may be obtained of bronzed *figures*; nor do they require chasing when taken out—nor do I apprehend inconvenient limitation as regards their size.

Assuming it to be advantageous to publishers of music to have their plates *in relief*, by this process they will be enabled, in the original engraving, to have them so.

I have seen nothing in wood engraving that might not be produced in copper, in relief, by this means; the chemical plates might, possibly, require retouching to a small extent, but, with careful manipulation, twenty or thirty such plates might be taken from one mould.

I may mention that the advantage of being able to produce a given effect from a plate in relief would be very considerable, as ten printed impressions may frequently be taken, in the time occupied in producing one by the ordinary method from a copper-plate. Plates *in relief* might also frequently be printed off in the body of the work—which, in point of economy, would be a very considerable advantage.

In the formation of that important implement in the manufacture of printing types—the matrix or mould,—advantages in the adoption of this operation appear to present themselves. And, I am assured by the printers of this pamphlet, that it gives fair promise to supply several important desiderata in the art of printing, and in its attendant operations,—more particularly in the stereotype process.

In general,—I feel convinced that it exhibits many promising indications of utility, should no obstacles in a pecuniary point of view present themselves, on occasion of attempts to extend the application of the discovery.

In the following paper I have detailed a few of the most illustrative experiments made during the investigation, trusting that they might be found interesting, not only to the general reader, as illustrating the progress of discovery, but to the future experimentalist, in pointing out to him the methods that have best succeeded, as well as those he ought to avoid. In all cases I deem details of chemical experiments essentially necessary; as one *apparent* trifle omitted, is more than likely to retard the labours of the future practitioner.

Having made many experiments on a larger scale than those detailed, since writing the following paper, I shall, at its conclusion, detail the methods to be adopted under different circumstances.

First:—*To engrave in relief on a plate of copper.*

Second:—*To deposit a voltaic copper-plate, having the lines in relief.*

Third:—*To obtain a fac-simile of a medal (reverse or obverse), or of a bronze cast.*

Fourth:—*To obtain a voltaic impression from plaster or clay.*

Lastly:—A method of *multiplying the number of already engraved copper-plates.* This last promises to be of vast import,—more especially in the Potteries, as there they require, in many instances, eight or ten copper-plates of a similar pattern. By the method I shall point out, I can see no reason why they should not be able to multiply them *ad infinitum.*

I shall also give some rules for the management of the apparatus, which my experience of the process has suggested.

When I have done this, I shall then have laid before the public the result of many an anxious—and, I may add, pleasant—hour: each experiment requiring a considerable lapse of time for its development; but when attended with success—no words of mine can convey the pleasurable feelings coupled with such a result.

I have been led on, by the fond hope that the present simple discovery may be the foundation of a vast structure of Synthetic Chemistry, which is perhaps destined, (at no distant date), to imitate, for the uses of humanity, all the most wonderful, but apparently complicated, elaborations of Nature.

PAPER.

Notice given May the 8th—read September the 12th, 1839.
HENRY BOOTH, ESQ. *President*, in the Chair.

In the paper I have now the honour to lay before the Society, I do not profess to have brought forward a perfect invention. My only object is to point out a means by which, I hope, practical men may ultimately be enabled to apply a great and universal principle of nature to the useful and ornamental purposes of life. In this I may be considered sanguine,—an error, I am aware, too often fallen into by those, who, like myself, imagine they have discovered an useful application of an important principle; but however this may fall out, I now proceed to lay an account of its results, successful and unsuccessful, before the members and the public,—previously stating, however, that all my first experiments were made on a small scale; a method of procedure attended with many advantages to the experimentalist himself, but having its disadvantage when laid before the public. In this first respect, the chemical experimenter has a decided advantage over the mechanical one; the success of his experiment, when tried on a small scale, doubly guarantees its success, if conducted on a still larger—with mechanical results I believe in most instances it is the reverse. But, when the chemist produces his microscopic proofs, the public are generally slow to believe that such minute appearances should warrant him in coming to any general conclusion.

In September, 1837, I was induced to try some experiments in Electro-chemistry, with a single pair of plates, consisting of a small piece of zinc and an equal sized piece of copper, connected together with a wire of the latter metal. It was intended that the action should be slow; the fluids in which the metallic electrodes were immersed were in consequence separated by a thick disc of plaster of paris. In one of the cells was sulphate of copper in solution, in the other a weak solution of common salt. I need scarcely add that the copper electrode was placed in the cupreous solution. I mention this experiment, briefly,—not because it is *directly* connected with what I shall have to lay before the Society, but because, by a portion of its results, I was induced to come to the conclusions I have done in the following paper.* I was desirous that no action should take place on the wire by which the electrodes were held together? To obtain this object I varnished it with sealing-wax varnish:—but, in so doing, I dropped a portion of it on the copper that was attached. I thought nothing of this circumstance at the moment, but put the experiment in action.

---

* The experiment here alluded to was to determine a most important point—and as it has an intimate connexion with the future

The operation was conducted in a glass vessel; I had consequently an opportunity of occasionally examining its progress. When, after the lapse of a few days, metallic crystals had covered the copper electrode,—*with the exception of that portion* which had been spotted with the drops of varnish, I at once saw that I had it in my power to guide the metallic deposition in any shape or form I chose, by a corresponding application of varnish, or other non-metallic substance.

I had been long aware of what every one who uses a sustaining galvanic battery with sulphate of copper in solution must know,—that the copper plates acquire a coating of copper from the action of the battery; but I had never thought of applying it to a useful purpose before. My first essay was with a piece of thin copper plate having about four inches of superfices, with an equal sized piece of zinc, connected together with a piece of copper wire. I gave the copper a coating of soft cement, consisting of bees' wax, resin, and a red earth—Indian or Calcutta red. The cement was compounded after the manner recommended by Dr. Faraday in his work on chemical manipulation; but with a larger proportion of wax. The plate received its coating while hot. On cooling, I scratched the initials of my own name rudely on the plate. taking special care that the cement was quite removed from the scratches, that the copper might be thoroughly exposed. This was put in action, in a cylindrical glass vessel about half filled with a saturated solution of sulphate of copper. I then took a common gas glass, similar to that used to envelope an

---

application of the results detailed in this paper, I may be excused in briefly alluding to it here. In fact no experiment can be made with any certainty, without keeping its results in view.

In September, 1837, at the Liverpool meeting of the British Association, a clever young demonstrator (Dr. Bird, of London) asserted that in an experiment he had made, he had obtained crystals of pure copper *without* the intervention of a metallic nucleus to commence with. I doubted this at the time, as it was opposed to all former experience. However, I made several very careful experiments, following Dr. Bird's plan in all he stated; then varied them in order to give it every chance of success. The result was that *no metallic crystallization will take place*, unless a metallic or metalliferous nucleus be present.

Argand burner, and filled one end of it with plaster of paris, to the depth of three-quarters of an inch. In this I put some water, adding a few crystals of sulphate of soda to excite action, the plaster of paris serving as a partition to separate the fluids, but sufficiently porous to allow the electro-chemical fluid to permeate its substance.

I now bent the wires in such a form that the zinc end of the arrangement should be in the saline solution, while the copper end should be in the cupreous one. The gas glass, with the wire, was then placed in the vessel containing the sulphate of copper.

It was then suffered to remain, and in a few hours I perceived that action had commenced, and that the portion of the copper rendered bare by the scratches was coated with the pure bright deposited metal, whilst all the surrounding portions were not at all acted on. I now saw my former observations realised;—but whether the deposition so formed would retain its hold on the plate, and whether it would be of sufficient solidity or strength to bear working if applied to a useful purpose, became questions which I now endeavoured to solve by experiment.

It also became a question whether—should I be successful in these two points—I should be able to produce lines sufficiently in relief to print from. This latter appeared to depend entirely on the nature of the cement or etching-ground I might use.

This last I endeavoured to solve at once. And (I may state) this appeared to be the principal difficulty; as my own impression then was, that little less than one-eighth of an inch of relief would be requisite.

I then took a piece of copper, and gave it a coating of a modification of the cement I have already mentioned, to about one-eighth of an inch in thickness; and, with a steel point, endeavoured to draw lines in the form of net-work, that should entirely penetrate the cement, and leave the surface of the copper exposed. But in this I experienced much difficulty, from the thickness I deemed it necessary to use; more especially, when I came to draw the cross lines of the net-work. When the cement was soft, the lines were pushed as it were into each other; and when it was made of harder texture, the intervening squares of the net-work chipped off the surface of the metallic plate. However, those that remained perfect I put in action as before.

In the progress of this experiment, I discovered that the solidity of the metallic deposition depended entirely on the weakness or intensity of the electro-chemical action, which I found I had in my power to regulate at pleasure, by the thickness of the intervening wall of plaster of paris, and by the coarseness and fineness of the material. I made three similar experiments, altering the texture and thickness of the plaster each time, by which I ascertained that if the plaster partitions were *thin* and *coarse,* the metallic deposition proceeded with great *rapidity,* but the crystals were friable and easily separated; on the other hand, if I made the partition thicker, and of a little finer material, the action was much slower, and the metallic deposition was as solid and ductile as copper formed by the usual methods,—indeed, when the action was exceedingly slow, I have had a metallic deposition apparently much harder than common sheet copper, but more brittle.

There was one most important, (and, to me, discouraging) circumstance, attending these experiments, which was, that when I heated the plates, to get off the covering of cement, the meshes of copper net-work invariably *came off with it.* I at one time imagined this difficulty insuperable, as it appeared to me that I had cleared the cement entirely from the surface of the copper I meant to have exposed,—but that there was a difference in the molecular arrangement of copper prepared by heat, and that prepared by voltaic action, which prevented their chemical combination. However, I then determined, should this prove so, to turn it to account, in another manner, which I shall relate in the second portion of this paper.

I then occupied myself for a considerable period in making experiments on this latter section of the subject.

In one of them I found, on examination, a portion of the copper deposition, which I had been forming on the surface of a coin, adhered so strongly that I was quite unable to get it off,—indeed, a chemical combination had apparently taken place. This was only in one or two spots, on the prominent parts of the coin. I immediately recollected that on the day I put the experiment in action, I had been using nitric acid, for another purpose, on the table I was operating on, and that in all probability the coin might have been laid down where a few drops of the acid had accidentally fallen. I then took a piece of copper, coated it with cement, made a few scratches on its surface until the copper appeared, and immersed it for a short time in dilute nitric acid, until I perceived, by an elimination of nitrous gas, that the exposed portions were acted upon sufficiently to be slightly corroded. I then washed the copper in water, and put it in action as before described. In forty-eight hours I examined it, and found the lines were entirely filled with copper. I applied heat, and then spirits of turpentine, to get off the cement; and, to my satisfaction, I found that the voltaic copper had completely combined itself with the sheet on which it was deposited.

I then gave a plate a coating of cement, to a considerable thickness, and sent it to an engraver; but when it was returned, I found the lines were cleared out so as to be wedge-shaped, or somewhat in the form of a ∇, leaving a hair line of the copper exposed at the bottom, and a broad space near the surface; and where the turn of the letters took place, the top edges of the lines were galled and rendered rugged by the action of the graver. This, of course, was an important objection; which I have since been able to remedy, in some respects, by an alteration in the shape of the graver, which should be made of a shape more resembling a narrow parallelogram than those in common use,—some engravers have many of their tools so made. I did not put this plate in action, as I saw that the lines, when in relief, would have been broad at the top and narrow at the bottom. I took another plate, gave it a coating of the wax, and had it written on with a mere point. I deposited copper on the lines, and afterwards had it printed from.*

I now considered part of the difficulties removed: the principal one that yet remained was, to find a cement or etching-ground, the texture of which should be capable of being cut to the required depth,† without raising (what is technically termed) *a burr*, and, at the same time, of sufficient toughness to adhere to the plate, when reduced to a small isolated point, which would necessarily occur in the operation which wood-engravers term cross-hatching.

I tried a number of experiments with different combinations of wax, resins, varnishes, and earths, also with metallic oxides, —all with more or less success.

The one combination that exceeded all the others in its texture, having nearly every requisite (indeed I was enabled to polish the surface nearly as smooth as a plate of glass), was principally composed of virgin wax, resin, and carbonate of lead—the white lead of the shops.

With this compound I had two plates, 5 inches by 7, coated over, and portions of maps cut on the cement, which I had intended should have been printed off and laid before the British Association at its meeting. I applied the same process, to these, as to others—dipping them in dilute nitric acid before putting them in action: indeed I suffered them to remain about ten minutes in the solution. I then put them into the voltaic arrangement. The action proceeded, slowly and perfectly, for a few days,—when I removed them. I then applied heat, as usual, to remove the cement,—when all came away as in a former instance; the voltaic copper peeling off the plate with the greatest facility. I was much puzzled at this unexpected result, but, on cleaning the plate, I discovered a delicate tracing of *lead*, exactly corresponding to the lines drawn on the cement previous to the immersion in the dilute acid. The cause of this failure was at once obvious; the carbonate of lead I had used to compound the etching-ground had been decomposed by the dilute nitric acid, and the metallic lead thus set free had deposited itself on the exposed portions of the copper-plates, preventing the voltaic copper from chemically combining with the sheet copper. I was now obliged with regret to give up this compound—although, under other circumstances, I have no doubt it may be rendered available.—I adopted another, consisting of bees' wax, common whiting, resin, a small portion of gum, and plaster of paris. This seems to answer the purpose tolerably—though I have no doubt, by an extended practice, a better may still be obtained.

---

* This plate was shown, and also specimens of printing from it.

† I have since learnt, from practical engravers, that much less relief is necessary, to print from, than I had deemed indispensible; and that on becoming more familiar with the cutting of the wax-cement, they would be enabled to engrave in it with great facility and precision.

I now proceed to the second, and I believe the most satisfactory, portion of the subject. Although I have placed these experiments last, they were made simultaneously with the others already described; but, to render the subject more intelligible, I have placed them thus.

I have already stated that I was desirous of executing metallic ornaments by this means, in either cameo or intaglio; but, being well aware of the apparent natural law which prevents metallic deposition by voltaic electricity, *without* the presence of a metallic body, I perceived, in consequence, its uses, if any, would be extremely limited, as, whatever ornament it might produce, it would only be by adhering to the condition of a metallic mould.

I accordingly determined to make my first experiment on a very prominent copper medal. I placed it in a voltaic circuit as already described, and deposited a surface of copper on one of its sides to about the thickness of a shilling. I then proceeded to get the deposition off. In this I experienced some difficulty, but ultimately succeeded. On examination with a magnifying glass, I found every line was as perfect as the coin was from which it was taken. I was then induced to use the same piece again, and let it remain a much longer time in action, that I might have a thicker and more substantial mould. I accordingly put it again in action, and let it remain until it had acquired a much thicker coating of the metallic deposition; but when I attempted to remove it from the medal, I found I was unable. It had, apparently, completely adhered to it.

I had often practised, with some degree of success, a method of preventing the oxidation of polished steel, by slightly heating it until it would melt virgin wax; it was then wiped, apparently, completely off,—but the pores of the metal became impregnated with the wax.

I thought of this method, and applied it to a copper coin. I first heated the piece,—applied wax—and then wiped it so completely off, that the sharpness of the coin was not at all interfered with. I proceeded as before, and deposited a thick coating of copper on its surface, after the lapse of a few days. When I wished to take it off, I applied the heat of a spirit-lamp to the back; when a sharp crackling noise took place, and I had the satisfaction of perceiving that the coin was completely loosened. In short, I had a most complete and perfect copper mould of a halfpenny.

I have since taken some impressions from the mould thus taken; and, by adopting the above method with the wax, I get them out with the greatest ease.

I was now of opinion that this latter method might be applied to engraving much better than the method described in the first portion of this paper. Being aware that copper in a voltaic circuit deposited itself on lead with as much rapidity as on copper, I took a silver coin, and put it between two pieces of clean sheet lead, and placed them under a common screw press. From the softness of the lead, I had a complete and sharp mould of both sides of the coin. I then took a piece of copper wire,—soldered the lead to one end—and a piece of zinc to the other, and put them into the same voltaic arrangement I have already described. I did *not*, in this instance, *wax* the mould, as I felt assured that the deposited copper would easily separate from the lead, by the application of heat,—from the different expansibility of the two metals.

In this result I was not disappointed. When the heat of a spirit-lamp was applied for a few seconds to the lead, the copper impression fell easily off. So complete do I think this latter portion of the subject, that I have no hesitation in asserting that fac-similes of any coin or medal, no matter of what size, may be readily taken, and as sharp as the original.

To further test the capabilities of this method, I took a piece of lead plate, and stamped some letters on its surface to a depth sufficient to print from when in relief. I deposited copper on it, and found it came easily off.

I now come to the conclusion of my experiments on this subject. As I stated at first, my object was to deposit a metallic surface on a model of clay, or other *non*-metallic body,—as, otherwise, I imagined the application of this principle would be extremely limited. I made many experiments to achieve this result, which I shall not detail, but content myself with describing that which was ultimately the most successful.

I took two models of an ornament, one made of clay, and the other of plaster of paris; soaked them for some time in linseed oil; took them out, and suffered them to dry—first getting the oil clean off the surface. When dry, I gave them a thin coat of mastic varnish. When the varnish was as nearly dry as possible—*but not thoroughly so,* I sprinkled some bronze powder on that portion I wished to make a mould of. This powder is principally composed of mercury and sulphur. I had, however, a complete metallic coating on the surface of my model, by which I was enabled to deposit a surface of copper on it, by the voltaic method I have already described. I have also gilt the surface of a clay model with gold leaf, and have been successful in depositing the copper on its surface. There is likewise another, and (as I trust it will prove) a simpler method of attaining this object, but as I have not yet sufficiently tested it by experiment, I shall take another opportunity of detailing the method.

[At the close of the paper, several specimens of coins and medals—some of them in the act of formation by the voltaic process—were exhibited to the members.]

ADDENDA.*

TO ENGRAVE IN RELIEF ON A PLATE OF COPPER.

Take a plate of copper, such as are in use among engravers. It is not essential that it should be highly polished.

Have a piece of copper wire neatly soldered to the back part of it, and then give it a coating of either of the cements already mentioned. This is best done by heating the plate as well as the wax; or, to level the wax after it has had a coat, hold the back part of the plate over a charcoal fire, or spirit-lamp,—taking care to hold it level.

---

* *Note*—By this process, iron castings that are required to be preserved from the weather may have a coating of copper given to them, of any requisite thickness.

Then write, or draw the design, on the wax, with a black-lead pencil or a point. The wax must now be cut through with a graver, or a steel point,—taking special care that the copper *is exposed on every line.*

It must now be immersed in dilute nitric acid—say, three parts water to one acid.. It will be at once seen whether it is strong enough, by the green colour of the solution, and the bubbles of nitrous gas eliminated. Let it remain long enough to allow the exposed lines on the plate to be *slightly* corroded; that the wax (which gets into the pores of the copper during the heating process), may be thoroughly got rid of. Practice will determine this, better than any rules.

The plate is now ready to be placed in the voltaic apparatus (see Engraving, p. 3.) After the voltaic copper has been deposited in the lines engraved in the wax, the surface of the formation will be found to be rough, more or less, according to the quickness of the action. To remedy this, rub the surface with a piece of smooth flag or pumice-stone, with water. Then heat the plate, and wash off the wax groundwork with spirits of turpentine and a brush. The plate is now ready to be printed from at an ordinary press.

TO DEPOSIT A SOLID VOLTAIC PLATE, HAVING THE LINES IN RELIEF.

Take a plate of copper, lead, silver, or type-metal, of the required size, and engrave in it, to the depth requisite to print from, when in relief.

Contrary to ordinary engraving, the lines must be *flat* at the bottom, and as nearly as possible of the same depth. When so engraved (should the plate be copper or silver), heat it, and then apply a little bees' wax (what is termed virgin wax is preferable) mixed with a very small proportion of spirits of turpentine; and give the plate a coating of it. It may be laid on in a lump; and the heat of the plate should be sufficient to melt it. When on the eve of cooling, the plate should be wiped clean, and all the wax taken off,—as sufficient will have entered the pores of the plate, to prevent the voltaic copper from adhering.

Then solder a piece of copper wire.

The plate must now receive a couple of coats of thick varnish on the back and edges (a preparation of shell-lac and alcohol does very well). I prefer, if the plate is large, to imbed it with plaster of paris or roman cement, in a box the size of the plate, allowing the wooden edge of the box to project just as much above the surface of the plate, as you wish the thickness of the voltaic one to be. (Care must be taken, to keep the engraved surface of the plate clean).

It is now ready to be placed in the apparatus to be deposited on.

Should the plate be lead,—or, what is still better, type-metal,—the preparation of wax does not require to be given to the plate, as, when it is deposited on to the given thickness, applying heat is sufficient to loosen the plates.

TO PROCURE FAC-SIMILES OF MEDALS, &c.

This may be done by two different methods; the one, by depositing a *mould* of the voltaic metal on the face of the medal, (having first heated it, and applied wax), and then depositing the metal (by a subsequent operation) in the mould so formed.

But the more ready way is, to take two pieces of milled *sheet* lead,[*] (cast lead not being equally soft), having surfaces perfectly clean and free from indentation. Put the medal between the two pieces of lead, subjecting the whole to pressure in a screw-press.[†] A complete mould of both sides is thus formed in the lead, showing the most delicate lines perfect (in reverse). Twenty – or even a hundred—of these may be so formed on one sheet of lead, and are deposited by the voltaic process with equal or greater facility; as, the more extensive the apparatus, the more regularly and expeditiously does the operation proceed.—Those portions of the surface of the lead, where the moulds do not occur, may be varnished,

[*] It may be necessary to note, that the voltaic mould will also require the application of the wax.

[†] A common copying-machine will serve the purpose, for a small medal, not having much relief.

Should the medal be large, and in bold relief, it would be better to have a small portion of the lead cut out, or turned in a lathe, so that the medal might (to a certain extent) fit into the lead before being pressed up; this will prevent injury of the medal, and give a rim to the fac-simile.

to neutralize the voltaic action ; or, (a whole sheet of copper being deposited), the voltaic medals may afterwards be cut out.

A piece of wire must now be soldered neatly to the back of the leaden plate ; it is then ready to be put in action.

[This applies to the formation of one side of a medal only. It requires extremely careful manipulation to form both sides; and as I think there may be a better method than the one I have hitherto adopted, I defer stating it until I have obtained the result of an experiment now in operation.]

A VOLTAIC IMPRESSION FROM A PLASTER OR CLAY MODEL.

This process is partially described in a preceding page;* in addition to which I may state, that when the plaster or clay ornament is gilt with gold leaf, or bronzed, a copper wire should be attached to it, by running through from the back, until the point appears above the front surface,—or level with it will be sufficient. The other end must then be attached to the binding screw connecting it with the zinc, in all respects similar to any of the foregoing methods.

TO OBTAIN ANY NUMBER OF COPIES FROM AN ALREADY ENGRAVED COPPER-PLATE.

A copper-plate may be taken, engraved in the common manner—the lines being in *intaglio.* Procure an equal-sized piece of sheet-lead ; lay it on the engraved side of the plate, and put both under a *very powerful* press ; when taken out, the lead will have every line, in relief, that had been sunk in the copper.

A wood engraving may be operated on in like manner ; —as lead, being pressed into it, will not injure it.

A wire may now be soldered to the lead, then bed it in a box ; and put it into the voltaic apparatus,—when a copper-plate, being an exact fac-simile of the original, will be formed.

In this process, care must be taken that the lead is clean and bright, as it comes from the roller in the milling-process, and consequently free from any oxidation, which it soon acquires, if exposed to the atmosphere. It should be put in action as soon as possible after being taken out of the press.

---

* See page 263.

REMARKS

ON THE MANAGEMENT OF THE APPARATUS.*

Next to electro-magnetism, there is no branch of science that requires more dexterous manipulation than voltaic, or electro-chemistry; the most trifling film of oxidation often retarding the action of the most powerful apparatus. But, in the present instance, slow action, and simplicity of arrangement, being the predominating features, such nice attention to minutiæ is not absolutely necessary,—or at least not so much so as to deter those hitherto unacquainted with the subject from practising.

In *all* cases, to ensure metallic connexion, binding-screws are preferable to cups of mercury; but, in using them, the copper wire, where the attachment is made, must be brightened with a piece of emery paper,—also the point of the screw, where it presses on the wire. In soldering the wires to the plates, let as little resin be used as possible; sal ammoniac, or dilute muriatic acid, answers the purpose much better.

In these experiments, I have invariably found an *equal sized* piece of zinc to answer best. In the construction of galvanic batteries in general, I am aware, this is a moot point with high authority; but my own practice, which has been by no means small, with batteries of every construction, has led me to the opinion that, wherever slow and equable action is required, the positive and negative electrodes should be of equal *superficial* area.—Although amalgamated zinc plates are preferable where combined intensity and continuity of action are required, they must not be used, under any circumstances, for the present purposes.—It will, likewise, be found to be essential that the *thickness* of the zinc be equal to that of the required deposition.

Let the porous bottom of the interior vessel, containing the zinc, be a little larger than either of the electrodes. I have hitherto used, for this purpose, either bottomless glass cylinders, or wooden boxes, varnished, with plaster bottoms; but I should recommend a well glazed earthenware vessel, having no bottom, but a slight rim projecting inwards, to secure the plaster. The zinc should be occasionally taken out of the arrangement, during continuance of the process, and cleansed by washing it in water; the saline solution may also be renewed.

---

[*] These observations are intended for the guidance, in the first instance, of those who are practically unacquainted with voltaic arrangements.

Crystals of sulphate of copper should be added, from time to time, to the cupreous solution; but, should the deposition require to be thick and long-continued, it will be necessary to take out the cupreous solution once or twice during the operation, and add an entirely fresh one,—as the sulphuric acid, necessarily set free after the de-oxidisement of the copper, when it predominates to any extent, prevents the required action from taking place on the copper; instead of which, a sub or di-oxide of copper is deposited, in the form of a reddish brown powder—the solution being rendered colourless. When this takes place, the plate should be taken out, and well washed in very dilute nitric acid. I have tried several methods to take up the sulphuric acid as it was set free; pure clay answers this purpose pretty well, the acid combining to a certain extent with it, and forming a sulphate of alumina, or alum, at the bottom of the vessel.

When the voltaic copper is bent, it breaks at a similar angle to cast copper; but when heated to a red heat, and slowly cooled, it assumes somewhat of the pliability of rolled sheet copper, requiring to be bent several times before breaking; should it now be beaten on an anvil, it will resume its brittleness.

It may be filed, polished, and cut with shears, in the usual manner—the surface acquiring as fine a polish as the copper in use among engravers.

Should a thick mass of metal be requisite for any practical purpose: as it would require a considerable lapse of time before it could be obtained by the voltaic process,* the back of the deposited metal may be thickened, or filled up with solder, in a manner already practised in the arts, without the slightest injury to the surface or texture of the deposited metal.

---

* To deposit metal equal to one-eighth of an inch in thickness, requires about eight or ten days' continuous action: the superficial extent of the deposition not being material, as regards the duration of the process.

DESCRIPTION (AND ACCOMPANYING VIEW) OF THE APPARATUS.

FIGURE 1.

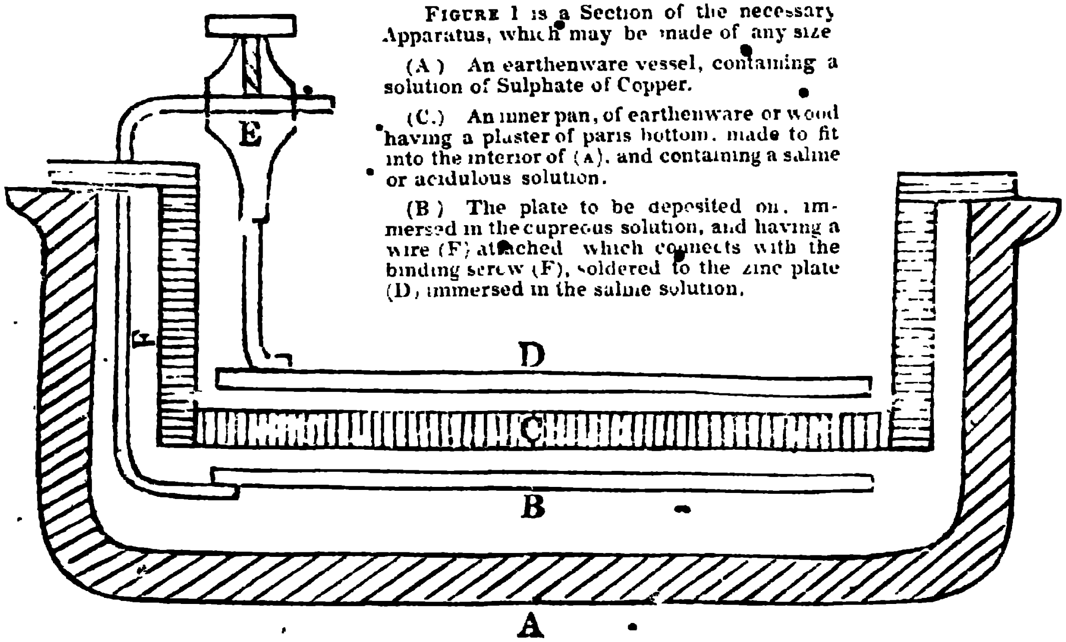


Figure 1 is a Section of the necessary Apparatus, which may be made of any size

(A) An earthenware vessel, containing a solution of Sulphate of Copper.

(C.) An inner pan, of earthenware or wood having a plaster of paris bottom, made to fit into the interior of (A), and containing a saline or acidulous solution.

(B) The plate to be deposited on, immersed in the cupreous solution, and having a wire (F) attached which connects with the binding screw (F), soldered to the zinc plate (D), immersed in the saline solution.

FIGURE 2.

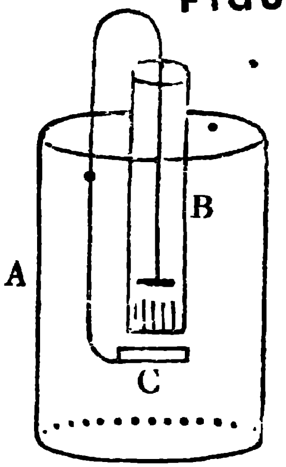


Figure 2 is a more simple apparatus,--but on the same principle as Fig 1. This is adapted for experiments on a small scale; or to take a fac-simile of a single medal

A, may represent a common drinking-glass, containing the copper solution

B, a gas-glass having one end closed with plaster of paris: and containing the saline solution.

C, the plate or medal, required to be deposited on, having a plate attached, to the other end of which is a piece of zinc,--the wire being bent into the requisite form

*** The above engraving has been produced (in relief) by the Electro-chemical Process described in the preceding pages;—and is the first result of that process appearing in print.

It is however by no means a fair specimen of what *may* be done by it.—The lines were originally cut in a sheet of soft lead, hastily, and without reference to ornamental beauty of execution. The want of a tool properly adapted to cut the lead, accurately, in the required manner, has made the lines less regular in formation than they would otherwise have been. The letters were punched into the lead, with types. The lead, thus prepared, was then put into the apparatus it illustrates, and a plate of copper deposited, the lines being in relief,—which is here printed off, accompanying the pages of the treatise.

NOTE.

We have read Mr. Spencer's paper with much pleasure and satisfaction; and must certainly express our extreme surprise at our author's extraordinary reception at the Birmingham Meeting of the British Association, *for the promotion of Science.* We have not the pleasure of knowing who Mr. Spencer is, nor what is his position in society: but no one, who reads the preceding paper, can hesitate acknowledging that he is a "scientific man." As for ourselves, we have no hesitation in stating, that Mr. Spencer's paper is, without exception, the most interesting one, on voltaic electricity, that has hitherto been presented to the notice of the British Association *for the promotion* of Science. EDIT.

ADDITIONAL INFORMATION FROM MR. SPENCER.

To the Editor of the Liverpool Mercury.

Sir,

In your last number you expressed some doubts as to the correctness of the account of my process, given in the Athenæum, and condensed from the account given in the pamphlet published by the Polytechnic Society. I have since looked over it, and find nothing absolutely incorrect, yet in the attempt to condense the details of the process, there is a turbidness which materially interferes with a proper understanding of the whole. I shall now detail to you, as briefly as possible, the method I would adopt to copy a wood engraving in copper,—and as it will apply to some other processes connected with the subject, it will, I trust, not be unacceptable to your readers.

I may premise that, but for the plasticity and perfectly unelastic property of lead, the discovery would be of but comparatively small value. Plumbers who have handled the substance for the greater portion of their lives, are astonished to find it so susceptible of pressure; on the contrary, wood engravers did not, until now, imagine their blocks would stand the pressure of a screw press on a lead surface without injury; but such is the fact in both instances. In the manner in which box wood is used for wood engravings, being horizontal sections, it will sustain a pressure of 8000lbs. without injury, provided the pressure is perfectly perpendicular.

The wood engraving being given, take a piece of sheet lead the requisite size; let its superfice be about one-eighth of an inch larger all round than that of the wooden block. The lead must now be planed with a common plane, just as a piece of soft wood: the tool termed by the joiner the try plane does best;—a clear bright surface is thus obtained, such as I have been unable to get by any other means. The engraved surface of the wood must now be laid on the planed surface of the lead, and both put carefully in the press; should the engraving have more than two inches of superfices, a copying press is not powerful enough. Whatever press is used, the subject to be copied must be cautiously laid in the centre of the pressure, as a very slight lateral force will in some degree injure the process. The lead to be impressed upon must rest on the iron plate of the press, as must the back part of the wood engraving; the pressure to be applied regularly, and not, as in some cases with a jerk. When the pressure is deemed complete, they may be taken out; and if, on examination, the lead is not found to be completely up, the wood engraving may be neatly relaid on the lead, and again submitted to the press, using the same precaution as before.

When the lead is taken out a wire should be soldered to it *immediately*, and put into the apparatus without loss of time, as the less it is subjected to the action of the atmosphere the better;—care should also be taken not to touch the surface with the fingers. In the pamphlet I stated the length of time usually taken to deposit the required thickness of metal;—I have been since able to abridge that period three or four fold, as I keep the solutions at a temperature of from 120 to 180 Farenheit. It has been suggested to me, by Mr. Crosse, of Broomfield, to keep the solutions boiling, which still further increases the rapidity of the deposition. Contrary to general chemical analogy the deposited metal is of a much superior quality to that deposited by the very slow action of a common temperature.

At the same time it must be borne in mind, that if the process is quickened by strengthening the solution in the positive cell by the addition of an acid, the metal deposited in the opposite one is of a very inferior quality; so much so as to be totally unfit for any practical purpose. Under these circumstances the deoxidizing process is not completed, the deposit being a reddish brown protoxide of copper; this last, if let remain for a few days longer, undergoes a still further change, it then becomes a black oxide of copper, such as may be used for organic analysis; and, were I to pursue this branch of chemistry, I should never resort to any other method of obtaining it.

The above process will apply to copying engraved copper-plates, or medallions.

I have also been able to obtain impressions from wood engravings by the following method. Take a piece of tinfoil the size, or thereabouts, of the engraving; place it on the engraved surface; over this place a piece of sheet India rubber, and put the whole in a press; on taking out of which it will be found the tin is thoroughly impressed into the lines of the wood. A coating of plaster of Paris must now be laid on the tin to about half an inch in thickness; when set, the whole may be taken off the wooden block. It will be found that the tin adheres to the plaster, and leaves the face of the engraving. The tin surface may now be deposited on to any required thickness. The above was tried on a coarse wood engraving. I am unable to say how it might answer for a fine one.

I have been more than once reminded of the fusible metal, that melts at a temperature of boiling water, but have had no opportunity of trying it; it might be applicable for copying wood engravings.

I have yet another method which I am in hopes will still further improve the process, but as it is not matured I shall take a future opportunity of communicating it: being a modification of the apparatus, it will require an engraving to explain it.

Yours, &c.,
THOMAS SPENCER.

XXXVII. *On the use of Voltaic Electricity. In a letter to the Rev. J. B. Reade. By* Mr. Sturgeon.

Westmoreland Cottage,
December 2, 1839.

My dear Sir,

During our conversation, this morning, on the subject of taking fac simile impressions, in copper, of medallions, coins, &c., by the process of Voltaism, you will remember that the idea occurred to me of giving them silver or golden surfaces, by a similar voltaic process; employing a solution of either of those metals in connexion with the *prepared* matrix, instead of a solution of copper. Turning the subject over in my mind whilst walking home, a thought struck me that a *complete* medallion of any kind of metal might easily be made by the voltaic process; or the medallion might be constructed of different metals and in a variety of ways, which it would be difficult to imitate by any other process.

The following are some of the methods:—

Let a matrix of each side of the medallion intended to be copied be made in the usual way, by means of the alloy usually called *Newton's fusible metal,* and let the metal be about an eighth of an inch in thickness. To the back of this metal is to be soldered one end of a copper wire, and to the other end a piece of zinc, which is afterwards to be amalgamated. The metal in which the matrix is formed is now to be covered with a thin stratum of either varnish or wax, leaving bare the matrix only. The wire is also to be covered in a similar manner, and is to be bent so as to adapt the voltaic metals to their respective positions in the vessels holding the liquids employed. In a few hours the matrix will have received a coating of precipitated metal from the solution, which may be either gold or silver: the thickness of the coating will depend upon the time. When this coating is supposed to be of sufficient thickness, remove the solution of the silver or the gold, as the case may be, and replace it by a solution of the sulphate of copper, and in the course of a few days you will have a considerable thickness of copper precipitated on the silver coating in the matrix. These two metals will adhere firmly together so as to be one piece. When this young semi-medallion is removed from the matrix, it will have a copper body with a silver or a gold face. Its twin sister may be formed by proceeding in the same way, with the matrix formed from the opposite face of the original medallion, and when the process is completed the flat copper sides may be soldered neatly together, so as to form a complete medallion similar to the original one.

By a similar process a complete medallion may be formed having a gold surface on one side and a silver one on the other.

Another beautiful variation may be made by the following process. Imagine that we wanted a medallion whose *prominent* parts should be of gold, and the rest silver. The head of NEWTON, for instance, with its motto, to be of gold. Varnish with wax every other part of the matrix, and put it in galvanic action in a solution of gold. In a few hours a golden head and motto will be formed. Now remove the gold solution; and clean the matrix of its coating of wax. Now put the matrix in voltaic action in a solution of silver, and the face of the new medallion will be filled up with silver. If the body of the medallion is to be silver, the action may be continued for a few days; but if the body is to be of copper, proceed as before directed with a solution of sulphate of copper. Similar processes give infinite scope to the ingenious in varying and ornamenting this class of voltaic productions.

I am, my dear Sir,
Yours very truly,
W. STURGEON.

To the Rev. J. B. Reade.

XXXVIII. *Contributions to Electricity and Magnetism.*

By Joseph Henry, L.L.D., *Professor of Natural Philosophy, in the College of New Jersey, Princeton.**

On Electro-Dynamic Induction.

INTRODUCTION.

1. Since my investigations in reference to the influence of a spiral conductor, in increasing the intensity of a galvanic current, were submitted to the Society, the valuable paper of Dr. Faraday, on the same subject, has been published, and also various modifications of the principle have been made by Sturgeon, Masson, Page, and others, to increase the effects. The spiral conductor has likewise been applied by Cav. Antinori to produce a spark by the action of a thermo-electrical pile: and Mr. Watkins has succeeded in exhibiting all the phenomena of hydro-electricity by the same means. Although the principle has been much extended by the researches of Dr. Faraday, yet I am happy to state that the results obtained by this distinguished philosopher are not at variance with those given in my paper.

2. I now offer to the Society a new series of investigations in the same line, which I hope may also be considered of sufficient importance to merit a place in the Transactions.

3. The primary object of these investigations was to discover, if possible, inductive actions in common electricity analogous to those found in galvanism. For this purpose a series of experiments was commenced in the spring of 1836, but I was at that time diverted, in part, from the immediate object of my research, by a new investigation of the phenomena known in common electricity by the name of the lateral discharge. Circumstances prevented my doing any thing further, in the way of experiment, until April last, when most of the results which I now offer to the Society were obtained. The investigations are not as complete, in

---

* Communicated by the Author.

several points, as I could wish, but as my duties will not permit me to resume the subject for some months to come, I therefore present them as they are; knowing, from the interest excited by this branch of science in every part of the world, that the errors which may exist will soon be detected, and the truths be further developed.

4. The experiments are given nearly in the order in which they were made; and in general they are accompanied by the reflections which led to the several steps of the investigation. The whole series is divided, for convenience of arrangement, into six sections, although the subject may be considered as consisting, principally, of two parts. The first relating to a new examination of the induction of galvanic currents; and the second to the discovery of analogous results in the discharge of ordinary electricity.*

5. The principal articles of apparatus used in the experiments, consist of a number of flat coils of copper riband, which will be designated by the names of coil No. 1, coil No. 2, &c.: also of several coils of long wire; and these, to distinguish them from the ribands, will be called helix No. 1, helix No. 2, &c.

6. Coil No. 1 is formed of thirteen pounds of copper plate, one inch and a half wide and ninety-three feet long. It is well covered with two coatings of silk, and was generally used in the form represented in fig. 2, Plate VI., which is that of a flat spiral sixteen inches in diameter. It was however sometimes formed into a ring of larger diameter, as is shown in fig. 5.

7. Coil No. 2 is also formed of copper plate, as the same width and thickness as coil No. 1. It is, however, only sixty feet long. Its form is shown at *b*, fig. 2. The opening at the centre is sufficient to admit helix No. 1. Coils No. 3, 4, 5, 6, &c. are all about sixty feet long, and of copper plate of the same thickness but of half the width of coil, No. 1.

8. Helix No. 1 consists of sixteen hundred and sixty yards of copper wire, $\frac{1}{49}$th of an inch in diameter. No. 2, of nine hundred and ninety yards: and No. 3, of three hundred and fifty yards, of the same wire. These helices are shown in fig. 3, and are so adjusted in size as to fit into each other; thus forming one long helix of three thousand yards: or, by using them separately, and in different combinations,

---

* The several paragraphs are numbered in succession, from the first to the last, after the mode adopted by Mr. Faraday. for convenience of reference.

seven helices of different lengths. The wire is covered with cotton thread, saturated with bees' wax, and between each stratum of spires a coating of silk is interposed.

9. Helix No. 4, is shown at *a*, fig. 5; it is formed of five hundred and forty-six yards of wire; $\frac{1}{45}$th of an inch in diameter, the several spires of which are insulated by a coating of cement. Helix No. 5 consists of fifteen hundred yards of silvered copper wire, $\frac{1}{125}$th of an inch in diameter, covered with cotton, and is of the form of No. 4.

10. Besides these I was favoured with the loan of a large spool of copper wire, covered with cotton, $\frac{1}{16}$th of an inch in diameter, and five miles long. It is wound on a small axis of iron, and forms a solid cylinder of wire, eighteen inches long, and thirteen in diameter.

11. For determining the direction of induced currents, a magnetizing spiral was generally used, which consists of about thirty spires of copper wire, in the form of a cylinder, and so small as just to admit a sewing needle into the axis.

12. Also a small horse-shoe is frequently referred to, which is formed of a piece of soft iron, about three inches long, and ^{2}ths of an inch thick; each leg is surrounded with about five feet of copper bell wire. This length is so small, that only a current of electricity of considerable quantity can develope the magnetism of the iron. The instrument is used for indicating the existence of such a current.

13. The battery used in most of the experiments is shown in fig. 2. It is formed of three concentric cylinders of copper, and two interposed cylinders of zinc. It is about eight inches high, five inches in diameter, and exposes about one square foot and three quarters of zinc surface, estimating both sides of the metal. In some of the experiments a larger battery was used, weakly charged, but all the results mentioned in the paper, except those with a Cruickshank trough, can be obtained with one or two batteries of the above size, particularly if excited by a strong solution. The manner of interrupting the circuit of the conductor by means of a rasp, *b*, is shown in the same figure.

SECTION I.

Conditions which influence the induction of a Current on itself.

14. The phenomenon of the spiral conductor is at present known by the name of the induction of a current on itself, to distinguish it from the induction of the secondary current,

discovered by Dr. Faraday. The two, however, belong to the same class, and experiments render it probable that the spark given by the long conductor is, from the natural electricity of the metal, disturbed for an instant by the induction of the primary current. Before proceeding to the other parts of these investigations, it is important to state the results of a number of preliminary experiments, made to determine more definitely the conditions which influence the action of the spiral conductor.

15. When the electricity is of low intensity, as in the case of the thermo-electrical pile, or a large single battery weakly excited with dilute acid, the flat riband coil No. 1, ninety-three feet long, is found to give the most brilliant deflagrations, and the loudest snaps from a surface of mercury. The shocks, with this arrangement, are, however, very feeble, and can only be felt in the fingers or through the tongue.

16. The induced current in a short coil, which thus produces deflagration, but not shocks, may, for distinction, be called one of quantity.

17. When the length of the coil is increased, the battery continuing the same, the deflagrating power decreases, while the intensity of the shock continually increases. With five riband coils, making an aggregate length of three hundred feet, and the small battery, fig. 2, the deflagration is less than with coil No. 1, but the shocks are more intense.

18. There is, however, a limit to this increase of intensity of the shock, and this takes place when the increased resistance or diminished conduction of the lengthened coil begins to counteract the influence of the increasing length of the current. The following experiment illustrates this fact. A coil of copper wire, $\frac{1}{16}$th of an inch in diameter, was increased in length by successive additions of about thirty-two feet at a time. After the first two lengths, or sixty-four feet, the brilliancy of the spark began to decline, but the shocks constantly increased in intensity, until a length of five hundred and seventy-five feet was obtained, when the shocks also began to decline. This was then the proper length to produce the maximum effect with a single battery, and a wire of the above diameter.

19. When the intensity of the electricity of the battery is increased, the action of the short riband coil decreases. With a Cruickshank's trough of sixty plates, four inches square, scarcely any peculiar effect can be observed, when the coil forms a part of the circuit. If however the length of the coil be increased in proportion to the intensity of the current, then the inductive influence becomes apparent.

When the current, from ten plates of the above-mentioned trough, was passed through the wire of the large spool (10), the induced shock was too severe to be taken through the body. Again, when a small trough of twenty-five one-inch plates, which alone would give but a very feeble shock, was used with helix No. 1, an intense shock was received from the induction, when the contact was broken. Also a slight shock in this arrangement is given when the contact is formed, but it is very feeble in comparison with the other. The spark, however, with the long wire and compound battery is not as brilliant as with the single battery and the short riband coil.

20. When the shock is produced from a long wire, as in the last experiments, the size of the plates of the battery may be very much reduced, without a corresponding reduction of the intensity of the shock. This is shown in an experiment with the large spool of wire (10). A very small compound battery was formed of six pieces of copper bell wire, about one inch and a half long, and an equal number of pieces of zinc of the same size. When the current from this was passed through the five miles of the wire of the spool, the induced shock was given at once to twenty-six persons joining hands. This astonishing effect placed the action of a coil in a striking point of view.

21. With the same spool and the single battery used in the former experiments, no shock, or at most a very feeble one, could be obtained. A current, however, was found to pass through the whole length, by its action on the galvanometer; but it was not sufficiently powerful to induce a current which could counteract the resistance of so long a wire.

22. The induced current in these experiments may be considered as one of *considerable intensity*, and *small quantity*.

23. The form of the coil has considerable influence on the intensity of the action. In the experiments of Dr. Faraday, a long cylindrical coil of thick copper wire, inclosing a rod of soft iron, was used. This form produces the greatest effect when magnetic reaction is employed; but in the case of simple galvanic induction, I have found the form of the coils and helices represented in the figures most effectual. The several spires are more nearly approximated, and therefore they exert a greater mutual influence. In some cases, as will be seen hereafter, the ring form, shown in fig. 5, is most effectual.

24. In all cases the several spires of the coil should be well insulated, for although in magnetizing soft iron, and in

analogous experiments, the touching of two spires is not attended with any great reduction of action; yet in the case of the induced current, as will be shown in the progress of these investigations, a single contact of two spires is sometimes sufficient to neutralize the whole effect.

25. It must be recollected that all the experiments with these coils and helices, unless otherwise mentioned, are made without the reaction of iron temporarily magnetized; since the introduction of this would, in some cases, interfere with the action, and render the results more complex.

SECTION II.

Conditions which influence the production of Secondary Currents.

26. The secondary currents, as it is well known, were discovered in the induction of magnetism and electricity, by Dr. Faraday, in 1831. But he was at that time urged to the exploration of new, and apparently richer veins of science, and left this branch to be traced by others. Since then, however, attention has been almost exclusively directed to one part of the subject, namely, the induction from magnetism, and the perfection of the magneto-electrical machine. And I know of no attempts, except my own, to review and extend the purely electrical part of Dr. Faraday's admirable discovery.

27. The energetic action of the flat coil, in producing the induction of a current on itself, led me to conclude that it would also be the most proper means for the exhibition and study of the phenomena of the secondary galvanic currents.

28. For this purpose coil No. 1 was arranged to receive the current from the small battery, and coil No. 2 placed on this, with a plate of glass interposed to insure perfect insulation; as often as the circuit of No. 1 was interrupted, a powerful secondary current was induced in No. 2. The arrangement is the same as that exhibited in fig. 4, with the exception that in this the compound helix is represented as receiving the induction, instead of coil No. 2.

29. When the ends of the second coil were rubbed together, a spark was produced at the opening. When the same ends were joined by the magnetizing spiral (11), the inclosed needle became strongly magnetic. Also when the secondary current was passed through the wires of the iron horse-shoe (12), magnetism was developed; and when the ends of the second coil were attached to a small decomposing

apparatus, of the kind which accompanies the magneto electrical machine, a stream of gas was given off at each pole. The shock, however, from this coil is very feeble, and can scacely be felt above the fingers.

30. This current has therefore the properties of one of moderate intensity, but considerable quantity.

31. Coil No. 1 remaining as before, a longer coil, formed by uniting Nos. 3, 4, and 5, was substituted for No. 2. With this arrangement, the spark produced when the ends were rubbed together, was not as brilliant as before; the magnetizing power was much less; decomposition was nearly the same, but the shocks were more powerful, or, in other words, the intensity of the induced current was increased by an increase of the length of the coil, while the quantity was apparently decreased.

32. A compound helix, formed by uniting Nos. 1 and 2, and therefore containing two thousand six hundred and fifty yards of wire, was next placed on coil No. 1. The weight of this helix happened to be precisely the same as that of coil No. 2, and hence the different effects of the same quantity of metal in the two forms of a long and short conductor, could be compared. With this arrangement the magnetizing effects, with the apparatus before mentioned, disappeared. The sparks were much smaller, and also the decompositions less, than with the short coil; but the shock was almost too intense to be received with impunity, except through the fingers of one hand. A circuit of fifty-six of the students of the senior class, received it at once from a single rupture of the battery current, as if from the discharge of a Leyden jar weakly charged. The secondary current in this case was one of small quantity, but of great intensity.

33. The following experiment is important in establishing the fact of a limit to the increase of the intensity of the shock, as well as the power of decomposition, with a wire of a given diameter. Helix No. 5, which consists of wire only, $\frac{1}{125}$th of an inch in diameter, was placed on coil No. 2, and its length increased to about seven hundred yards. With this extent of wire, neither decomposition nor magnetism could be obtained, but shocks were given of a peculiarly pungent nature; they did not however produce much muscular action. The wire of the helix was further increased to about fifteen hundred yards; the shock was now found to be scarcely perceptible, in the fingers.

34. As a counterpart to the last experiment, coil No. 1 was formed into a ring of sufficient internal diameter to admit the great spool of wire (11), and with the whole length of

this (which, as has before been stated, is five miles) the shock was found so intense as to be felt at the shoulder, when passed only through the forefinger and thumb. Sparks and decomposition were also produced, and needles rendered magnetic. The wire of this spool is $\frac{1}{10}$th of an inch thick, and we therefore see from this experiment, that by increasing the diameter of the wire, its length may also be much increased, with an increased effect.

35. The fact (33) that the induced current is diminished by a further increase of the wire, after a certain length has been attained, is important in the construction of the magneto-electrical machine, since the same effect is produced in the induction of magnetism Dr. Goddard of Philadelphia, to whom I am indebted for coil No. 5, found that when its whole length was wound on the iron of a temporary magnet, no shocks could be obtained. The wire of the machine may therefore be of such a length, relative to its diameter, as to produce shocks, but no decomposition; and if the length be still further increased, the power of giving shocks may also become neutralized.

36. The inductive action of coil No. 1, in the foregoing experiments, is precisely the same as that of a temporary magnet in the case of the magneto-electrical machine. A short thick wire around the armature gives brilliant deflagrations, but a long one produces shocks. This fact, I believe, was first discovered by my friend Mr. Saxton, and afterwards investigated by Sturgeon and Lenz.

37. We might, at first sight, conclude, from the perfect similarity of these effects, that the currents, which, according to the theory of Ampère, exist in the magnet, are like those in the short coil, of great quantity and feeble intensity; but succeeding experiments will show that this is not necessarily the case.

38. All the experiments given in this section have thus far been made with a battery of a single element. This condition was now changed, and a Cruickshank trough of sixty pairs substituted. When the current from this was passed through the riband coil No. 1, no indication, or a very feeble one, was given of a secondary current in any of the coils or helices, arranged as in the preceding experiments. The length of the coil, in this case, was not commensurate with the intensity of the current from the battery. But when the long helix No. 1, was placed instead of coil No. 1, a powerful inductive action was produced on each of the articles, as before.

39. First, helices No. 2 and 3 were united into one, and placed within helix No. 1, which still conducted the battery current. With this disposition a secondary current was produced, which gave intense shocks but feeble decomposition, and no magnetism in the soft iron horseshoe. It was therefore one of intensity, and was induced by a battery current also of intensity.

40. Instead of the helix used in the last experiment for receiving the induction, one of the coils (No. 3) was now placed on helix No. 1, the battery remaining as before. With this arrangement the induced current gave no shocks, but it magnetized the small horseshoe; and when the ends of the coil were rubbed together, produced bright sparks. It had therefore the properties of a current of quantity; and it was produced by the induction of a current, from the battery, of intensity.

41. This experiment was considered of so much importance, that it was varied and repeated many times, but always with the same result; it therefore establishes the fact *that an intensity current can induce one of quantity*, and, by the preceding experiments, the converse has also been shown, that *a quantity current can induce one of intensity*.

42. This fact appears to have an important bearing on the law of the inductive action, and would seem to favour the supposition that the lower coil, in the two experiments with the long and short secondary conductors, exerted the same amount of inductive force, and that in one case this was expended (to use the language of theory) in giving a great velocity to a small quantity of the fluid, and in the other in producing a slower motion in a larger current; but in the two cases, were it not for the increased resistance to conduction in the longer wire, the quantity multiplied by the velocity would be the same. This, however, is as yet a hypothesis, but it enables us to conceive how intensity and quantity may both be produced from the same induction.

43. From some of the foregoing experiments we may conclude, that the quantity of electricity in motion in the helix is really less than in the coil, of the same weight of metal; but this may possibly be owing simply to the greater resistance offered by the longer wire. It would also appear, if the above reasoning be correct, that to produce the most energetic physiological effects, only a small quantity of electricity, moving with great velocity, is necessary.

44. In this and the preceding section, I have attempted to give only the general conditions which influence the galvanic induction. To establish the law would require a great num-

ber of more refined experiments, and the consideration of several circumstances which would affect the results, such as the conduction of the wires, the constant state of the battery, the method of breaking the circuit with perfect regularity, and also more perfect means than we now possess of measuring the amount of the inductive action; all these circumstances render the problem very complex.

SECTION III.

On the Induction of Secondary Currents at a distance.

45. In the experiments given in the two preceding Sections, the conductor which received the induction, was separated from that which transmitted the primary current by the thickness only of a pane of glass; but the action from this arrangement was so energetic, that I was naturally led to try the effect at a greater distance.

46. For this purpose coil No. 1 was formed into a ring of about two feet in diameter, and helix No. 4 placed as is shown in fig. 5. When the helix was at the distance of about sixteen inches from the middle of the plane of the ring, shocks could be perceived through the tongue, and these rapidly increased in intensity as the helix was lowered, and when it reached the plane of the ring they were quite severe. The effect, however, was still greater, when the helix was moved from the centre to the inner circumference, as at *c*: but when it was placed without the ring, in contact with the outer circumference, at *b*, the shocks were very slight; and when placed within, but its axis at right angles to that of the ring, not the least effect could be observed.

47. With a little reflection, it will be evident that this arrangement is not the most favourable for exhibiting the induction at a distance, since the side of the ring, for example, at *c*, tends to produce a current revolving in one direction in the near side of the helix, and another in an opposite direction in the farther side. The resulting effect is therefore only the difference of the two, and in the position as shown in the figure; this difference must be very small, since the opposite sides of the helix are approximately at the same distance from *c*. But the difference of action on the two sides constantly increases as the helix is brought near the side of the ring, and becomes a maximum when the two are in the position of internal contact. A helix of larger diameter would therefore produce a greater effect.

48. Coil No. 1 remaining as before, helix No. 1, which is nine inches in diameter, was substituted for the small helix

of the last experiment, and with this the effect at a distance was much increased. When coil No. 2 was added to coil No. 1, and the currents from two small batteries sent through these, shocks were distinctly perceptible through the tongue, when the distance of the planes of the coils and the three helices, united as one, was increased to thirty-six inches.

49. The action at a distance was still further increased by coiling the long wire of the large spool into the form of a ring of four feet in diameter, and placing parallel to this another ring, formed of the four ribands of coils No. 1, 2, 3, and 4. When a current from a single battery of thirty-five feet of zinc surface was passed through the riband conductor, shocks through the tongue were felt when the rings were separated to the distance of four feet. As the conductors were approximated, the shocks became more and more severe; and when at the distance of twelve inches, they could not be taken through the body.

50. It may be stated in this connexion, that the galvanic induction of magnetism in soft iron, in reference to distance, is also surprisingly great. A cylinder of soft iron, two inches in diameter and one foot long, placed in the centre of the ring of copper riband, with the battery above mentioned, becomes strongly magnetic.

51. I may perhaps be excused for mentioning in this communication that the induction at a distance affords the means of exhibiting some of the most astonishing experiments, in the line of *physique amusante*, to be found perhaps in the whole course of science. I will mention one which is somewhat connected with the experiments to be described in the next section, and which exhibits the action in a striking manner. This consists in causing the induction to take place through the partition wall of two rooms. For this purpose coil No. 1 is suspended against the wall in one room, while a person in the adjoining one receives the shock, by grasping the handles of the helix, and approaching it to the spot opposite to which the coil is suspended. The effect is as if by magic, without a visible cause. It is best produced through a door, or thin wooden partition.

52. The action at a distance affords a simple method of graduating the intensity of the shock in the case of its application to medical purposes. The helix may be suspended by a string passing over a pulley, and then gradually lowered down towards the plane of the coil, until the shocks are of the required intensity. At the request of a medical friend, I have lately administered the induced current precisely in this way, in a case of paralysis of a part of the nerves of the

53. I may also mention that the energetic action of the spiral conductors enables us to imitate, in a very striking manner, the inductive operation of the magneto-electrical machine, by means of an uninterrupted galvanic current. For this purpose it is only necessary to arrange two coils to represent the two poles of a horse-shoe magnet, and to cause two helices to revolve past them in a parallel plane. While a constant current is passing through each coil, in opposite directions, the effect of the rotation of the helices is precisely the same as that of the revolving armature in the machine.

54. A remarkable fact should here be noted in reference to helix No. 4, which is connected with a subsequent part of the investigation. This helix is formed of copper wire, the spires of which are insulated by a coating of cement instead of thread, as in the case of the others. After being used in the above experiments, a small discharge from a Leyden jar was passed through it, and on applying it again to the coil, I was much surprised to find that scarcely any signs of a secondary current could be obtained.

55. The discharge had destroyed the insulation in some part, but this was not sufficient to prevent the magnetizing of a bar of iron introduced into the opening at the centre. The effect appeared to be confined to the inductive action. The same accident had before happened to another coil of nearly the same kind. It was therefore noted as one of some importance. An explanation was afterwards found in a peculiar action of the secondary current.

SECTION IV.

On the effects produced by interposing different Substances between the Conductors.

56. Sir H. Davy found, in magnetizing needles by an electrical discharge, that the effect took place through interposed plates of all substances, conductors and non-conductors.* The experiment which I have given in paragraph 51 would appear to indicate that the inductive action which produces the secondary current might also follow the same law.

57. To test this the compound helix was placed about five inches above coil No. 1, fig. 6, and a plate of sheet iron, about $\frac{1}{10}$th of an inch thick, interposed. With this arrangement no shocks could be obtained; although, when the plate was withdrawn, they were very intense.

---

* Philosophical Transactions, 1821.

58. It was at first thought that this effect might be peculiar to the iron, on account of its temporary magnetism; but this idea was shown to be erroneous by substituting a plate of zinc of about the same size and thickness. With this the screening influence was exhibited as before.

59. After this a variety of substances was interposed in succession, namely, copper, lead, mercury, acid, water, wood, glass, &c.; and it was found that all the perfect conductors, such as the metals, produced the screening influence; but non-conductors, as glass, wood, &c., appeared to have no effect whatever.

60. When the helix was separated from the coil by a distance only equal to the thickness of the plate, a slight sensation could be perceived even when the zinc of $\frac{1}{10}$th of an inch in thickness was interposed. This effect was increased by increasing the quantity of the battery current. If the thickness of the plate was diminished, the induction through it became more intense. Thus a sheet of tinfoil interposed produced no perceptible influence; also four sheets of the same were attended with the same result. A certain thickness of metal is therefore required to produce the screening effect, and this thickness depends on the quantity of the current from the battery.

61. The idea occurred to me that the screening might, in some way, be connected with an instantaneous current in the plate, similar to that in the induction by magnetic rotation, discovered by M. Arago. The ingenious variation of this principle by Messrs. Babbage and Herschell, furnished me with a simple method of determining this point.

62. A circular plate of lead was interposed, which caused the induction in the helix almost entirely to disappear. A slip of the metal was then cut out in the direction of a radius of the circle, as is shown in fig. 7. With the plate in this condition, no screening was produced; the shocks were as intense as if the metal were not present.

63. This experiment however is not entirely satisfactory, since the action might have taken place through the opening of the lead; to obviate this objection, another plate was cut in the same manner, and the two interposed with a glass plate between them, and so arranged that the opening in the one might be covered by the continuous part of the other. Still shocks were obtained with undiminished intensity.

64. But the existence of a current in the interposed conductor was rendered certain by attaching the magnetizing spiral by means of two wires to the edge of the opening in the circular plate, as is shown in fig. 8. By this arrange-

ment the latent current was drawn out, and its direction obtained by the polarity of a needle placed in the spiral at *b*.

65. This current was a secondary one, and its direction, in conformity with the discovery of Dr. Faraday, was found to be the same as that of the primary current.

66. That the screening influence is in some way produced by the neutralizing action of the current thus obtained, will be clear, from the following experiment. The plate of zinc before mentioned, which is nearly twice the diameter of the helix, instead of being placed between the conductors, was put on the top of the helix, and in this position, although the neutralization was not as perfect as before, yet a great reduction was observed in the intensity of the shock.

67. But here a very interesting and puzzling question occurs. How does it happen that two currents, both in the same direction, can neutralize each other? I was at first disposed to consider the phenomenon as a case of real electrical interference, in which the impulses succeed each other by some regular interval. But if this were true the effect should depend on the length and other conditions of the current in the interposed conductor. In order to investigate this, several modifications of the experiments were instituted.

68. First a flat coil (No. 3) was interposed instead of the plates. When the two ends of this were separated, the shocks were received as if the coil were not present; but when the ends were joined, so as to form a perfect metallic circuit, no shocks could be obtained. The neutralization with the coil in this experiment was even more perfect than with the plate.

69. Again, coil No. 2, in the form of a ring, was placed not between the conductors, but around the helix. With this disposition of the apparatus, and the ends of the coil joined, the shocks were scarcely perceptible, but when the ends were separated, the presence of the coil has no effect.

70. Also when helix No. 1 and 2 were together submitted to the influence of coil No. 1, the ends of the one being joined, the other gave no shock.

71. The experiments were further varied by placing helix No. 2 within a hollow cylinder of sheet brass, and this again within coil No. 2 in a manner similar to that shown in fig. 13 which is intended to illustrate another experiment. In this arrangement the neutralizing action was exhibited, as in the case of the plate.

72. A hollow cylinder of iron was next substituted for the one of brass, and with this also no shocks could be obtained.

73. From these experiments it is evident that the neutralization takes place with currents in the interposed or adjoin-

ing conductors of all lengths and intensities, and therefore cannot, as it appears to me, be referred to the interference of two systems of vibrations.

74. This part of the investigation was, for a time, given up almost in despair, and it was not until new light had been obtained from another part of the inquiry, that any further advances could be made towards a solution of the mystery.

75. Before proceeding to the next Section, I may here state that the phenomenon mentioned, paragraph 54, in reference to helix No 4, is connected with the neutralizing action. The electrical discharge having destroyed the insulation at some point, a part of the spires would thus form a shut circuit, and the induction in this would counteract the action in the other part of the helix; or, in other words, the helix was in the same condition as the two helices mentioned in paragraph 70, when the ends of the wire of one were joined.

76. Also the same principle appears to have an important bearing on the improvement of the magneto-electrical machine: since the plates of metal which sometimes forms the ends of the spool containing the wire, must necessarily diminish the action, and also from experiment of paragraph 72 the armature itself may circulate a closed current which will interfere with the intensity of the induction in the surrounding wire. I am inclined to believe that the increased effect observed by Sturgeon and Bachhoffner, when a bundle of wire is substituted for a solid piece of iron, is at least in part due to the interruption of these currents. I hope to resume this part of the subject, in connexion with several other points, in another communication to the Society.

77. The results given in this Section may, at first sight, be thought at variance with the statements of Sir H. Davy, that needles could be magnetized by an electrical discharge with conductors interposed. But from his method of performing the experiment, it is evident that the plate of metal was placed between a straight conductor and the needle. The arrangement was therefore similar to the interrupted circuit in the experiment with the cut plate (62), which produces no screening effect. Had the plate been curved into the form of a hollow cylinder, with the two ends in contact, and the needle placed within this, the effect would have been otherwise.

SECTION V.

On the Production and Properties of induced Currents of the Third, Fourth, and Fifth order.

78. The fact of the perfect neutralization of the primary current by a secondary, in the interposed conductor, led me

to conclude that if the latter could be drawn out, or separated from the influence of the former, it would itself be capable of producing a new induced current in a third conductor.

79. The arrangement exhibited in fig. 9, furnishes a ready means of testing this. The primary current, as usual, is passed through coil No. 1, while coil No. 2, is placed over this to receive the induction, with its ends joined to those of coil No. 3. By this disposition the secondary current passes through No. 3; and since this is at a distance, and without the influence of the primary, its separate induction will be rendered manifest by the effects on helix No. 1. When the handles *a*, *b*, are grasped a powerful shock is received, proving the induction of a tertiary current.

80. By a similar but more extended arrangement, as shown in fig. 10, shocks were received from currents of a fourth and fifth order; and with a more powerful primary current, and additional coils, a still greater number of successive inductions might be obtained.

81. The induction of currents of different orders, of sufficient intensity to give shocks, could scarcely have been anticipated from our previous knowledge of the subject. The secondary current consists, as it were, of a single wave of the natural electricity of the wire, disturbed but for an instant by the induction of the primary; yet this has the power of inducing another current, but little inferior in energy to itself, and thus produces effects apparently much greater in proportion to the quantity of electricity in motion than the primary current.

82. Some difference may be conceived to exist in the action of the induced currents, and that from the battery, since they are apparently different in nature; the one consisting, as we may suppose, of a single impulse, and the other of a succession of such impulses, or a continuous action. It was therefore important to investigate the properties of these currents, and to compare the results with those before obtained.

83. First, in reference to the intensity, it was found that with the small battery a shock could be given from the current of the third order to twenty-five persons joining hands; also shocks perceptible in the arms were obtained from a current of the fifth order.

84. The action at a distance was also much greater than could have been anticipated. In one experiment shocks from the tertiary current were distinctly felt through the tongue, when helix No. 1, was at the distance of eighteen inches above the coil transmitting the secondary current.

85. The same screening effects were produced by the interposition of plates of metal between the conductors of the different orders, as those which have been described in reference to the primary and secondary currents.

86. Also when the long helix is placed over a secondary current generated in a short coil, and which is therefore, as we have before shown, one of quantity, a tertiary current of intensity is produced.

87. Again, when the intensity current of the last experiment is passed through a second helix, and another coil is placed over this, a quantity current is again produced. Therefore in the case of these currents, as in that of the primary, a *quantity current can be induced from one of intensity, and the converse.* By the arrangement of the apparatus as shown in fig. 10, these different results are exhibited at once. The induction from coil No. 3, to helix No. 1, produces an intensity current, and from the helix No. 2 and 4, a quantity current.

88. If the ends of coil No. 2, as in the arrangement of fig. 9, be united to helix No. 1, instead of coil No. 3, no shocks can be obtained; the quantity current of coil No. 2, appears not to be of sufficient intensity to pass through the wire of the long helix.

89. Also, no shocks can be obtained from the handles attached to helix No. 2, in the arrangement exhibited in fig. 11. In this case the quantity of electricity in the current from the helix appears to be too small to produce any effect, unless its power is multiplied by passing it through a conductor of many spires.

90. The next inquiry was in reference to the direction of these currents, and this appeared important in connexion with the nature of the action. The experiments of Dr. Faraday would render it probable, that at the beginning and ending of the secondary current, its induction on an adjacent wire is in contrary directions, as is shown to be the case in the primary current. But the whole action of a secondary current is so instantaneous, that the inductive effects at the beginning and ending cannot be distinguished from each other, and we can only observe a single impulse, which, however, may be considered as the difference of two impulses in opposite directions.

91. The first experiment happened to be made with a current of the fourth order. The magnetizing spiral (11) was attached to the ends of coil No. 4, fig. 10, and by the polarity of the needle it was found that this current was in

the same direction with the secondary and primary currents.[*] By a too hasty generalization, I was led to conclude, from this experiment, that the currents of all orders are in the same direction as that of the battery current, and I was the more confirmed in this from the results of my first experiments on the currents of ordinary electricity. The conclusion, however, caused me much useless labour and perplexity, and was afterwards proved to be erroneous.

92. By a careful repetition of the last experiment, in reference to each current, the important fact was discovered, that *there exists an alternation in the direction of the currents of the several orders, commencing with the secondary.* This result was so extraordinary, that it was thought necessary to establish it by a variety of experiments. For this purpose the direction was determined by decomposition, and also by the galvanometer, but the result was still the same; and at this stage of the inquiry I was compelled to the conclusion that the directions of the several currents were as follows:

Primary current, ...	+
Secondary current, ...	+
Current of the third order, . . .	—
Current of the fourth order, . . .	+
Current of the fifth order, . . .	—

93. In the first glance at the above table, we are struck with the fact that the law of alternation is complete, except between the primary and secondary currents, and it appeared that this exception might possibly be connected with the induced current which takes place in the first coil itself, and which gives rise to the phenomena of the spiral conductor. If this should be found to be *minus*, we might consider it as existing between the primary and secondary, and the anomaly would thus disappear. Arrangements were therefore made to fully satisfy myself on this point. For this purpose the decomposition of dilute acid and the use of the galvanometer were resorted to, by placing the apparatus between the ends of a cross wire attached to the extremities of the coil, as in the arrangement described by Dr. Faraday (ninth series); but all the results persisted in giving a direction to this current the same as stated by Dr. Faraday, namely, that of the

---

[*] It should be recollected that all the inductions which have been mentioned were produced at the moment of breaking the circuit of the battery current. The induction at the formation of the current is too feeble to produce the effects described.

primary current. I was therefore obliged to abandon the supposition that the anomaly in the change of the current is connected with the induction of the battery current on itself.*

94. Whatever may be the nature or causes of these changes in the direction, they offer a ready explanation of the neutralizing action of the plate interposed between two conductors, since a secondary current is induced in the plate; and although the action of this, as has been shown, is in the same direction as the current from the battery, yet it tends to induce a current in the adjacent conducting matter of a contrary direction. The same explanation is also applicable to all the other cases of neutralization, even to those which take place between the conductors of the several orders of currents.

95. The same principle explains some effects noted in reference to the induction of a current on itself. If a flat coil be connected with the battery, of course sparks will be produced by the induction, at each rupture of the circuit. But if in this condition another flat coil, with its ends joined, be placed on the first coil, the intensity of the shock is much diminished, and when the several spires of the two coils are mutually interposed by winding the two ribands together into one coil, the sparks entirely disappear in the coil transmitting the battery current, when the ends of the other are joined. To understand this, it is only necessary to mention that the induced current in the first coil is a true secondary current, and it is therefore neutralized by the action of the secondary in the adjoining conductor; since this tends to produce a current in the opposite direction.

96. It would also appear from the perfect neutralization which ensues in the arrangement of the last paragraph, that the induced current in the adjoining conductor is more powerful than that of the first conductor; and we can easily see how this may be. The two ends of the second coil are joined, and it thus forms a perfect metallic circuit; while the circuit of the other coil may be considered as partially interrupted, since to render the spark visible the electricity must be projected, as it were, through a small distance of air.

97. We would also infer that two contiguous secondary currents produced by the same induction, would partially counteract each other. Moving in the same direction, they would each tend to induce a current in the other of an opposite direction. This is illustrated by the following experiment: helix No. 1 and 2 were placed together, but not

---

* Our theory, as given in Vol. I. of these Annals, fully explains the whole phenomena. Edit.

united, above coil No. 1, so that they each might receive the induction; the larger was then gradually removed to a greater distance from the coil, until the intensity of the shock from each was about the same. When the ends of the two were united, so that the shock would pass through the body from the two together, the effect was apparently less than with one helix alone. The result, however, was not as satisfactory as in the case of the other experiments; a slight difference in the intensity of two shocks could not be appreciated with perfect certainty.

SECTION VI.

The production of induced Currents of the different Orders from ordinary Electricity.

98. Dr. Faraday, in the ninth series of his researches, remarks that " the effect produced at the commencement and the end of a current (which are separated by an interval of time when that current is supplied from a voltaic apparatus) must occur at the same moment when a common electrical discharge is passed through a long wire. Whether if it happen accurately at the same moment they would entirely neutralize each other, or whether they would not still give some definite peculiarity to the discharge, is a matter remaining to be examined."

99. The discovery of the fact that the secondary current, which exists but for a moment, could induce another current of considerable energy, gave some indication that similar effects might be produced by a discharge of ordinary electricity, provided a sufficiently perfect insulation could be obtained.

100. To test this a hollow glass cylinder, fig. 12, of about six inches in diameter, was prepared with a narrow riband of tinfoil, about thirty feet long, pasted spirally around the outside, and a similar riband of the same length, pasted on the inside; so that the corresponding spires of the two were directly opposite each other. The ends of the inner spiral passed out of the cylinder through a glass tube, to prevent all direct communication between the two. When the ends of the inner riband were joined by the magnetizing spiral (11), containing a needle, and a discharge from a half gallon jar sent through the outer riband, the needle was strongly magnetized in such a manner as to indicate *an induced current through the inner riband in the same direction as that of the current of the jar.* This experiment was repeated many times, and always with the same result.

101. When the ends of one of the ribands were placed very nearly in contact, a small spark was perceived at the opening, the moment the discharge took place through the other riband.

102. When the ends of the same riband were separated to a considerable distance, a larger spark than the last could be drawn from each end by presenting a ball or the knuckle.

103. Also if the ends of the outer riband were united, so as to form a perfect metallic circuit, a spark could be drawn from any point of the same, when a discharge was sent through the inner riband.

104. The sparks in the two last experiments are evidently due to the action known in ordinary electricity by the name of the lateral discharge. To render this clear, it is perhaps necessary to recall the well known fact, that when the knob of a jar is electrified positively, and the outer coating in connexion with the earth, then the jar contains a small excess of positive electricity beyond what is necessary to perfectly neutralize the negative surface. If the knob be put in communication with the earth, the extra quantity, or the free electricity, as it is sometimes called, will be on the negative side. When the discharge took place in the above experiments, the inner riband became for an instant charged with this free electricity, and consequently threw off from the outer riband, by ordinary induction, the sparks described. It therefore became a question of importance to determine, whether the induced current described in paragraph 100 was not also a result of the lateral discharge, instead of being a true case of a secondary current analogous to those produced from galvanism. For this purpose the jar was charged, first with the outer coating in connexion with the earth, and again with the knob in connexion with the same, so that the extra quantity might be in the one case *plus* and in the other *minus*; but the direction of the induced current was not affected by these changes; it was always the same, namely, from the positive to the negative side of the jar.

105. When, however, the quantity of free electricity was increased, by connecting the knob of the jar with a globe about a foot in diameter, the intensity of magnetism appeared to be somewhat diminished, if the extra quantity was on the negative side; and this might be expected, since the free electricity, in its escape to the earth through the riband, in this case would tend to induce a feeble current in the opposite direction to that of the jar.

106. The spark from an insulated conductor may be considered as consisting almost entirely of this free or extra

electricity, and it was found that this was also capable of producing an induced current, precisely the same as that from the jar. In the experiment which gave this result, one end of the outer riband of the cylinder (100) was connected with the earth, and the other caused to receive a spark from a conductor fourteen feet long, and nearly a foot in diameter. The direction of the induced current was the same as that of the spark from the conductor.

107. From these experiments it appears evident that the discharge from the Leyden jar possesses the property of inducing a secondary current precisely the same as the galvanic apparatus, and also that this induction is only so far connected with the phenomenon of the lateral discharge as this latter partakes of the nature of an ordinary electrical current.

108. Experiments were next made in reference to the production of currents of the different orders by ordinary electricity. For this purpose a second cylinder was prepared with ribands of tinfoil, in a similar manner to the one before described. The two were then so connected that the secondary current from the first would circulate around the second. When a discharge was passed through the outer riband of the first cylinder, a tertiary current was induced in the inner riband of the second. This was rendered manifest by the magnetizing of a needle in a spiral joining the ends of the last mentioned riband.

109. Also by the addition, in the same way, of a third cylinder, a current of the fourth order was developed. The same result was likewise obtained by using the arrangement of the coils and helices shown in fig. 10. For these experiments, however, the coils were furnished with a double coating of silk, and the contiguous conductors separated by a large plate of glass.

110. Screening effects precisely the same as those exhibited in the action of galvanism were produced by interposing a plate of metal between the conductors of different orders, figs. 9 and 10. The precaution was taken to place the plate between two frames of glass, in order to be assured that the effect was not due to a want of perfect insulation.

111. Also analogous results were found when the experiments were made with coils interposed instead of plates, as described in paragraph 68. When the ends of the interposed coils were separated, no screening was observed, but when joined, the effect was produced. The existence of the induced current, in all these experiments, was determined by the magnetism of a needle in a spiral attached to one of the coils.

112. Likewise shocks were obtained from the secondary current by an arrangement shown in fig. 13. Helices No. 2 and No. 3 united are put within a glass jar, and coil No. 2 is placed around the same. When the handles are grasped, a shock is felt at the moment of the discharge, through the outer coil. The shocks, however, were very different in intensity with different discharges from the jar. In some cases no shock was received, when again with a less charge, a severe one was obtained. But there irregularities find an explanation in a subsequent part of the investigation.

113. In all these experiments, the results with ordinary and galvanic electricity are similar. But at this stage of the investigation there appeared what at first was considered a remarkable difference in the action of the two. I allude to the direction of the currents of the different orders. These, in the experiments with the glass cylinders, instead of exhibiting the alternations of the galvanic currents (92), were all in the same direction as the discharge from the jar, or, in other words, they were all *plus*.

114. To discover, if possible, the cause of this difference, a series of experiments was instituted; but the first fact developed, instead of affording any new light, seemed to render the obscurity more profound. When the directions of the currents were taken in the arrangement of the coils (fig. 10) the discrepancy vanished. *Alternations were found the same as in the case of galvanism.* This result was so extraordinary that the experiments were many times repeated, first with the glass cylinders, and then with the coils; the results, however, were always the same. The cylinders gave currents all in one direction; the coils in alternate directions.

115. After various hypotheses had been formed, and in succession disproved by experiment, the idea occurred to me that the direction of the currents might depend on the distance of the conductors, and this appeared to be the only difference existing in the arrangement of the experiments with the coils and the cylinders.* In the former the distance between the ribands was nearly one inch and a half, while in the latter it was only the thickness of the glass, or about $\frac{1}{20}$th of an inch.

116. In order to test this idea, two narrow slips of tinfoil, about twelve feet long, were stretched parallel to each other, and separated by thin plates of mica to the distance of about

---

* This idea was not immediately adopted, because I had previously experimented on the direction of the secondary current from galvanism, and found no change in reference to distance.

$\frac{1}{50}$th of an inch. When a discharge from the half gallon jar was passed through one of these, an induced current in the same direction was obtained from the other. The ribands were then separated, by plates of glass, to the distance of $\frac{1}{20}$th of an inch; the current was still in the same direction, or *plus.* When the distance was increased to about $\frac{1}{6}$th of an inch, no induced current could be obtained; and when they were still further separated the current again appeared, but was now *found to have a different direction, or to be minus.* No other change was observed in the direction of the current; the intensity of the induction decreased as the ribands were separated. The existence and direction of the current, in this experiment, were determined by the polarity of the needle in the spiral attached to the ends of one of the ribands.

117. The question at this time arose, whether the direction of the current, as indicated by the polarity of the needle, was the true one, since the magnetizing spiral might itself, in some cases, induce an opposite current. To satisfy myself on this point a series of charges, of various intensity and quantity, from a single spark of the large conductor to the full charge of nine jars, were passed through the small spiral, which had been used in all the experiments, but they all gave the same polarity. The interior of this spiral is so small, that the needle is throughout in contact with the wire.

118. The fact of a change in the direction of the induced current by a change in the distance of the conductors, being thus established, a great number and variety of experiments were made to determine the other conditions on which the change depends. These were sought for in a variation of the intensity and quantity of the primary discharge, in the length and thickness of the wire, and in the form of the circuit. The results were, however, in many cases, anomalous, and are not sufficiently definite to be placed in detail before the Society. I hope to resume the investigation at another time, and will therefore at present briefly state only those general facts which appear well established.

119. With a single half gallon jar, and the conductors separated to a distance less than $\frac{1}{20}$th of an inch, the induced current is always in the same direction as the primary. But when the conductors are gradually separated, there is always found a distance at which the current begins to change its direction. This distance depends certainly on the amount of the discharge, and probably on the intensity; and also on the length and thickness of the conductors. With a battery of eight half gallon jars, and parallel wires of about ten feet

long, the change in the direction did not take place at a less distance than from twelve to fifteen inches, and with a still larger battery and longer conductors, no change was found, although the induction was produced at the distance of several feet.

120. The facts given in the last paragraph, relate to the inductive action of the primary current; but it appears from the results detailed in paragraphs 110 and 114, that the currents of all the other orders also change the direction of the inductive influence with a change of the distance. In these cases however, the change always takes place at a very small distance from the conducting wire; and in this respect the result is similar to the effect of a *primary current* from the discharge of a small jar.

121. The most important experiments, in reference to distance, were made in the lecture room of my respected friend, Dr. Hare, of Philadelphia, with the splendid electrical apparatus described in the Fifth volume (new series) of the Transactions of this Society. The battery consists of thirty-two jars, each of the capacity of a gallon. A thick copper wire of about $\frac{1}{10}$th of an inch in diameter and eighty feet in length, was stretched across the lecture room, and its ends brought to the battery, so as to form a trapezium, the longer side of which was about thirty-five feet. Along this side a wire was stretched of the ordinary bell size, and the extreme ends of this joined by a spiral, similar to the arrangement shown in fig. 14. The two wires were at first placed within the distance of about an inch, and afterwards constantly separated after each discharge of the whole battery through the thick wire. When a break was made in the second wire at *a*, no magnetism was developed in a needle in the spiral at *b*, but when the circuit was complete, the needle at each discharge indicated a current in the same direction as that of the battery. When the distance of the two wires was increased to sixteen inches, and the ends of the second wire placed in two glasses of mercury, and a finger of each hand plunged into the metal, a shock was received. The direction of the current was still the same, but the magnetism not as strong as at a less distance.

122. The second wire was next arranged around the other, so as to enclose it. The magnetism by this arrangement appeared stronger than with the last; the direction of the current was still the same, and continued thus, until the two wires were at every point separated to the distance of twelve feet, except in one place where they were obliged to be crossed at the distance of seven feet, but here the wires were

made to form a right angle with each other, and the effect of the approximation was therefore (46) considered as nothing. The needle at this surprising distance was tolerably strongly magnetized, as was shown by the quantity of filings which would adhere to it. The direction of the current was still the same as that of the battery. The form of the room did not permit the two wires to be separated to a greater distance. The whole length of the circuit of the interior large wire was about eighty feet; that of the exterior one hundred and twenty. The two were not in the same plane, and a part of the outer passed through a small adjoining room.

123. The results exhibited in this experiment are such as could scarcely have been anticipated by our previous knowledge of the electrical discharge. They evince a remarkable inductive energy, which has not before been distinctly recognized, but which must perform an important part in the discharge of electricity from the clouds. Some effects which have been observed during thunder storms, appear to be due to an action of this kind.

124. Since a discharge of ordinary electricity produces a secondary current in an adjoining wire, it should also produce an analagous effect in its own wire; and to this cause may be now referred the peculiar action of a long conductor. It is well known that the spark from a very long wire, although quite short, is remarkably pungent. I was so fortunate as to witness a very interesting exhibition of this action during some experiments on atmospheric electricity made by a committee of the Franklin Institute, in 1836. Two kites were attached one above the other, and raised with a small iron wire in place of a string. On the occasion at which I was present, the wire was extended by the kites to the length of about one mile. The day was perfectly clear, yet the sparks from the wire had so much projectile force (to use a convenient expression of Dr. Hare) that fifteen persons joining hands and standing on the ground, received the shock at once, when the first person of the series touched the wire. A Leyden jar being grasped in the hand by the outer coating, and the knob presented to the wire, a severe shock was received, as if by a perforation of the glass, but which was found to be the result of the sudden and intense induction.

125. These effects were evidently not due to the accumulated intensity at the extremities of the wire, on the principles of ordinary electrical distribution, since the knuckle required to be brought within about a quarter of an inch before the spark could be received. It was not alone the quantity, since the experiments of Wilson prove that the same effect is not produced with an equal amount of electricity on the surface

of a large conductor. It appears evidently therefore a case of the induction of an electrical current on itself. The wire is charged with a considerable quantity of feeble electricity, which passes off in the form of a current along its whole length, and thus the induction takes place at the end of the discharge, as in the case of a long wire transmitting a current of galvanism.*

126. It is well known that the discharge from an electrical battery possesses great divellent powers; that it entirely separates, in many instances, the particles of the body through which it passes. This force acts, in part, at least, in the direction of the line of the discharge, and appears to be analogous to the repulsive action discovered by Ampère, in the consecutive parts of the same galvanic current. To illustrate this, paste on a piece of glass a narrow slip of tinfoil, cut it through at several points, and loosen the ends from the glass at the places so cut. Pass a discharge through the tinfoil from about nine half gallon jars; the ends, at each separation, will be thrown up, and sometimes bent entirely back, as if by the action of a strong repulsive force between them. This will be understood by a reference to fig. 14; the ends are shown bent back at *a*, *a*, *a*, *a*. In the popular experiment of the pierced card, the bur on each side appears to be due to an action of the same kind.*

127. It now appears probable, from the facts given in paragraphs 119 and 120, that the table in paragraph 92 is only an approximation to the truth, and that each current from galvanism, as well as from electricity, first produces an inductive action in the direction of itself, and that the inverse influence takes place at a little distance from the wire.

128. To test this the compound helix was placed on coil No. 1, to receive the induction, and its ends joined to those of the outer riband of tinfoil of the glass cylinder, while the magnetizing spiral was attached to the ends of the inner riband. A feeble tertiary current was produced by this arrangement, which in two cases gave a polarity to the needle indicating a direction the same as that of the primary current. In other cases the magnetism was either imperceptible or *minus*. With an arrangement of two coils of wires around two glass cylinders, one within the other, the same effect was produced. The magnetism was less when the distance of the two sets of spires was smaller, indicating, as it would

---

* We have witnessed this fact many years since in our strips of tin-foil for *protecting* jars; Vol. II. p. 86. Even when these strips are laid *over* the top of the lining, more than an inch, they are sometimes blown off, and much perforated. Edit.

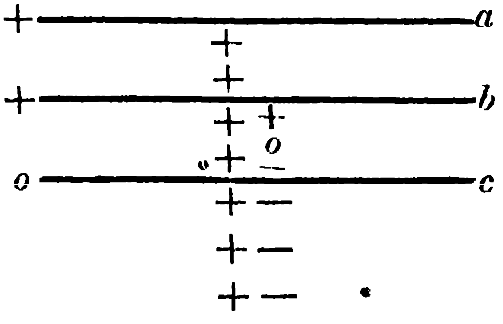
appear, an approximation to a position of neutrality. These results are rather of a negative kind, yet they appear to indicate the same change with distance in the case of the galvanic currents, as in that of the discharge of ordinary electricity. The distance however at which the change takes place would seem to be less in the former than in the latter.

129. There is a perfect analogy between the inductive action of the primary current from the galvanic apparatus and of that from the larger electrical battery. The point of change, in each, appears to be at a great distance.

130. The neutralizing effect described in Section IV. may now be more definitely explained by saying that when a third conductor is acted on at the same time by a primary and secondary current (unless it be very near the second wire), it will fall into the region of the *plus* influence of the former, and into that of the *minus* influence of the latter; and hence no induction will be produced.

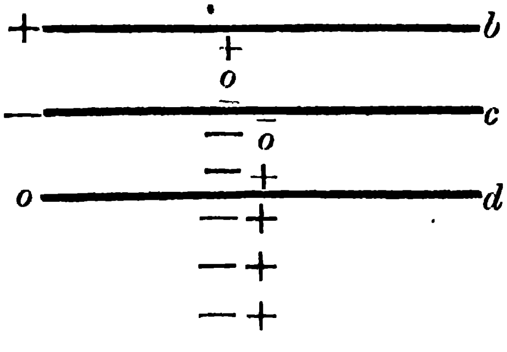
131. This will be rendered perfectly clear by fig. A, in which *a* represents the conductor of the primary current, *b* that of the secondary, and *c* the third conductor. The characters + + +, &c. beginning at the middle of the first conductor and extending downwards, represent the constant *plus* influence of the primary current, and those + 0 — —, &c., beginning at the second conductor, indicate its inductive influence as changing with the distance. The third conductor, as is shown by the figure, falls in the *plus* region of the primary current, and in the *minus* region of the secondary, and hence the two actions neutralize each other, and no apparent result is produced.

Fig. A



132. Fig. B indicates the method in which the neutralizing effect is produced in the case of the secondary and tertiary currents. The wire conducting the secondary current is represented by *b*, that conducting the tertiary by *c*, and the other wire, to receive the induction from these, by *d*. The direction of the influence, as before, is indicated by + 0 — —,

Fig. B



&c., and the third wire is again seen to be in the *plus* region of the one current, and in the *minus* of the other. If, however, *d* is placed sufficiently near *c*, then neutralization will not take place, but the two currents will conspire to produce in it an induction in the same direction. A similar effect would also be produced were the wire *c*, in fig. A, placed sufficiently near the conductor *b*.

133. Currents of the several orders were likewise produced from the excitation of the magneto-electrical machine. The same neutralizing effects were observed between these as in the case of the currents from the galvanic battery, and hence we may infer that also the same alternations take place in the direction of the several currents.

134. In conclusion, I may perhaps be allowed to state, that the facts here presented have been deduced from a laborious series of experiments, and are considered as forming some addition to our knowledge of electricity, independently of any theoretical considerations. They appear to be intimately connected with various phenomena, which have been known for some years, but which have not been referred to any general law of action. Of this class are the discoveries of Savary, on the alternate magnetism of steel needles, placed at different distances from the line of a discharge of ordinary electricity,* and also the magnetic, screening influence of all metals, discovered by Dr. Snow Harris, of Plymouth.† A comparative study of the phenomena observed by these distinguished *savants*, and those given in this paper, would probably lead to some new and important developments. Indeed, every part of the subject of electro-dynamic induction appears to open a field for discovery, which experimental industry cannot fail to cultivate with immediate success.

NOTE.

[illegible] were presented to the Society, my friend, Dr. Bache of the Girard College, gave an account of the investigations of Professor Ettingshausen, of Vienna, in reference to the improvement of the magneto-electric machine, some of the results of which he had witnessed at the University of Vienna about a year since. No published account of these experiments has yet reached this country, but it appears that Professor Ettingshausen had been led to suspect the develop-

---

* Annales de Chimie et de Physique, 1827.

† Philosophical Transactions, 1831.

ment of a current in the metal of the keeper of the magneto-electric machine, which diminished the effect of the current in the coil about the keeper, and hence to separate the coil from the keeper by a ring of wood of some thickness, and afterwards, to prevent entirely the circulation of currents in the keeper, by dividing it into segments, and separating them by a non-conducting material. I am not aware of the result of this last device, nor whether the mechanical difficulties in its execution were fully overcome. It gives me pleasure to learn that the improvements, which I have merely suggested as deductions from the principles of the interference of induced currents (76), should be in accordance with the experimental conclusions of the above named philosopher.

XXXIX. *On Lightning Conductors, and on certain Principles in Electrical Science; being an investigation of Mr. Sturgeon's Experimental and Theoretical Researches in Electricity, published by him in the "Annals of Electricity," &c.* *By* W. Snow Harris, *Esq., F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In the Annals of Electricity for October last will be found a memoir on Marine Lightning conductors. This memoir is addressed to the British Association, and is considered by Mr. Sturgeon, the author of it, to merit in a high degree the especial consideration of all the learned scientific bodies in Europe and America.

The author endeavours to show, that a metallic rod whilst transmitting a charge of electricity, is always productive of powerful lateral explosions, not only on near bodies, but on bodies at very great distances. This effect, he thinks, in the case of a lightning rod, is a very fearful circumstance.

2. If this deduction be worth anything, it is altogether subversive of the use of such rods as a means of protection from lightning. I have thought it right, therefore, to examine carefully the experiments and reasonings, which have led the author to this conclusion; and since the inquiry bears materially on a question of great public interest, and contains many new phenomena of electrical action, I hope it may not be considered unworthy a place in your very valuable Journal.

3. Although Mr. Sturgeon has spoken in a slighting way of me and my experiments, and has laboured hard to invalidate them, I still feel, that any personal consideration is comparatively of minor consequence. I will not, therefore, trouble your readers on the subject. I merely wish to have it understood, that this is not a reply to that large part of the memoir levelled at myself, but is simply an investigation of the author's "Theoretical and Experimental Researches," and of his claims to our confidence as a writer on Electrical Science.

4. So long since as the years 1728 and 1729, Mr. Grey observed the phenomena of electrical conduction and insulation.

(*a*). Thus a metallic ball, J, fig. 1, Plate VII. supported on the glass rod *g*, is said to be insulated, and if electrified, will cause a spark in the opening between the metallic body B and the ball J.

(*b*). If we connect the ball J with any distant body *c*, by means of a metallic wire as in fig. 2, and electrify it as before, the spark will still occur in the opening at the distant body *c*, the electricity being conducted by the intermediate wire.

(*c*). The distance at which this effect may ensue, is very considerable. Mr. Grey succeeded in making it sensible at a distance of 765 feet.*

(*d*). The effect is more sensible when the body B is connected with the ground, which places it, by a law of electrical action, in the most favourable state for receiving the spark.

5. I am desirous to call especial attention to these results, notwithstanding their elementary character, because, as we shall presently see, they are really nothing more or less than the essence of Mr. Sturgeon's *new researches*, and which he claims to have considered by all the learned societies of Europe and America.

6. When we attempt to charge an electrical jar, J. fig. 3, it is observable, that as the charge accumulates on the inner surface, a corresponding quantity of electricity is forced off from the outer, and without this double effect takes place we fail to accumulate a charge.

(*e*). To render this evident, we have only to place the jar on an insulator, as in fig. 3; we shall then find, that for every spark we send into the jar, a similar spark will leave its outside, either from the coating directly, or from any distant body *c* connected with it as in fig. 4.

---

* Priestley's History of Electricity.

The outer coating J, therefore, and distant body *c*, may be considered in their insulated state as being insulated conductors under the conditions represented in fig. 2.

(f). Suppose the jar charged, and that it remains insulated; then we may discharge it, either by one dense shock through the rod *t*, fig. 4, or gradually, in the reverse way of charging; viz. by continuing to draw sparks from the knob *m*, and add them to the coating J: the circumstance however of our being enabled to take a finite spark, from either side alternately, whilst the jar rests on an insulator, is sufficient to show, that the accumulated electricity is never exactly balanced between the opposed coatings, so that there will always be an excess of either positive or negative electricity over the neutralizing quantities themselves, disposed on the coatings of the jar.

(*g*). When therefore we discharge the jar, this excess of free electricity will speedily expand itself over the outer surface J, the discharging rod *t*, the knob of the jar *m*, or any other body, *c*, fig. 4, connected with it, which, as in the case of the simply electrified conductor, J, fig. 2, will cause a spark to occur in either of those places. The intensity of this spark however will depend on the capacity of the jar. It is *less* with a large jar, and *greater* with a small one, the quantity of electricity discharged being the same.

(*h*). When the jar has been discharged, the knob, the outer coating, and all the bodies connected with it, will be found in the same electrical state. We may make this state either positive or negative, by taking a spark either from the knob or coating previously to discharging the jar.

(*i*). This small spark caused by the excess of free electricity, may be obtained even though the jar be connected with the earth, provided we seize it before the conductors have had time to operate in carrying off the residuary accumulation; Professor Wheatstone having shown by his unrivalled experiments on electrical conduction, that some portion of time elapses in the passage of electricity through wires.

By bringing a metallic ball, B, fig. 3 and 4, therefore in a free state, either very near the discharging rod *c c*, fig. 3, the outer coating J, or any body, *c*, fig. 4 , in connexion with it, previously to making the discharge, we seize as it were some of the residuary electricity before it has time to pass off, and hence it becomes evident in this particular direction. The effect, however, will be necessarily greatest when the jar and its appendages are quite insulated. After this spark has taken place, the jar will be found again slightly charged, with what has been called a residuary charge, so that the phenomenon itself is actually the *same as that already* observed in charging the jar originally (*e*).

7. Now these simple experiments (*g*), (*h*), (*i*), are just the experiments described by Mr. Sturgeon, in which he imagines that the small spark above described, is produced by a lateral action of the rod carrying off the discharge. He seems to consider it as a novel and important fact, and calls upon the "principal scientific bodies in Europe and America," and "the ablest electricians the world can produce," in order that it may be fully sifted and explained. He takes great credit for having placed this subject before them in a "*proper* light," and cannot account for the circumstance of my having overlooked it.*

8. But since it is clear that this supposed lateral explosion really resolves itself into one or two simple facts (*a*) (*b*), known to electricians for more than a century since, "the ablest electricians the world can produce," may, perhaps, be disposed to think such an occupation of their time unnecessary, and the several "Learned Societies in Europe and America" may consider it would have been quite as well for Mr. Sturgeon's credit, as a lecturer on natural philosophy, if he had not troubled them on the occasion.

9. The following is Mr. Sturgeon's version of these experiments :

This kind of lateral discharge, "consists in the displacement of the electrical fluid of bodies vicinal to a continuous conductor carrying the primitive discharge."

Exp.—If a Leyden jar, J, fig 2, be discharged through a rod *c c*, a spark will appear at the opening *o*, between the metallic body B placed near the rod.

Exp.—If instead of discharging the jar through the rod *c c*, fig. 4, we discharge it by a common discharging rod *t*, still the spark will appear at *o*, as before.

"The effect," he says, "is much increased by connecting the body B with the ground, and diminished to a certain extent by connecting the outside of the jar with the ground." I have produced the spark, he says, between *c c*, and the body B when placed at 50 feet from the *direct* discharge.

"By this kind of lateral discharge," he observes, "a dense spark may be produced when the bodies B and *c c*, fig. 3, are half an inch apart. Though the jar be only of the capacity of a quart, chemical decompositions may be effected by it."

---

* "I mean to submit the substance of my Memoir to the consideration of the principal scientific bodies in Europe and America, in order that the subject may be fully sifted and explained by the ablest electricians the world can produce."—*Ann. of Elect.*, p. 191.

10. Mr. Sturgeon does not state precisely how these experiments were conducted, but the nature of the manipulations would have a material effect on the result. If for example a small jar of a quart capacity were charging from a very powerful machine, and the discharge produced at the time of charging, either by a spontaneous explosion between the balls, *m c*, fig. 3, or by an insulated discharger, then, as is evident, not only would the outer coating and its appendages become charged with the residuary electricity proper to the jar, but also by electricity from the prime conductor, which would assuredly pass over at the instant of the discharge. In Mr. Sturgeon's account of his experiments this fallacious method would appear to have been resorted to. He says, " a spark is felt at every discharge through the circuit represented in the figure," that is *m c c*, fig. 3. Now the continued discharges implied in this statement, could only be produced by continuing to work the machine in connexion with the jar. This circumstance alone would be sufficient to falsify the whole.

21. The following experiments are not unimportant as bearing on the present question.

(*k*). Let a jar. J, fig. 3, be charged positively, removed from the machine, and insulated.—Under this condition discharge it. When discharged, let the electrical state of the knob *m*, discharging conductor *c c*, the outer coating J, or any distant body *c c*, fig. 4, connected with it, be examined; they will all be found in the same electrical state, which state will be precisely that, exhibited by the outer coating and knob, whilst charging, and the small residuary spark will be plus.

(*l*). Charge the jar as before; but before discharging it, withdraw the free electricity from the knob. The electrical state of the coating and appendages will be now changed, and the small residuary spark will be minus.

(*m*). Immediately *after* the discharge, apply a metallic body B, fig. 3 and 4, either to the coating J, or any body connected with it. A residuary spark will be thrown off.

(*n*). Place a metallic body B near the discharger, or outer coating, previously to making the discharge; the spark will then appear to ensue at the time of the discharge.

(*o*). Examine the jar after this residuary spark has been taken from the outer coating, and it will be found again slightly charged as at first,

(*p*). Charge a jar, exposing about two square feet of coating, with a given quantity of electricity, measured by the unit jar *u*, fig. 5. Let a conducting rod terminating in a ball *r*, project from the outer coating, and place near it the electroscope E.* Discharge the jar through the rod *c c*, as before, and observe the amount of divergence of the electroscope. Double the capacity of the jar, and again accumulate and discharge the same quantity. The divergence of the electroscope will be very considerably decreased. Add a second and a third jar to the former, and the effect will be at last scarcely perceptible: connect the jar with the ground, and with a given quantity the spark will vanish altogether.

(*q*). Accumulate a given quantity as before, and observe the effect of the residuary charge on the electroscope. Let a double, treble, &c., quantity be accumulated and discharged from a double, treble, &c., extent of surface; that is to say, for a double quantity employ two similar jars, and so on: the effect will remain the same.

(*r*). The quantity and surface remaining constant, let the discharge be effected by discharging circuits *c c*, fig 3, of different dimensions from a large rod down to a fine wire which the charge in passing can make red-hot. Observe the effect on the electroscope in each case: it will be found nearly the same, being rather less where the tension in the discharging wire is very considerable.

(*s*). Connect the jar with the ground, and place between the discharging conductor *c* fig. 3, and a metallic mass B, a small quantity of percussion powder, inclosed in thin paper. The powder will not be inflamed, even in the case of the discharging conductor becoming red-hot: whereas in passing the slightest spark, it inflames directly.

(*t*). Insulate a circular conducting disc, M, fig. 6, of four feet in diameter: it may be made of wood covered with tin foil; oppose to it a similar disc, N, connected with the ground. Place a conducting rod, *c c*, on the lower plate, and near it a metallic body, *o*; electrify the upper plate, *m*; dense sparks will fall on the rod, *c c*, but no effect is observable on the vicinal body, *o*, even though percussion powder be placed in the opening.

12. These experiments are conclusive of the nature of Mr. Sturgeon's experiments.

Exp. (*k*). (*l*).—show, that the electricity of the spark varies with that of the coatings.

Exp. (*m*).—proves that the spark is readily obtained *after* the discharge has taken place; it is not therefore any lateral explosion caused by the discharging rod.

---

* The electroscope I employed is described in the Transactions of the Royal Society for 1834, Part 2, page 214. For more accurate measurement we should employ the electrometer. p. 215.

Exp. (*o*).—proves that the spark is merely a residual accumulation.

Exp. (*p*). (*q*).—prove that the spark is of different degrees of force, when the electricity is discharged from a greater or less extent of surface, whilst double, treble, &c., quantities, when discharged from double, treble, &c., surfaces, give the same spark. Now as no one can doubt but that the effect of a double, &c. quantity should be greater than a single, &c. quantity, it is again evident that the spark is not caused by any lateral explosion from the discharging rod; it being a well-established law, that the same quantity has the same heating effect on wires, whether discharged from a great surface or a small one, from thick glass or thin; some little allowance being made for the greater number of rods, &c., when the surface is increased by an additional number of jars.* The effect therefore depending on the jar, Mr. Sturgeon had a greater chance with a small jar than with a large one.

Exp. (*r*).—proves that the degree of tension in the rod is not of any consequence.

Exp. (*s*). (*t*).—show, that no kind of lateral action arises during the passage of the charge.

13. Mr. Sturgeon confounds this residuary spark, with the Earl of Stanhope's experiments on induction: he observes, p. 176, "Viscount Mahon studied this kind of lateral discharge very extensively." But any one who considers His Lordship's work, will soon detect the fallacy of such a conclusion. Lord Mahon shows, that when an electrical charge is about to pass from a body M, fig 7, in the direction C L, the action upon a near body N will displace some of its electricity; hence a spark will take place at E between that body and another connected with the ground whenever the discharge takes place from M, in consequence of the return of the displaced electricity. This effect His Lordship termed the "returning stroke." Now to apply this to the operation of a thunder cloud. Let M, fig. 6, represent a mass of cloud covering a portion of the earth's surface N. Let *c c* be a discharging rod, and *o* some near body. Then by Lord Stanhope's experiment the charged cloud M will displace from the surface N, and all the bodies on it as *c c*, *o*, &c. a portion of their natural electricity, which will again return when the discharge has been effected. The conditions of Lord Mahon's experiment cannot obtain between the conductor *c c* and the

---

* Philosophical Transactions for 1834. *Part* II. p. 225, and Faraday's Researches.

body *o*, since they are both in the same forced state.* It is very easy to perceive, that the electrical relations of two bodies *o* and *a between* the boards, is different from that between a conductor J, fig. 1, charged with electricity, and a body B in its natural state; or that of a conductor C, fig. 6, carrying off the displaced electricity of the lower plate N, and a body B. neutral. Besides, in Lord Mahon's experiment, fig. 7, the electricity of the return spark is different from that of the primitive charge in M; whereas, in Mr. Sturgeon's experiment, the spark is of the same kind. So little did His Lordship anticipate any objection to the use of lightning rods in consequence of his experiments, that he declares his conviction of their passive operation, and reproves those who "ignorantly conclude" that they are of a dangerous nature.

14. We have been here discussing what the author calls a *third* kind of lateral discharge; but he mentions a *first* and *second* kind also. The first kind, he says, "takes place at every interruption of a metallic circuit;" "it displaces loose bodies," &c. This is evidently the effect of mechanical expansion, and is the very effect we avoid by means of a lightning rod. He alludes to Dr. Priestley as authority on this point; how unfortunate for his whole doctrine! Let us consider for a moment what Dr. Priestley says: "That the cause of this dispersion of bodies in the neighbourhood of electrical explosions is *not their being suddenly charged with electric matter*, is, I think, evident. I never observed the *least attraction of these bodies toward the brass rods, through which the explosion passed*, although I used several methods which could not fail to show it. I even found that the explosion of a battery made ever so near a brass rod, did not so much as disturb its electric fluid; for when I had insulated the rod, and hung a pair of pith balls on the end opposite to that near which the explosion passed, I found the balls were not in the least moved.†

---

* This applies to Mr. Sturgeon's Exp. (9).—If B fig. *c*, 3, were on the same insulation with the jar J and rod *c*, *no spark could occur at o*, except by a division of the charge, whatever quantity passed through *c*. This fact alone is conclusive of the point in question, proving clearly that the spark is *not* a lateral explosion.

† The reader will distinguish here between this experiment and Lord Mahon's. The latter relates to the influence of a permanently charged conductor on a body neutral; whereas Priestley's applies to the action of wires carrying vanishing quantities of electricity, the very essence of Mr. Sturgeon's experiment. Dr. Priestley would not have told us, had he brought his rod near the *free side* of his battery, that then the pith balls were not moved.

We have seen how little support Mr. Sturgeon derived from Lord Mahon; he obtains still less from Priestley, who, without any compromise, sweeps away his whole theory. Lord Stanhope and Dr. Priestley, eminent amongst the philosophers of their day, will be doubtless admitted to be as good authority as Mr. Sturgeon.

15. The *second* kind of lateral discharge is, we are informed, "a radiation of electric matter from conductors carrying the primitive discharge." It takes place, the author says, from edges, and that hence "sharp edges of metal carrying a flash of lightning would discharge necessarily a great quantity of fluid into neighbouring bodies." No author is pressed into the service on this occasion, and for the best possible reason, no accredited writer has ever treated of such a phenomenon as applying to a lightning rod. It is in fact applicable only to charged conductors. Thus ragged or pointed rods attached to the prime conductor of the electrical machine exhibit brushes of light, whilst other similar bodies, within their influence, have the appearance of stars. The lights on steeples, and on the sail yard and masts of ships, mentioned by Pliny, are of this kind. Franklin explained these phenomena, and showed that pointed bodies were favourable to the rapid dissipation of electrical accumulations, and, as is well known, availed himself of the important fact in his application of the pointed lightning rod. How Mr. Sturgeon has contrived to associate this effect with the effects of discharges of lightning *through* conductors it is difficult to say. It is certainly a very strange confusion of things. That the effect in question has nothing to do with a sharp or round edge, or angular discharges, may be shown by the following experiments:—

(*u*). Dr. Priestley discharged a battery over a wire circuit perfectly straight, and also over the same circuit passed about pins so as to make sharp angles:—the result of the charge on fusing a given length of wire was not influenced, which could hardly have been if the angular portion had thrown off or discharged into the neighbouring pins, &c. any of the charge, it being well known that the least diminution of quantity is fatal to a delicate experiment on the fusion of wire.

(*v*). Discharge a given quantity of electricity by a continuous rod free of edges, through a wire passed through the ball of an air thermometer, and also by a similar rod with ragged edges, placed near other metallic masses: the effect on the wire remains unchanged.*

---

* For a description of this instrument, termed an electro-thermometer, see Transactions of the Royal Society for 1837, p. 18.

It is not difficult to perceive the distinction of the two cases just alluded to. If Dr. Priestley had *insulated* his wire, and then charged it in the ordinary way, brushes of light would doubtless have escaped from the angular portions; whereas the wire when acting as a discharging circuit can exhibit no such appearance. The electricity is then evanescent, and by a law of electrical action determined rapidly toward the negative surface. Many facts might be adduced conclusive of this point, but it seems scarcely worth while to dwell longer on it.

16. The great end which the author proposes to himself in this memoir, is an exposition of the danger attendant on my method of fixed lightning conductors for ships, successfully tried in the British navy for upwards of ten years;—with a view to a substitution of an untried method of his own. It may be worth while, therefore, in conclusion, to see whether the objections he so strongly insists on, do not equally apply to his own conductors as well as to mine, and, in short, to lightning conductors generally.

17. In the first place, he tells us (see 191.) "that it is possible for the most spacious conductor that can be applied to a ship to be rendered sufficiently hot by lightning to ignite gunpowder."

18. In the next place, he says, (202.) that the "lateral discharge will *always* take place when the vicinal bodies are capacious, and near the principal conductor or any of its metallic appendages." This was the case, he says, when only his small jar was used, and with this small jar he could produce lateral discharges at a distance of fifty "feet from the direct discharge."

19. Thirdly, he tells us (203.) that "the magnitude and intensity of a flash of lightning being *infinitely* greater than anything which can be produced artificially, the lateral discharges must be *proportionally greater*," that is to say *infinitely* great.

20. Taking these data as true then, it follows that any lightning conductor carrying a flash of lightning, would at an *infinite* distance, produce a lateral explosion *infinitely* great, and of course do an *infinite* deal of mischief. Hence, every powder magazine having a lightning conductor, every ship with a lightning chain in her rigging, should whenever lightning struck the conductor be destroyed; for in no case is the conductor at one third the distance from the inflammable matter, of that, at which Mr. Sturgeon can produce a lateral discharge with a jar of "only a quart capacity," viz. "50 feet."

21. But Mr. Sturgeon proposes to apply cylindrical copper rods in the rigging; their "upper extremities to be attached to the tops, &c. &c.," "their lower extremities to the chains of the shrouds," and to be united "by broad straps of copper to the sheathing," that is to say, by conductors with edges, which he says throw off the charge into neighbouring bodies; this too after having told us, that the most spacious conductor may become red-hot, and that lateral discharges *always* take place when the vicinal bodies are *capacious,* and near the principal conductor or *any of its metallic appendages.* Under such circumstances what is to become of the rigging, sails, masts? will they not be set on fire? Are not the massive iron hoops and other metals about the masts, the chains of the shrouds bolted through the ship's side, and other metallic bodies in the hull, such as bolts, tanks, chain cables, &c. &c., *vicinal capacious bodies,* and reaching by interrupted metallic circuits up to the very magazines Mr. Sturgeon talks so much about? Must not a ship with such conductors be necessarily destroyed? Surely he must give the British Association and the learned bodies of Europe and America, &c., very little credit for philosophical penetration, if he thinks they will not immediately discard such philosophy as this.

22. Either his "theoretical and experimental researches" are true, and his system of conductors fatal and absurd, or otherwise, if his conductors be good for anything, then his theoretical and experimental researches are good for nothing. He may adhere either to the one or the other, but he cannot have both; such is the *reductio ad adsurdum* in which he is involved.

Mr. Sturgeon's anxiety to arrive at conclusions unfavourable to my conductors, has led him to conclusions subversive of *all* conductors, his *own especially.*

23. The mere circumstance of finding his "*third* kind of lateral explosion" decrease in power, by uninsulating his jar, might alone have led him to doubt the accuracy of his deduction. On so important a point, and before he ventured to awaken the prejudices and fears of the uninformed, we had a right to expect at his hands a profound scientific inquiry. He should, at least, have tried whether he could not get this spark after the main charge had passed (*m*) as well as at the *apparent* time of passing. The quantity of electricity should have been accurately measured, and its effects in producing the spark determined, both in relation to the quantity and surface over which it was distributed (*p*). The form and dimensions of the discharging conductor should have been

varied (*r*). The final electrical state of his apparatus, as also the electricity of the spark, should in common prudence have been examined, (*k*), together with other manipulations quite inexcusable to neglect on such an occasion. He has however, failed in everything calculated to give value to his inquiries, as I think has been fully shown. They are hence not entitled to the smallest confidence, and it is not a little extraordinary that he should have done so, whilst taking credit to himself for *superior sagacity*, and an acquaintance with facts of which he says I did "not seem to be aware," e. g. the most common-place facts in electricity.

24. In conclusion, I have no hesitation in giving it as my confirmed opinion, after a long and severe examination of the laws of electrical action, and of cases of ships and buildings struck by lightning;—that a lightning rod is purely passive, that it operates simply in carrying off the lightning which falls on it, without any lateral explosive action *whatever*. I do not deny the general inductive effect mentioned by Lord Stanhope on bodies opposed to the influence of the thunder-cloud, and that the displaced electricity will again find its equilibrium of distribution, and return to those bodies, which effect would necessarily take place, whether we had a lightning rod or not (13); an additional reason for linking the detached conductors in a ship's hull into one great mass, so as to have as few interrupted circuits as possible in any direction.

This opinion, by the citation of a few striking cases in which ships have been struck by lightning, I hope in a future paper fully to substantiate, should you think the subject of sufficient consequence.*

APPENDIX.

The author, probably perceiving how little he had gained by quoting Lord Mahon and Dr. Priestley, observes, in a supplementary note, page 235, "Perhaps the experiments of Professor Henry would be more to my purpose." These experiments, however, are no more to his "purpose" than the others, as any one may see who will examine the Professor's communication, in the seventh report of the British Association, page 25. The experiments there described relate to minor electrical discharges, similar to those already mentioned (*i*). These were obtained by throwing simple sparks

---

[* We shall be most happy to receive and insert any further communication from Mr. Harris.—Edit.]

from an electrical machine, on small wires or rods, either insulated or connected with the earth: the wires became luminous and the rods emitted sparks. In this case, as Professor Henry observes, the electricity of the machine must be considered as free electricity; and as the bodies on which they fell were all in their natural state, the spark is immediately thrown off as a lateral discharge. Whether insulated or not, the electricity of the body is evidently acted on by induction, before the spark can be distributed over it or the earth. Hence, when sparks of about an inch long are thrown on the upper end of a lightning-rod, or other metallic body passing into the earth, the induction upon the rod and earth requiring a short time for its development, a spark is thrown off upon any adjacent conductor in a state to receive it. Such experiments, therefore, apply only to small quantities of electricity suddenly thrown upon conductors in a neutral state. This, as I have shown, (13, figure 6,) is a distinct case from that, in which a charged surface throws off its redundant electricity upon an opposite surface eager to receive it through a conducting-rod sharing in the electrical state of that surface, and which is consequently prepared already by induction to discharge it. One might be led to infer, from the particular description given by the author of this experiment, page 235, that sparks had been obtained from a lightning-rod at the time of its conveying a discharge of lightning. It may not be amiss to add, that Professor Henry did not consider these experiments as applicable to lightning-rods; and that in accordance with the opinion of Biot, he thinks the spark observable at the time of discharging a jar—that is, Mr. Sturgeon's *new* fact—is entirely owing to a small quantity of redundant electricity always existing on one side of the jar, as I have already stated, (*f*), *and not to the whole charge.*

I am, Gentlemen,
Yours, &c.
W. SNOW HARRIS.

Plymouth, Nov. 5, 1839.

Westmoreland Cottage,
December 2, 1839.

My dear Sir,

I have read your preceding paper very carefully, under the expectation of finding some close dispassionate reasoning from the pen of one who has so deservedly the reputation of being an indefatigable experimenter in electricity. I expected, also, from the title of your paper, that you would have *inves-*

tigated my fourth memoir, paragraph by paragraph, in the same uniform manner in which they are arranged; pointing out their correctness or incorrectness, in a manly and scientific order. But, although I have been sadly disappointed in this particular, I am yet willing to believe that the next time you attempt to investigate any of the results of my enquiries, your present irritation will have subsided; and that you will see the necessity and importance of keeping *close* to your subject: for no irritated man can be expected to reason well.

I am exceedingly sorry to find that you think I have "laboured hard to invalidate" your experiments which were shown to the Navy Board, at Plymouth, and the British Association, at Liverpool, &c., when no effort of the mind was necessary for the purpose. No electrician need "labour hard" to show the deceptive character of those experiments; nor would it require much effort of the mind to come to the conclusion that those experiments were either *intended* to deceive, or that their author was sadly abroad from his subject. It would be impossible for me to know which side of this dilemma you mean to choose: but I hope you will be enabled to clear up this point and that without delay; for upon *this point* alone hangs much of your credit (which I hope never to see sullied) as an electrician and a philanthropist. My only motive for reviewing your *illustrative* experiments was that of "placing them in a proper light," and I can never expect that you will object to an examination of your illustrations of a topic of such deep interest as that of marine lightning conductors, where thousands of brave men's lives are either to be protected or placed in wanton jeopardy. Think seriously on the importance of this subject before you venture one step farther in your project, and allow candour and experience to be well weighed in your mind on this momentous occasion. No one would have been more delighted than myself had your long paper shown anything like dispassionate controversial argument with close adherence to the subject; instead of which I am sorry to say, you have indulged in blunt and useless asperities which are foreign to scientific discussion, and fatal to the progress of all rational pursuits.

You must excuse my discussing the various parts of your paper individually, at this moment, as my duties press too closely on my time to give them proper attention. I can only now repeat that I am much disappointed at your not touching on the most vital part of my memoir, nor of producing any argument in favour of your favourite plan of marine lightning conductors. In the next number of these

Annals you may expect a full and ample analysis of your paper: at present I will merely offer a few questions for your solution, which, as a gentleman and electrician, you will undoubtedly attend to.

Have I, or have I not, given a fair and candid explanation of your experiments before the Navy Board, at Plymouth? (Fourth Memoir, 176, 177, 178, 179).

Have I, or have I not, pointed out other experiments which, as an electrician, you ought to have made the Navy Board acquainted with in such an important enquiry? (180).

Do you mean to be considered a philosopher, or a necromancer, by endeavouring to persuade the British Association that your blowing asunder two pieces of wood by *gunpowder*, was a true representation of the effects of lightning on a ship's mast? (181).

Have you, or have you not, made any other experiments to show the superior efficacy of your proposed conductors?

Have you, or have you not, made yourself well acquainted with atmospheric electricity by a long series of kite experiments?

To what *kind* of electrical action do you allude the bursting of the iron hoops of the mainmast, &c., of the *Rodney*, and the springing of the nails, and displacement of the lead of " the lantern of the dome" of the *Hôtel des Invalides?*

Which do you think most prudent, to endeavour to lead lightning *into* the ship, or to endeavour to keep it *out* of the ship?

These are plain simple questions, and require nothing more than plain, simple, and unequivocal answers.

I am, dear Sir,
Yours very truly,
W. STURGEON.

To W. Snow Harris, Esq.

P.S. I hope you will perceive that I have no motive in this great question, further than that of eliciting truth and protecting our brave tars from the most formidable of all nature's elements: and you may depend upon my giving you every advantage that these Annals will afford, to support the plan which you have proposed. You will acknowledge that I have hitherto been candid in this particular, by transplanting your paper from another Journal to the Annals; and as it is possible that your letter of the 15th of September may have some weight in your favour, I now offer it to the perusal of our readers. W. S.

Plymouth, September, 15, 1839.

Dear Sir,

I have never received the papers on electricity alluded to in your letter of the 12th instant, and with which I have been duly favoured. I do not think any communications of the kind were received for the Physical Section, of which I was one of the Secretaries at the last meeting of the British Association, at Birmingham ; at least, if they were, I know nothing about it. I cannot understand how any one acquainted with the nature of ordinary electrical discharges, and conversant with the practical results on the great scale of nature, can at all dissent from the simple and plain method I employ for guarding shipping against lightning. However, you seem to think my scheme a dangerous one ; and I will allow that your opinions are entitled to much consideration ; you have entered with considerable ability and skill into electrical actions, and you have my best acknowledgments of your talents. I cannot say as much for those who have been lately engaged in the illiberal crusade against me and my opinions, in London. But as I do not in any way care for, or value what they say, I do not think it worth my while to notice them. Mr. Clarke, Mr. Roberts, with a few ignorant naval men, are quite welcome to visit the Polytechnic daily for the purpose of depreciating my labours, and may publish as many pamphlets for circulation at the different bridges in London as they please. That is a mode of proceeding which must eventually recoil on themselves ; to say nothing of its being unhandsome, illiberal, and uncalled for. I must say I was not a little annoyed at finding you associated against me with others ; since I had always from the time I first met you at Oxford, at the meeting of the British Association, thought we were on better terms ; and that any difference about a philosophical subject might have been settled between us in a better way. However, I cannot help it.

Well now, you say you are about to publish some communications which are to point out the danger of my system of defence from lightning. I cannot possibly have any objection to this. I only hope you will be careful to inform yourself respecting the true state of the question, and not misrepresent me as others have done (unintentionally it will be I have no doubt). You will excuse, I am sure, my saying, if I may judge by your letter, that you have not examined the question faithfully. Let me, therefore, put you in possession of a few points as it may probably save both of us trouble. Like some others you begin by assuming that I have overlooked some important facts connected with discharges of lightning. Perhaps the contrary may be found to be the case and that those who oppose me have mistaken the road; and I think I see where the mistake made by those who talk of danger from a lateral discharge lies. However, of this more by-and-bye.

I beg you to observe that you are quite wrong in supposing that my conductors pass through the magazines.* Why I never dreamt of such a thing; neither do they exclusively go into the body of the hull; since large metallic bands lead off under the deck to the iron knees, &c., in the side. My object has been to connect all the masses of metal in the hull, and the conductors on the masts into one general system, so as to admit of a general and rapid distribution of the fusing charge without explosion or damage.

You say "there is an apparent intention to introduce my conductors in the navy." Are you not aware of the *fact* of the *conductors having been used in the navy for the last* 12 *years or more!* Why they have been fitted in six frigates, many line of battle ships, and smaller craft. Men have been exposed to lightning in all parts of the world—South America, Tropics, Coast of Africa, Mediterranean—some have been struck by lightning. I understand in the late inquiry which the government ordered with a view of examining the success of my plan, that extremely valuable evidence has been obtained from naval officers in command of their ships, and from others who have been exposed to lightning under various circumstances. It is, I am told by the Secretary of the Admiralty, very voluminous, will be printed and laid before Parlament. I do not know the amount of it myself, but I think it would be as well to examine the documents before we enter upon the public discussion you have marked out; as to mere opinion it will go for nothing any way; and you must go to facts. Allow me to call your attention to the Nautical Magazine, No. 2, for February last, 1839, for the actual effects of lightning on three ships of the navy; and if you will go to *Mr. Payne*, at the *Polytechnic*, he will show you the diagram I left there, illustrative on a large scale of those effects. Tell me, where was the calorific and lateral discharge to which you allude in this case? If we could meet and examine this subject together experimentally, I believe we should soon settle the difference. Whatever I may be induced to do by way of reply to anything you advance, will be simply an appeal to facts. I possess a great body of evidence from a history of cases of lightning on ships which bear out my views; but I certainly shall not write anything until I see the result of the inquiry and investigation lately instituted by the Admiralty. I think you would do well not to advance anything without a pretty close appeal to experience. I shall always consider that I have been illiberally treated by many persons in this affair, as you would say if you knew all.

---

* Will Mr. Harris say that not one of his conductors passed through the powder magazine of H.M.S. JAVA? See Lieut. Green's Letter, p. 329. Edit.

I am, dear Sir,
Yours faithfully,
W. SNOW HARRIS.

To W. Sturgeon, Esq.

XL. *On the effects of Lightning on H.M.S. Beagle.* *By* LIEUT. SUTWAY. *In a letter to the Editor.*

Hushing, near Falmouth,
October 9, 1839.

Sir,

Having considered your communication in the Annals of Electricity, on marine lightning conductors, containing observations on the stroke of lightning which fell on the masts of H.M.S. Beagle, I think it fair, both to Mr. Harris and the naval service, to describe the phenomenon I witnessed on that occasion; first stating that at the time of my joining the Beagle in 1831, previously to her leaving England, I had no acquaintance with Mr. Harris, and certainly no *bias* in favour of the conductors with which the ship was fitted. I may, therefore, claim to be considered an impartial observer.

At the time alluded to, I was first Lieutenant of the Beagle, and was attending to the duty on deck. She was at anchor off Monte Video, in the Rio de la Plata, a part of the world very often visited by severe lightning storms. Having been on board H. M. Frigate, Thetis, at Rio Janeiro a few years before, when her foremast was totally destroyed by lightning, my attention was always particularly directed to approaching electric storms, and especially so on the occasion alluded to, as the storm was unusually severe. The flashes succeeded each other in rapid succession, and were gradually approaching; and I was watching aloft for them when the ship was apparently wrapt in a blaze of fire, accompanied by a *simultaneous* crash, which was equal, if not superior, to the shock I felt in the Thetis; one of the clouds by which we were enveloped, had evidently burst on the vessel, and as the mainmast appeared for the instant to be in a mass of fire, I felt certain that the lightning had passed down the conductor on that mast. The vessel was shaken by the shock and an unusual tremulous motion could be distinctly felt; as soon as I had recovered from the surprise of the moment, I ran down below to state what I saw and to see if the conductors below had been affected, and just as I entered the gunroom, the purser, Mr. Rowlett, ran out of his cabin (along the beam of which a main branch of the conductor passed), and said that he was sure the lightning had passed down the conductor, for at the moment of the shock he heard a sound like rushing water passing along the beam. Not the slightest ill-consequence was experienced; and I cannot refrain from expressing my conviction that had it not been for the conductor, the results would have been of very serious moment. This was not the only instance, when we considered that the vessel had been saved from being damaged by lightning by Mr Harris's conductors; and I believe that in saying I had the most perfect confidence in the protection which those conductors afforded us, I express the opinion of every officer and man in the ship; and as Captain Fitzroy's opinion must have much greater weight than mine, from his superior knowledge on the subject of electricity, I cannot refrain from copying his opinion of the conductors in the Beagle, which is published in his appendix to the Beagle's voyage.

"Previous to sailing from England in 1831, the Beagle was fitted with permanent lightning conductors invented by Mr. W. S. Harris, F.R.S.

"During the five years occupied in the voyage she was frequently exposed to lightning but never received the slightest damage, although supposed to have been struck by it on, at least, two occasions; when at the moment of a vivid flash of lightning, accompanied by a crashing peal of thunder, a hissing sound was heard on the masts, and a strange though very light tremulous motion in the ship, indicated that something unusual had happened.

"The Beagle's masts, so fitted, answered well during the five years' voyage above mentioned; and are still in use on board the same vessel on foreign service.

"Even in such small spars as her royal masts and flying jib-boom, the plates of copper held their places firmly, and increased rather than diminished their strength.

"No objection which appears to me valid has yet been raised against them; and were I allowed to choose between having masts so fitted and the contrary, I should not have the slightest hesitation in deciding on those with Mr. Harris's conductors.

"Whether they might be further improved, as to position and other details, is for their ingenious inventor to consider and determine. He has already devoted so many years of valuable time and attention to the very important subject of defending ships against the stroke of electricity, and has succeeded so well for the benefit of others, at great inconvenience and expense to himself, that it is earnestly to be hoped that the government, on behalf of this great maritime country, will, at the least, indemnify him for time employed and private funds expended in a public service of so useful and necessary a character."

Not being sufficiently acquainted with electrical experiments, I cannot remark upon those you have adduced in support of your opinions detrimental to Mr. Harris's conductors. I can, therefore, only repeat my conviction that the Beagle was struck by lightning in the usual way, and certainly without any *lateral explosion* or other ill effects, similar to those you insert in your Annals of Electricity.

I am, Sir,
Your obedient servant,
B. T. SUTWAY,
Lieut. R. N.

Observations.

Lieut. Sutway's description of the lightning rods on the Beagle is obviously of a very different character to that given by Capt. Fitzroy, and certainly much more favourable to the idea of the ship being struck than given by the latter officer. I consider Lieut. S's description of the occurrence exceedingly valuable; for it is the minute detail of the effects of lightning that we are most in want of, and it is much to be lamented that our data on this momentous topic is yet so scanty. There is nothing, however, in this letter that can in the least affect my statements regarding the electrical principles that would be brought into play by flashes of lightning striking vessels.

W. STURGEON.

XLI. *On Mr. Snow Harris's Lightning Conductors, as applied to Shipping. In a letter to the Editor. By* W. Pringle Green, Lieut. R.N.

4, James Street Adelphi, July 18, 1839.

Sir,

An important epoch has arrived in practical electricity by the Government appointing a committee to determine on the subject of fixed conductors, fitted to the masts of several of her Majesty's ships, passing through the hull and after-magazine. Having in the year 1822, on the scheme being introduced into the Navy, by order of the Navy Board, opposed its adaptation, upon incontrovertible evidence, I am again prepared to show, its being an ill-copied plan of Mr. Marrot, published in 1812, in the Naval Chronicle, Vol. I, p. 201, and the extreme danger of such conductors, proved, by experiment, and a mass of electrical phenomena: and by my representation of these facts the then existing Board of Admiralty countermanded the N. B's. order. As this plan has been introduced into the Navy, and the necessity of investigating a matter of such vital importance to the state at this time, needs no comment: as I do not believe it possible otherwise than by a perusal of the account of experiments made at Plymouth; of my queries and experiments; and a review of my researches during 35 years in every quarter of the globe, illustrated by drawings, for the most experienced theoretical electrician to give a correct decision. I am, therefore, desirous to put you in possession of the whole of this matter, upon which I take my stand.* At this moment the subject acquires a great interest throughout the Naval Service, and very gross deception has been, and continues to be, practised upon that service and the public, at a heavy cost to the nation, by making experiments which seems to demand the protection of the public press.

I have the honour to be, Sir,
Your obedient servant,
W. PRINGLE GREEN,
Lieut. R. N.

Lieut. Green's Queries.

1. Will not the superabundant electric fluid from the spindle in the truck, which passes six inches into the body of the mast, explode and destroy it?

2. How is the spindle to be substituted when the top-gallant mast is on deck, which is generally the case in stormy weather, the cap has much iron about it this being the highest point? It is not possible to place the spindle and connecting copper across the cap, without being in contact with much iron about it. Will not this iron draw off the fluid and cause an explosion? And will not the nails in the copper strips

---

* We have in our possession much valuable information on this subject from Lieut. Green, but only give a few of that Officer's queries in this place. Edit.

do so? If these conductors, such as the proposed, are sufficient safeguards, when passing through a ship, how is it that Heckenham Poor-house was set on fire though it had eight of the largest and most approved conductors placed on the outside; and what must have been the result had they passed through the building? As conductors can be surcharged, broken, and fused, and electric fluid becomes sensible in the form of a spark upon the surface, and, as it has been shown by experiment, streams of flame are sometimes conducted along the surface of a conductor; are not these facts alone sufficient proof, that it is dangerous to conduct these electric streams through a ship's *powder magazine?* When the electric fluid is sensible in the form of a spark or sparks, or in such streams, and conveyed by the conductor to the inflammable air in the bottom of a ship, will it not cause ignition of this imflammable air and and burn the ship? Hydrogen is put into a gaseous state by the agency of electricity, and the bilge water would be decomposed into oxygen and hydrogen gas and instantly blaze. Will not the electric sparks which form upon the conductors pass off to the iron tanks and iron ballast, and may it not explode under the powder magazine where it is conveyed by iron ballast?

3. How is it to be presumed that conductors, such as the proposed, can guard a ship from a stroke of lightning when it is known that a single flash fused a conductor on the mainmast, shivered the foremast, splinters distant parts of the deck, and a sufficiency of the electric fluid passes down below, destroying bulk-heads and fusing a bar of metal there. The spare topmasts and topgallant masts being fitted with conductors, and placed in the centre of the ship, as is the custom in all her Majesty's ships between the fore and mainmasts, pointing both to the quarter-deck and forecastle, on which the officers and crew are always in considerable numbers. Will not these people be killed by a discharge from these longitudinal conductors?

4. Should the conductor convey any portion of the fluid to the bolts in the keel touching the copper sheets on the bottom, will it not pass along the bottom and knock off the remainder, and will not these bolts be driven out and the keel split? What will be the expense to complete the Navy with such a scheme? Upon a very moderate estimate it will require £500. to dock and complete each ship, and £300,000. be required to complete the whole Navy. That there is not the smallest difficulty attending the hoisting up a chain conductor, it is a fact, for one man and a boy can accomplish this; and when up and fastened to the back-stays, it cannot

be injured though it remain up for a voyage. In what does the plan differ from Mr. Singer's, proposed nine years previously, who first put bolts through the keel, or those thirty years ago in use in the French Navy; both being abandoned by the inventors as chimerical and dangerous in the extreme.

As long back as Capt. Cook's being at Java, in the Endeavour, in 1769, the dangerous effects of spindles in the mast of ships are recorded by Mr. Green who accompanied him. During a storm of thunder and lightning and rain, the mast of a Dutch Indiaman was split and destroyed from the spindle to the deck. So great was the shock, that considerable fear was entertained for the safety of the Endeavour, as the explosion shook her like an earthquake; proving that it is not only the ship or building to which a conductor is affixed, that is endangered, but all for a considerable distance around. If a single spindle can invite so powerful and dangerous an agent, how much greater must be the stroke in presence of three of them such as are placed in the masts of her Majesty's ships, in ordinary at Plymouth. A link of the chain conductor such as used at sea is put over the massive spindle and continued to the water. The first experiment made was to prove the danger of such a scheme. A model being produced, and the spindle exposed to an ordinary discharge of the fluid from the battery, the chain was instantly fused by the lateral discharge, and the mast splintered, proving the danger of the plan, and that the strips of copper of the fixed conductors to the masts of ships would be fused from the spindle.

A mast thus splintered and set on fire would involve the ship in destruction. It has been asserted by the suggester of this scheme, that although the fixed conductor had been cut through by a saw, or a break made in it, this would not impede the passage of the fluid. "That sparks were passed through gunpowder without igniting it, and that electric fluid is always transmitted along the surface of conductors."

2nd. Ex. A copper conductor was passed through the centre of the magazine of a ship fitted after the plan in question, *precisely as H. M. Ship Java was fitted,* with the exception of it being nailed to the mast. The conductor was cut through to represent the break said to be made in the one fixed to the mast of the cutter, in the public experiment, powder placed near to the fracture, in a shock being passed through the conductor instantly ignited. A greater charge was then sent through the conductor which was instantly melted, globular metal being produced. Several other experiments were made to exemplify more satisfactorily the *fusion of conductors* by lightning.

XLIII. MISCELLANEOUS ARTICLES.

Letter to the Editor of the Annals of Electricity, &c., &c. From the REV. N. CALLAN, Professor of Natural Philosophy.

Maynooth College, Nov. 11, 1839.

Dear Sir,

I have read, within the last week, a letter from Professor Forbes, of Aberdeen, to Dr. Faraday, in which he states that Mr. Davidson, of Aberdeen, has been eminently successful in the production of a moving power by electro-magnetism; and that Mr. Davidson "is the first who employed the electro-magnetic power in producing motion by simply suspending the magnetism without a change of the poles. This, he says, Mr. Davidson accomplished about two years ago." I believe I may fairly dispute Mr. Davidson's claim to be the first who employed that method of applying electro-magnetism as a moving power. It is about two years since I first constructed an electro-magnetic engine for the production of motion, in which there was no reversion of the poles of the magnets, but only a suspension of their magnetism. In a letter of mine, dated February 20, 1838, and published in the Annals of Electricity, on the first of April, in the same year, I refer to three different electro-magnetic engines which I had then made. In one of these there was no reversion of poles; but only a suspension of the magnetism. I have since made several engines on the same principle. I made one in August, 1838, for the Right Rev. Dr. Carew, Coadjutor, Bishop of Madras, which he brought with him to India, for the use of his Seminary. In this there were two magnets; the motion was produced not by a reversion of poles, but by a suspension of the magnetism. In the commencement of the present year, I made one on the same principle for the College. It worked very well, although it contained only a single magnet: it was exhibited to my class last February. Within the last two years I have made a great variety of machines for the production of motion by electro-magnetism. In some of these the poles were reversed; in others, the magnetism was only suspended; and in others, the magnetism was constantly maintained. In some the motion produced by the magnetic force was rectilinear, and, of course, a crank was employed to convert the rectilinear into a curvilinear motion: in others, a rotary motion was directly produced by the magnets. The crank engines differed from all the crank engines of which I have seen any description, in this respect, that the length of the stroke might be ten or twenty feet if necessary, without any diminution of the *moving* power of the magnets. The immediate publication of the results of my experiments would be premature as some still remain to be made. I intend soon to make an engine of considerable power. My experiments give me every reason to think that, with a given battery, the moving force of each magnet, in that machine, will be at least eight or ten times as great as it would be if the magnet were placed in an engine constructed on the plan of Professor Jacobi. I do not know the plan of Mr. Davidson, and therefore cannot compare it with mine. The moving force which he obtained appears to me very small, when I consider the size of the battery employed. However, that may have arisen from a defect in the construction of the machine rather than from any defect in his plan. He certainly deserves encouragement. I agree with Professor Forbes that "it would be much for the interest of railroad proprietors," and still more for the interest of companies who use stationary engines, to take up the subject; and to incur the expense of making experiments, on a large scale, on the best method of applying electro-magnetism to the working of machinery. I am fully convinced by the experiments of Professor Jacobi and of Mr. Davidson, and still more by my own, that electro-magnetism will ere long be substituted for steam. I intend to send you, as soon as convenient, an account of the principal experiments which I have made for the purpose of ascertaining the best means of employing electro-magnetism as a moving power.

I have the honour to remain,
Your very obedient humble servant,
N. CALLAN.

Method of distinguishing the Arsenuretted, from the Antimoniuretted Hydrogen Gas. *By* PROFESSOR MAX.

The metalluretted hydrogen gas for examination being ignited at a jet, a piece of porcelain is held over it till a dark speck is produced. The speck is then moistened with a drop of nitro-muriatic acid. Then by adding a drop of the aqueous solution of sulphuretted hydrogen a precipitation takes place. If the tested gas be arsenuretted hydrogen the precipitated substance is of a *pure yellow colour*: but if it be antimoniuretted hydrogen the precipitated substance is of a deep *orange colour*. The difference of colour is so distinct that no one can mistake the one from the other. The whole process is exceedingly simple and may be performed in a few minutes. *Poggendorff's Annallen des Physic and Chemie*, No. 3, 1838.

The following method has lately been given by Mr. Marsh. The piece of glass or porcelain intended to receive the metallic crusts is to have a drop of distilled water placed on it: and then, with the drop of water on the lower surface, held a little above the apex of the cone of flame of the burning gas which is issuing from the jet. If arsenic be the metal in the hydrogen, arsenical acid formed by this process is dissolved by the drop of water, and is is easily detected by a drop of the ammoniacal nitrite of silver, which immediately produces the arsenite of oxide of silver, which is of a lemon yellow colour. If antimony be the metal in combination with the hydrogen gas under examination, no such result is produced by this process. When much arsenic is present it will be advantageous to employ a clean glass tube, about six inches long, and slightly moistened inside with distilled water. The tube, thus prepared, is to be held vertically over the flame of the burning gas, and a strong solution of the substance is soon obtained, which may be tested as before stated.

My dear Sir,

Your correspondent, C. Barker, Esq., has noticed my question in your Annals for June 1838, respecting the spotted jar. If he did not observe the appearance of the sparks as I then stated, it is possible I may have been mistaken; it is many years ago since I noticed what I remarked. To save the trouble of fixing spots on the inside of a Leyden jar, I lined it with plain tinfoil, and on the outside fixed very small spots the size of those used for spiral tubes. Now, the pieces of tinfoil being so diminutive, they could hold but a small portion of electricity, and therefore the spark might be almost imperceptible. I regret not having leisure to try another jar lined with plain tinfoil, for, if it answers, a great saving of trouble is effected.

I feel greatly obliged to Mr. Barker for reminding me of this circumstance, as it gives me an opportunity of correcting a mistake; and also for the valuable information contained in his letter inserted in the last No. of your Annals. His recommendation of an insulated stand is well deserving the attention of all experimenters; I have used it for several years to exhibit the electric fly, orery, dancing figures, &c. but lately it occurred to me that a resinous plate about 12 inches in diameter, and a third of an inch thick, will answer all the purposes of an insulated stand.

Mr. Barker enquires how to line the inside of large carboys, to make them Leyden jars, which you have informed him, but I think he will find that although green glass does very well for electrical machines, it will not answer for Leyden jars; I once constructed a battery of them which proved useless.

I am,
Very truly yours,
J. HARPER.

Oxford, Dec. 26, 1839.

THE ANNALS

OF

ELECTRICITY, MAGNETISM,

AND CHEMISTRY;

AND

Guardian of Experimental Science.

APRIL, 1840.

LXV.—*On the Connection between Electricity and Vegetation.*

By Thomas Pine, Esq. (Resumed from page 253.)

If the air in a state of purity imparts a strong electric excitement to the embrios of plants, and thus produces a commencing movement in the vegetable juices, no sooner is the germ beginning to open than it craves the influence of vapours to maintain its increasing vitality, and promote its growth. Accordingly it is endowed with powerful conducting qualities in its structure and functions suited to the new element on which it has to act. The low springing herb, and the shooting and slightly expanding leaf of every description are now offered to our notice. For the fixed, rigid, texture of the seed and bud, is substituted the lithe, elongated, but acutely edged and pointed, form of the leaf, waving with every breeze, as if to catch and appropriate to its use every approaching vapour. These most intense attractors of electricity find it in the atmospheres of vapours as they gradually condense into the liquid state. In nothing is it more conspicuous than in the action of the tender herd upon the morning dew. The creeping species are the most remarkable for this quality, as they receive their watering in a great degree, particularly on fine summer mornings, from the dew which then appears to arise from the soil and to undergo a condensation from the attraction of the young leaves, aided probably by the cold produced by the evaporation. The down of the leaf, distinctly discernable only by the microscope, is in this case the principle organ of attraction, of which that of the *strawberry*, affords one of the most remarkable specimens. When in its growing state, its fine *needles*, placed at convenient distances from each other, exhibit at each point a transparent globule at a considerable distance from the surface of the leaf.

The same effect is observable as the general attendant of the settling of dew on the herbage, and on the early shooting of plants. Observing the dew on some herbs on which no down was apparent to the naked eye, I found on a minute microscopic inspection, that points of extreme tenuity were the agents in producing the watery effusion. This agency, is peculiarly conspicuous in the *vegetable marrow*, the stem and leaves of which are every where bristling with fibrils, to whose acute extremities the globules attach. A beautiful display of the principle appears in the herb called *alpine wall cress;* the leaves of which are furnished with innumerable stems perpendicular to either surface, and branching out into four needles after the manner of our metallic protectors; at every point of which a globule attaches, while a larger globule is formed at their common centre. What but that peculiarly energetic property of electric attraction with which all vegetating points are endowed, can have operated to produce this effect? The condensing vapour exudes its latent imponderable fluid, which, entering the pores of the leaf, leaves a portion of water in the liquid state upon its surface; while a larger portion probably descends more completely deprived of its electric matter to the roots; thus furnishing an opportunity for its action from above in causing the absorption of the liquid forming the materials of the sap, through the exquisitely minute channels of the stem, from the innumerable storms at the ramifying extremities! A powerful attraction is thus manifested by each minute vegetable point for the floating vapours, which, operating in conjuction with a low temperature deprives them of their gaseous caloric, and reduces them to their liquid state. These points being exquisite conductors of electricity, and acting with peculiar energy, as such, upon the clouds and vapours of the atmosphere and at considerable distances, can it be questioned that this is the species of attraction that they have exerted in condensing the vapour, and attaching the liquid water to their extremities; that a portion of the subtile fluid has been imbibed by the leaf, and through the channels of the wood, which from their extreme minuteness may be considered as so many *tubulated points*[*] by which the electric influence is acting with the greatest possible efficacy on the rising sap. It is thus made to mount and spread, and impart vital energy and expansion to the whole plant. The same principle which acting from the pure air produces a first vegetable excitement on the prepared materials of the seeds and buds, and the commencing shoots, and which so evidently operates in proportion as it is increased by natural or artificial means in promoting this result, is now administered in much larger quantities through points of far greater number and efficacy, and conveys with it one of the most essential ingredients of nutrition; and thus the source of increasing vitality and an essential material of growth and vigour are furnished by the same process.

By a similar process, the loftier plants and more advanced vegetation receive a like vitalizing and nutritive influence from the clouds and vapours. The upper branches and spreading ramifications of the trees must act with great energy, and with little interruption from any contiguous bodies, on the clouded atmosphere which forms around them with every approaching shower. They are well known to be strong attracters of clouds; and it has been observed, that in insular or detached situations in which a few trees form the sole attracters, the atmosphere of vapour is in a great degree confined to their summits, and they have been described as so many "alembics" from whose leaves water is continually "distilled," and that "in some of the smaller islands of the West Indies, where there are no rivers or springs, the people are supplied with water merely by the dripping of large tall trees, which, standing in the bosom of a mountain, keep their heads constantly inveloped in fogs and clouds, from whence they dispense their kindly never-ceasing moisture."[†] But as I am acquainted with no facts which so distinctly manifest the agency of plants in depriving vapour of electricity, as those which appear in several experiments made by J. Williams, Esq. as related by him in his "Climate of Great Britain," I shall extract the particulars from his valuable work, to be inserted if it be thought requisite for the convenience of your readers, as an appendix to these remarks.

---

[*] Though *tubes* cannot in strictness be *points*, yet as these natural tubes bored by the divine hand with a minuteness precisely adapting them for the action of a fluid which was ascertained by the numerous experiments of the Abbé Nollet, to promote the flow of liquids through tubes, in degrees increasing with their minuteness, and as they approach by many degrees nearer to physical points than any that can be discerned by our unaided vision, I have used the above expression the more effectually to convey an idea of their conducting power. And when it is considered that the solid matter through which they are bored is a complete *non-conductor*, confining the action of the electricity entirely to the liquids contained in the tubes, they must, I think, be seen to be performing the functions of exquisite electrical points in promoting the rise and flow of the vegetable juices.

[†] White's "Natural History of Selborn."

It is easy to see that several important consequences, highly beneficial to the animal as well as the vegetable system, must result from this arrangement. The attraction of the leaves must operate to produce the condensation of the vapours, at much higher temperatures, and in a more gentle and beneficial manner than could be effected by the mere action of cold; and as large quantities of the subtile fluid are thus gradually imbibed into the substance of plants, and in part transmitted to the earth, injurious accumulations of it in the atmosphere are averted. I conceive that in the absence of plants when there could be no cause of the condensation of vapours, except a low temperature, and were considerable quantities of aqueous gas actually formed in the atmosphere, the result must be, that no condensation would be effected till the cold had become extreme, when there would be a sudden immense percipitation of vapour, and its gaseous caloric being as suddenly set loose, a portion of it would rapidly combine with the particles of air, thus heating and rarifying it to an extraordinary degree, and another portion remaining in a state of separation from any gravitating matter would exhibit electrical phœnomena in degrees which must greatly disturb the harmony of nature, and produce the most disastrous effects. There are some neglected or unproductive districts where an approach toward this condition of the elements has been experienced, of which I find the following general description. "In countries which are uncultivated the weather is generally in extremes. Rain when it falls takes the form of an overwhelming flood, not gently entering and moistening the soil, but rushing along the surface, tearing up one place, strewing another with *debris*, and reducing both to a state of indiscriminate ruin; while scarcely has the flood gone by, when the returning heat evaporates the little moisture which is left behind, and burns-up the coarse and scanty vegetation which the rains have fostered." Of the salutary change from such a state to one in which the extremes of cold and heat, of moisture and dryness, have been greatly mitigated with the progress of cultivation, an example is alleged from the central part of Scotland.* No particular notice is here taken of the electrical phœnomena attendant upon the other extremes, nor on the effects produced by an improved vegetation in softening their character; but it is well known, that intense heats and sudden depositions of water are the ordinary precursors and attendants of strokes of lightning. One of the heaviest and most sudden *sheets* of rain that has fallen under my observation, and indeed, I think it considerably exceeded any that I had previously experienced, was accompanied with a still more extraordinary display of lightning, the fluid flying with zig-zag course incessantly in all directions, and occasionally sweeping round and exhibiting to view the whole black concave whence it issued!—Storms and hurricanes are very much the attendants of a partial and irregular distribution of plants, such as is incident to uncultivated spots,* and in high latitudes where there is a large effusion of the solar beams;—but more especially over small, rich, cultivated clumps in islands surrounded by the ocean. The heavy accumulations of clouds, and rains on those spots, particularly when the winds set in a corresponding direction, are the well known accompanyments of those terrific electrical discharges which are occasionally witnessed in the islands of the western ocean. Since the temperature in those latitudes is seldom sufficiently reduced to be of much avail in the condensing process, the attraction of lofty and luxurient trees is almost the sole instrument in producing the effect, and the immense quantities of caloric which are abstracted by evaporation from the ocean and plants united are concentrated on the devoted spots; the consequences are those prodigious discharges of water,

---

* "Library of Useful Knowledge" Vol. xv. Part I. pp. 2, 3. The

---

particulars of this change are as follows; "Within the experience of persons still living, the snow which in that country began to fall in November, was not wholly gone until the month of April; while in the middle of Summer the heat was so excessive, that agricultural labourers were obliged so suspend their toil during four or five hours in the middle of the day. At that time the autumnal rains frequently descended with so much violence, that the crops which had been retarded by the coldness of the spring, were prevented from ripening in the high grounds, were lodged and rotted in the lands that were lower, and swept away by the swelling of the streams over the holms and meadows. In the same spots at the present day, the quantity of snow which usually falls during the winter, is comparatively small, appears rarely before Christmas, and is gone in February or early in March. The summer heat is more uniformly distributed, seldom amounting to a degree oppressive to the labourer, or protracted to a term injurious to the crops, while the rain which follows is neither so violent in degree, nor so long continued, and happening when the grain is far advanced toward ripeness, the injury which it does is comparatively trifling." *ibid.* pp. 3, 4.

* *Australia* is one of the uncultivated spots instanced under the above general description.

of heat, and of electricity which are incident to those islands.

The co-operation of cold and of vegetation, in producing the condensation of vapours, or rather perhaps their respective effects in the absence of each other, forms of itself a curious and interesting subject of inquiry? When the sun's rays are so few and inefficient as to leave the ordinary temperature of the atmosphere considerably below the point at which vapour is transformed into water, they rarely if ever produce any electrical effects upon, or very near, the earth's surface. But when the temperature advances considerably above that point, there appears to be no other means of counteracting their tendency to float loosely in the atmosphere, and appear in the form of electricity in degrees which must prove highly injurious, but by their uniting with water in the form of vapour. This they do in the first instance from the ocean, seas, and rivers; but as they accumulate over particular spots, the agency of plants becomes more and more necessary in aiding the process, and in so appropriating and disposing of the vapours as to render them most effective both in carrying forward their own progressive advancement, and in protecting animals against the effect of extreme heat and pernicious accumulations of electricity. But a better opportunity will be afforded of treating of this subject in reviewing the state of vegetation in its most advanced stages.

It may deserve consideration how far the carbonic acid operates as a mean of exalting the conducting efficacy of plants, since they appear to live and thrive in proportion as a sufficient supply of this acid in its combination with water, is furnished to the roots. A sprig of mint, though peculiarly adapted to live and strike root in water, slightly impregnated with this gas, will speedily wither in water from which all access of this gas is excluded. If I might be allowed to conclude this paper with a conjecture concerning the superior conducting efficacy of vegetable points above those of metals and all other substances, and that notwithstanding the strong non-conducting property of their solids, it should be by a reference to the *expansive* nature of all growing substances, in which they greatly exceed even metals, together with the quantity of oxygen which is conveyed by means of the acid and water to the positive electricity from the upper extremities of the plant, while the remaining elements of carbon and hydrogen under the influence of negative electricity, combine to form the larger portion of the materials of the produce. The solid portion of the leaves is I believe known to be principally composed of those two elements, and their attraction for the carbonic acid from the atmosphere, when it is decomposed, and yields pure oxygen gas under the influence of the sun's rays, while the carbon is retained, strongly favours the conclusion; and the ripening process of fruits, which seems to be in a great degree effected by the extraction of oxygen from their juices under the same influence, allowing those rays, to be positively electrical, tends to its confirmation. Hydrogen seems to be a promoter of the green colour of the leaves, as plants confined in this gas improve in greenness, and the discovery of the decomposition of water in vegetation by Mr. Weeks, enables us to perceive that hydrogen is supplied to plants from water, no less certainly than carbon from the carbonic acid. I apprehend that electricity will be allowed to be an essential agent in producing these decompositions. We are here reminded of a statement of Dr. Darwin, that "the production of oxygen gas from green leaves, and other green vegetable matter is probably owing to the decomposition of the water perspired by the plant, and as the oxygen may be expanded into gas by the sun's light, the hydrogen may be detained in the pores of the vegetables. Hence plants growing in the shade are white, and become green by being exposed to the sun's light, for their natural colour being blue, the addition of hydrogen adds yellow to the blue, and *turns* them green." It does indeed seem to be highly probable that as carbon and hydrogen, when separated from oxygen as they probably are in the most perfect manner in the leaves of plants, by the potent agency to which they are exposed from the solar rays, should unite in forming the substance and colour of the leaf. And to what but electricity or galvanism can we ascribe this twofold decomposition and recomposition in the production of the substance and beautiful compound colour which distinguishes the vegetable kingdom!

From Mr. Williams' "Climate of Great Britain," with Remarks.

His chapter on "the power of Vegitables to deprive Vapour of its Electricity," &c. abounds in evidence of the principles we are maintaining; but as his views in several respects do not coincide with the objects for which they are here adduced, I shall accompany the extracts with such observations as may throw some further light upon their principles.—He says p. 63 that "he was principally led to the consideration of this property in vegetables by remarking the drops of water on the edges and angular points of the leaves of grass about sunsetting, before any general precipitation of nocturnal dew was perceptable; and from observing that trees and hedges occasioned a precipitation of fog, when attended with *a gentle wind, but not in a calm.*—"This important fact" he had "repeadedly verified" by the use of the electroscope. "Upon the 15th and 16th days of September, 1805," he writes "there was a very dense fog. On the morning of the 15th it was attended with a perfect calm: the trees and hedges being loaded with dew; but no precipitation of the fog, and the electricity strongly positive; at eight A. M. it began to clear away; and at ten A. M. the sun shone bright, and the day was tolerably fair. On the following morning the fog was equally dense; but about seven A. M a gentle wind arose from the south, which bringing new particles of vapour within the conducting influence of trees and hedges, occasioned a copious fall of vapour from their leaves and small branches, but no general precipitation occurred." In general he observes that "if the electroscope shows signs of electricity it ceases to do so when brought within six or ten feet of a tree, or hedge, owing to the power these possess of drawing off the electricity of the vapour which comes within the power of attraction." This power may be rather referable to the general electricity of the atmosphere than of the vapours which are diffused through it, especially if no vapours are visible, since it may be questioned whether in a partially condensed state, or on their having been recently condensed by the conducting agency of plants a larger portion of the fluid may not float for a time in their vicinity than in situations in which the vapours remain in a state of transparent gas having no electrical atmosphere around them. But on the experiments on fogs we see a clear exemplification of the agency of vegitation, especially when assisted by winds, in causing the condensation of vapours to their liquid state at temperatures in which in its absence they would remain partially condensed yet floating in the atmosphere. It must also assist in causing a farther precipitation by keeping the temperature comparatively low in consequence of the quantities of the fluid it imbibes, and which would were the whole of what formed the caloric of vapour diffused through the atmosphere raise it to a temperature that would operate to prevent the process of precipitation from proceeding. In like manner it must avert dangerous accumulations of electricity, for a portion only of the fluid which is set loose by the previous cold, entering into combination with the particles of air, the remaining portion must float in an uncombined electrical state, and animal life unprotected by the conducting agency of plants must be exposed to frequent attacks. What are the pestilential *simoons* of Arabia, but electric matter bursting from an atmosphere already charged with it, and which finding no medium of conveyance or opportunity of distribution, but through the bodies of animals thus exposed to its influence, produces effects upon the frame equalling or exceeding that of a powerful galvanic battery.

The following beautiful experiment clearly shews the existence of an electric atmosphere aronnd the surface of vapour as it condences by cooling, and that it is attracted and absorbed by the leaves of plants. "To the cap of a gold-leaf electroscope, I affixed a horizontal support for a candle, which projected two feet from the cap of the instrument placed near the edge of a table; on the floor immediately below was an eathern vessel containing hot water about one inch in depth; the candle being lighted, two or three red hot embers were dropped into the vessel of water, which instantly raised a sudden cloud of vapour; the electricity of this being collected by the candle connected with the electroscope, the gold leaf opened suddenly and struck the sides positively. Some branches of trees with their foliage were now placed between the vessel on the floor and the candle; the experiment being repeated the vapour passed through the interstices of the boughs but the electroscope opened only half an inch; more boughs were now added and slightly sprinkled with water to increase their conducting power, the experiment was again repeated; a great part of the vapour still made its way through the interstices of the leaves and branches, but so completely deprived of its electricity that the gold leaf did not diverge in the smallest degree."—pp. 73,74—The effect of the sprinkling of the water was probably to aid a little in the condensation by its cooling influence, and thus to add rather to the quantity of electrict matter discharged than by its far inferior conducting agency to that of living leaves to aid in its removal. The great efficacy of the leaves in absorbing the fluid is thus rendered strikingly apparent; the continuance of a great portion of the vapour in an uncondensed state must have been the consequent of the raised temperature; in nature temperature and vegitable attraction are often so admirably arranged that the vapour disappears, and a clear, mild, and moderately electrified atmosphere is the frequent result of their cooperation.

On the *several species of vapours.*

It may not be improper here to observe that there appears to be a threefold provision in nature for the irrigation of plants of different magnitudes and under different circumstances. The clouds which sail aloft are the evident result of the action of the solar beams upon the waters; and these are much devoted to the larger and loftier vegetation. After a drying day the creeping species and the forming shoots of plants require immediate moisture, and it accumulates upon them in the absence of the sun in the form of dews, the sources of which have been differently ascribed either to a concentration of the cooling atmosphere, or to a rise of vapour from the soil in a similar state of cooling. My own observations by glasses inverted upon the soil during the night season have been altogether in favour of the latter conclusion. They are uniformly wetted *internally* in quantities proportioned to the dew which appears upon the herbage, and its disappearance is accompanied with that of the wetness in the glasses even though they remain inverted on the soil. It is indeed a curious circumstance which I have proved by repeated observations that in a clear still atmosphere in the sun's absence when dew is forming, a corresponding wetness appears on the internal surface of any vessel whether of glass or earthenware which is inverted on the soil; and on the other hand when there is an opposite tendency in the atmosphere, when it inclines to the formation of clouds and rain, an opposite effect will appear in the inverted vessel, no moisture will arise in it from the soil, and if any had been previously formed in it, or is produced, as by breathing, it will presently disappear. Are not these phænomena ascribable to variations in the relative electricity in the soil and the atmosphere, co-operating probably with corresponding changes of temperature? In the sun's absence no evaporation proceeds from plants, the atmosphere cools and becomes less impregnated with electric matter, the soil retains a larger proportion of warmth and electricity it had imbibed in the preceding day, than the atmosphere and the moisture which by the sun's rays is drawn through the leaves, remaining also in the soil, combines with the warmth and electricity to form a rising vapour; the evaporation produces a coolness on the surface of the ground, which co-operating with the strong attraction of the young and almost exhausted germs and herbage condences the vapour, and thus a seasonable supply of electricity and moisture with an improved temperature is administered to these minute and tender productions. This appears to be the case when the atmosphere clears in the sun's absence and a transparent sky is seen, the particles of electricity and of moisture in its combining into pure aqueous gas, and leaving it in a dry and comparatively negative state; but when an opposite tendency prevails and the atmosphere becomes suffused with clouds and vapours, the caloric and electricity of these vapours being released from an opposite relation to the soil which is now comparatively cool and negatively electrical, and consequently in a condition to *absorb* the moisture and electricity of the condensing vapour. This condition of the soil imparts a corresponding state to the plants which extends to their upper extremities, and hence a general disposition both in the soil and plants to imbibe the combined vitality and nutriment which is thus imparted to them.

Fogs appear to have a somewhat different origin from either clouds or dews. They are chiefly prevalent in the winter and late autumn seasons; and seem to result from a condensation in the lower regions of the atmosphere by the action of cold in the sun's absence, or in part from the imperfect action of his few and languid rays in raising vapour from a previously moistened soil. The plants then in a state of vevetation being evergreen shrubs and quickset hedges, chiefly, the moisture attaches to them in large quantities, and seems to furnish them with a supply of nutriment in its union with electric matter that greatly conduces to their support and progress, at seasons when the more active species are nearly dormant.

LXVI. *Extract from the Instructions for the Scientific Expedition to the Antarctic Regions, prepared by the President and Council of the Royal Society*[*].

PHYSICS AND METEOROLOGY.

The council of the royal society are very strongly impressed with the number and importance of the desiderata in physical and meteorological science, which may wholly or in part be supplied by observations made under such highly favourable and encouraging circumstances as those afforded by the liberality of her majesty's government on this occasion. While they wish therefore to omit nothing in their enumeration of those objects which appear to them deserving of attentive inquiry on sound scientific grounds, and from which consequences may be drawn of real importance, either for the settlement of disputed questions, or for the advancement of knowledge in any of its branches,—they deem it equally their duty to omit or pass lightly over several points which, although not without a certain degree of interest, may yet be regarded in the present state of science rather as matters of abstract curiosity than as affording data for strict reasoning; as well as others, which may be equally well or better elucidated by inquiries instituted at home and at leasure.

---

[*] The President and Council having been informed by the Lords Commissioners of the Admiralty that it had been determined, in conformity with their recommendation, to send out captain James C. Ross on an Antarctic Expedition for scientific objects, and having been requested to communicate any suggestions upon subjects to which they might wish his attention to be called, referred the consideration of each to distinct Committees, namely, those of Physics, Meteorology, Geology, Botany, and Zoology, the result of whose labours is the Report from which the above is an extract.—Ed.

TERRESTRIAL MAGNETISM.

The subject of most importance, beyond all question, to which the attention of Captain James Clark Ross and his officers can be turned,—and that which must be considered as, in an emphatic manner, the great scientific object of the expedition,—is that of Terrestrial Magnetism; and this will be considered: Ist, as regards those accessions to our knowledge which may be supplied by observations to be made during the progress of the expedition, independently of any concert with or co-operation of other observers; and 2ndly, as regards thosewhich depend on and require such concert; and are therefore to be considered with reference to the observations about to be carried on simultaneously in the fixed magnetic observatories, ordered to be established by Her Majesty's Government with this especial view, and in the other similar observatories, both public and private, in Europe, India, and elsewhere, with which it is intended to open and maintain a correspondence.

Now it may be observed, that these two classes of observations naturally refer themselves to two chief branches into which the science of terrestrial magnetism in its present state subdivides itself, and which bear a certain analogy to the theories of the elliptic movements of the planets, and of their periodical and secular perturbations. The first comprehends the actual distribution of magnetic influence over the globe, at the present epoch, in its mean to average state, when the effects of temporary fluctuations are either neglected or eliminated by extending the observations over a sufficient time to neutralize their effects, The other comprises the history of all that is not permanent in the phænomena, whether it appear in the form of momentary, daily, monthly, or annual change and restoration, or in progressive changes not compensated by counter changes, but going on continually accumulating in one direction, so as in the course of many years to alter the mean amount of the quantities observed. These last-mentioned changes hold the same place, in the analogy alluded to, with respect to the mean quantities and temporary fluctuations, that the secular variations in the planetary movements must be regarded as holding, with respect to their mean orbits on the one hand, and their perturbations of brief period on the other.

There is, however, this difference, that in the planetary theory all these varieties of effect have been satisfactorily traced up to a single cause, whereas in that of terrestrial magnetism this is so far from being demonstrably the case, that the contrary is not destitute of considerable probability. In fact, the great features of the magnetic curves, and their general displacements and changes of form over the whole surface of the earth, would seem to be the result of causes acting in the interior of the earth, and pervading its whole mass; while the annual and diurnal variations of the needle, with their train of subordinate periodical movements, may, and very probably do arise from, and correspond to electric currents produced by periodical variations of temperature at its surface, due to the sun's position above the horizon, or in the ecliptic, modified by local causes; while local or temporary electric discharges, due to the thermic, chemical, or mechanical causes, acting in the higher regions of the atmosphere, and relieving themselves irregularly or at intervals, may serve to render account of those unceasing, as they seem to us casual movements, which recent observations have placed in so conspicuous and interesting a light. The electrodynamic theory, which refers all magnetism to electric currents, is silent as to the causes of those currents, which may be various, and which only the analysis of their effects can teach us to regard as internal, superficial, or atmospheric.

It is not merely for the use of navigators that charts, giving a general view of the lines of magnetic declination, inclination, and intensity, are necessary. Such charts, could they really be depended on, and where they in any degree complete, would be of the most eminent use to the theoretical inquirer, not only as general directions in the choice of empirical formulæ, but as powerful instruments for facilitating numerical investigation, by the choice they afford of data favourably arranged; and above all, as affording decidedly the best means of comparing any given theory with observation. In fact, upon the whole, the readiest, and beyond comparison the fairest and most effectual mode of testing the numerical applicability of a theory of terrestrial magnetism, would be, not servilely to calculate its results for given localities, however numerous, and thereby load its apparent errors with the real errors, both of observation and local magnetism, but to compare the totality of the lines in our charts with the corresponding lines, as they result from the formulæ to be tested, when their general agreement or disagreement will not only show how far the latter truly represent the facts, but will furnish distinct indications of the modifications they require.

Unfortunately for the progress of our theories, however, we are yet very far from possessing charts even of that one element, the Declination, most useful to the navigator, which satisfy these requisites; while as respects the others (the Inclination and Intensity) the most lamentable deficiences occur, especially in the Antarctic regions. To make good these deficiencies by the continual practice of every mode of observation appropriate to the circumstances in which the observer is placed throughout the voyage, will be one of the great objects to which attention must be directed. And first—

At sea.—We are not to expect from magnetic observations made at sea the precision of which they are susceptible on land. Nevertheless, it has been ascertained that not only the Declination, but the Inclination and Intensity can be observed, in moderate circumstances of weather and sea, with sufficient correctness, to afford most useful and valuable information, if patience be bestowed, and proper precautions adopted. The total intensity, it is ascertained, can be measured' with some considerable degree of certainty by the adoption of a statical method of observation recently devised by Mr. Fox, whose instrument will be a part of the apparatus provided. And when it is recollected that but for such observations the whole of that part of the globe which is covered by the ocean must remain for ever a blank in our charts, it will be needless further to insist on the necessity of making a daily series of magnetic observations, in all the three particulars above-mentioned, whenever weather and sea will permit, an essential feature in the business of the voyage, in both ships. Magnetic observations at sea will, of course be affected by the ship's magnetism, and this must be eliminated to obtain results of any service. To this end, First. Every series of observations made on board should be accompanied with a notice of the direction by compass of the ship's head at the time.

Secondly. Previous to sailing, a very careful series of the apparent deviations, as shown by two compasses permanently fixed, (the one as usual, the other in a convenient position, considerably more forward in the ship,) in every position of the ship's head, as compared with the real position of the ship, should be made and recorded, with a view to attempt procuring the constants of the ship's action according to M. Poisson's theory*; and this process should be repeated on one or more convenient occasions during the voyage; and, generally, while at anchor, every opportunity should be taken of swinging round the ship's head to the four cardinal points, and executing in each position a complete series of the usual observations.

Thirdly. Wherever magnetic instruments are landed and observations made on *terra firma*, or on ice, the opportunity should be seized of going through the regular series on shipboard with more than usual diligence and care, so as to establish by actual experiment in the only unexceptionable manner the nature and amount of the corrections due to the ship's action for that particular geographical position, and by the assemblage of all such observations to afford data for concluding them in general.

Fourthly. No change possible to be avoided should be made in the disposition of considerable masses of iron in the ships during the whole voyage; but if such change be necessary, it should be noted.

Fifthly. When crossing the magnetic line of no dip it would be desirable to go through the observations for the dip with the instrument successively placed in a series of different magnetic azimuths, by which the influence of the ship's magnetism in a vertical direction will be placed in evidence.

On land, or on ice.—As the completeness and excellence of the instruments with which the Expedition will be furnished will authorise the utmost confidence in the results obtained by Captain Ross's well known scrupulosity and exactness in their use, the redetermination of the magnetic elements at points where they are already considered as ascertained, will be scarcely less desirable than their original determination at stations where they have never before been observed. This is the more to be insisted on, as lapse of time changes these elements in some cases with considerable rapidity; and it is therefore of great consequence that observations to be compared should be as nearly contemporary as possible, and that data should be obtained for eliminating the effects of secular variations during short intervals of time, so as to enable us to reduce the observations of a series to a common epoch.

---

* See Appendix A.

On the other hand it cannot be too strongly recommended, studiously to seek every opportunity of landing on points (magnetically speaking) unknown, and determining the elements of those points with all possible precision. Nor should it be neglected, whenever the slightest room for doubt subsists, to determine at the same time the geographical position of the stations of observation in latitude and longitude. When the observations are made on ice, it is needless to remark that this will be universally necessary.

With this general recommendation it will be unnecessary to enumerate particular localities. In fact, it is impossible to accumulate too many. Nor can it be doubted that in the course of antarctic exploration, many hitherto undiscovered points of land will be encountered, each of which will, of course, become available as a magnetic station, according to its accessibility and convenience.

There are certain points in the regions about to be traversed in this voyage which offer great and especial interest in a magnetic point of view. These are, first, the south magnetic pole (or poles), intending thereby the point or points in which the horizontal intensity vanishes and the needle tends virtically downwards; and secondly, the points of maximum intensity, which, to prevent the confusion arising from a double use of the word poles, we may provisionally term magnetic *foci*.

It is not to be supposed that Captain Ross, having already signalized himself by attaining the northern magnetic pole, should require any exhortation to induce him to use his indeavours to reach the southern. On the contrary, it might better become us to suggest for his consideration, that no scientific datum of this description, nor any attempt to attain very high southern lattitudes, can be deemed important enough to be made a ground for exposing to *extraordinary* risk the lives of brave and valuable men. The magnetic pole, though not attained will yet be pointed to by distinct and unequivocal indications; viz. by the approximation of the dip to 90°; and by the convergence of the magnetic meridians on all sides towards it. If such convergence be observed over any considerable region, the place of the pole may hence be deduced, though its locality may be inaccessible.

M. Gauss, from theoretical considerations, has recently assigned a probable position in lon. 146° E., lat. 66° S., to the southern magnetic pole, denying the existence of two poles of the same name, in either hemisphere, 'which, as he justly remarks, would entail the necessity of admitting also a third point, having some of the chief characters of such a pole intermediate between them.' That this is so, may be made obvious without following out his somewhat intricate demonstration, by simply considering, that if a needle be transported from one such pole to another of the same name, it will *begin* to deviate from perpendicularity *towards* the pole it has quitted, and will end in attaining perpendicularity again, after pointing in the latter part of its progress obliquely *towards the pole to which it is moving*, a sequence of things impossible without an intermediate passage through the perpendicular direction.

It is not improbable that the point indicated by M. Gauss will prove accessible; at all events it cannot but be approachable sufficiently near to test by the convergence of meridians the truth of the indication; and as his theory gives within very moderate limits of error the true place of the northern pole, and otherwise represents the magnetic elements in every explored region with considerable approximation, it is but reasonable to recommend this as a distinct point to be decided in Captain Ross's voyages. Should the decision be in the negative, i. e. should none of the indications characterizing the near vicinity of the magnetic pole occur in that region, it will be to be sought; and a knowledge of its real locality will be one of the distinct scientific results which may be confidently hoped from this Expedition, and which can only be attained by circumnavigating the antarctic pole compass in hand.

The actual attainment of a *focus* of maximum intensity is rendered difficult by the want of some distinct character by which it can be known, previous to trial, in which direction to proceed, when after increasing to a certain point the intensity begins again to diminish. The best rule to be given, would be (supposing circumstances would permit it) on perceiving the intensity to have become nearly stationary in its amount, to turn short and pursue a course at right angles to that just before followed, when a change could not fail to occur, and indicate by its direction towards which side the focus in question were situated.

Another, and as it would appear, a better mode of conducting such a research, would be, when in the presumed neighbourhood of a focus of maximum intensity, to run down two parallels of latitude or two arcs of meridians separated by an interval of moderate extent, observing all the way in each, by which observations, when compared, the concavities of the isodynamic lines would become apparent, and perpendiculars to the chords, intersecting in or near the foci, might be drawn.

Two foci or points of maximum *total* intensity are indicated by the general course of the lines in Major Sabine's chart in the Southern Hemisphere, one about long. 140° E., lat. 47° S., the other more obscurely in long. 235° E., lat. 60° S., or thereabouts. Both these points are certainly accessible; and as the course of the Expedition will lead not far from each of them, they might be visited with advantage by a course calculated to lead directly across the isodynamic ovals surrounding them.

Pursuing the course of the isodynamic lines in the chart above mentioned, it appears that one of the two points of *minimum* total intensity, which must exist, if that chart be correct, may be looked for nearly about lat. 25° S., long. 12° W., and that the intensity at that point is probably the least which occurs over the whole globe. Now this point does not lie much out of the direct course usually pursued by vessels going to the Cape. It would therefore appear desirable to pass directly over it, were it only for the sake of determining by direct measure the least magnetic intensity at present existing on the earth, an element not unlikely to prove of importance in the further progress of theoretical investigation, Excellent opportunities will be afforded for the investigation of all these points, and for making out the true form of the isodynamic ovals of the South Atlantic, both in beating up for St, Helena, and in the passage from thence to the Cape; in the course of which, the point of least intensity will, almost of necessity, have to be crossed, or at least approached very near.

Nor is the theoretical line indicated by Gauss, as dividing the northern and southern regions, in which free magnetism may be regarded as superficially distributed, undeserving of attention. That line cuts the equator in 6° east longitude, being inclined thereto (supposing it a great circle) 15°, by which quantity it recedes from the equator northward in going towards the west of the point of intersection. Observations made at points lying in the course of this line may hereafter prove to possess a value not at present contemplated.

As a theoretical datum, the horizontal intensity has been recommended by Gauss, in preference to the total, not only as being concluded from observations susceptible of great precision, but as affording immediate facilities for calculation. As it cannot now be long before the desideratum of a chart of the horizontal intensity is supplied, the maxima and minima of this element may also deserve especial inquiry, and may be ascertained in the manner above pointed out.

The maxima of horizontal intensity are at present undetermined by any direct observation. They must of necessity, however, lie in lower magnetic latitudes than those of the total intensity, as its minima must in higher; and from such imperfect means as we have of judging, the conjectural situations of the maxima may be stated as occurring in

20° N.	80° E.	I.
7 N.	260 E.	II.
3 S.	130 E.	III.
10 S.	180 E.	IV.

Observations have been made of the horizontal intensity it the vicinities of II, and III., and are decidedly the highest which have been observed anywhere.

In general, in the choice of stations for determining the absolute values of the three magnetic elements, it should be borne in mind, that the value of each new station is directly proportional to its remoteness from those already known. Should any doubt arise, therefore, as to the greater or less elegibility of particular points, a reference to the existing magnetic maps and charts, by showing where the known points of observation are most sparingly distributed, will decide it.

For such magnetic determinations as those above contemplated, the instruments hitherto in ordinary use, with the addition of Mr. Fox's apparatus for the statical determination of the intensity, will suffice; the number of the sea observations compensating for their possible want of exactness. The determinations which belong to the second branch of our subject,—viz. those of the diurnal and other periodical variations, and of the momentary fluctuations of the magnetic forces,—require, in the present state of our knowledge, the use of those more refined instruments recently introduced. Being comparative rather than absolute, they depend in great measure (and as regards the momentary changes, wholly) on combined and simultaneous observation.

The variations to which the earth's magnetic force is subject, at a given place, may be classed under three heads, namely, 1. the *irregular* variations, or those which *apparently* observe no law; 2. the *periodical* variations whose amount is a function of the *hour* of the day, or of the *season* of the year; and, 3. the *secular* variations, which are either slowly progressive, or else return to their former values in periods of very great and unknown magnitude.

The recent discoveries connected with the *irregular* variations of the magnetic declination, have given to this class of changes a prominent interest. In the year 1818 M. Arago made, at the Observatory of Paris, a valuable and extensive series of observations on the declination changes; and M. Kupffer having about the same time undertaken a similar research at Cazan, a comparison of the results led to the discovery that the perturbations of the needle were *synchronous* at the two places, although these places differed from one another by more than forty-seven degrees of longitude. This seems to have been the first recognition of a phenomenon, which now, in the hands of Gauss and those who are labouring with him, appears likely to receive a full elucidation.

To pursue this phenomenon successfully, and to promote in other directions the theory of terrestrial magnetism, it was necessary to extend and vary the stations of observation, and to adopt at all a common plan. Such a system of simultaneous observations was organized by Von Humboldt in the year 1827. Magnetic stations were established at Berlin and Freyberg; and the Imperial Academy of Russia entering with zeal into the project, the chain of stations was carried over the whole of that colossal empire. Magnetic *houses* were erected at Petersburg and at Cazan; and magnetic instruments were placed, and regular observations commenced, at Moscow, at Sitka, at Nicolajeff in the Crimea, at Barnaoul and Nertschinsk in Siberia, and even at Pekin. The plan of observation was definitely organized in 1830; and simultaneous observations were made seven times in the year, at intervals of an hour for the space of forty-four hours.

In 1834 the illustrious Gauss turned his attention to the subject of terrestrial magnetism; and having contrived instruments which were capable of yielding results of an accuracy before unthought of in magnetic researches, he proceeded to inquire into the simultaneous movements of the horizontal needle at distant places. At the very outset of his inquiry he discovered the fact, that the synchronism of the perturbations was not confined (as had been hitherto imagined) to the larger and extraordinary changes; but that even the minutest deviation at one place of observation had its counterpart at the other. Gauss was thus led to organize a plan of simultaneous observations, not at intervals of an hour, but at the short intervals of five minutes. These were carried on through twenty-four hours six* times in the year; and magnetic stations taking part in the system were established at

---

* Recently reduced to *four*.

Altona, Augsburg, Berlin, Bonn, Brunswick, Breda, Breslau, Cassel, Copenhagen, Dublin, Freyberg, Gottingen, Greenwich, Halle, Kazan, Cracow, Leipsic, Milan, Marburg, Munich, Naples, St. Petersburg, and Upsala.

Extensive as this plan appears, there is much yet remaining to be accomplished. The stations, numerous as they are, embrace but a small portion of the earth's surface; and what is of yet more importance, none of them are situated in the neighbourhood of those *singular points* or curves on the earth's surface, where the *magnitude* of the changes may be expected to be excessive, and perhaps even their *direction* inverted. In short, a wider system of observation is required to determine whether the amount of the changes (which is found to be very different in different places) is dependent simply on the *geographical* or on the *magnetic* co-ordinates of the place; whether, in fact, the variation in that amount be due to the greater or less distance of a disturbing centre, or to the modifying effect of the mean magnetic force of the place, or to both causes acting conjointly. In another respect also, the plan of the simultaneous observations admits of a greater extension. Until lately the movements observed have been only those of the magnetic *declination*, although there can be no doubt that the *inclination* and the *intensity* are subject to similar perturbations. Recently, at many of the German stations, the *horizontal component* of the intensity has been observed, as well as the declination; but the determination of another element is yet required, before we are possessed of all the data necessary in this most interesting research.

The magnetic observations about to be established in the British Colonies. by the liberality of the Government, will (it is hoped) supply in a great measure these desiderata. The stations are widely scattered over the earth's surface, and are situated at points of prominent interest with regard to the Isodynamic and Isoclinal lines. The point of maximum intensity in the northern hemisphere is *in* Canada; the corresponding maximum in the southern hemisphere is *near* Van Diemen's Land; St. Helena is close to the line of *minimum intensity*; and the Cape of Good Hope is of importance on account of its southern latitude. At each observatory the changes of the *vertical component* of the magnetic force will be observed, as well as those of the *horizontal component* and *declination;* and the variations of the two components of the force being known, those of the *inclination* and of the *force* itself are readily deduced. The simultaneous observations of these three elements will be made at numerous and stated periods, and we have every reason to hope that the directors of the various European observatories will take part in the combined system.

But interesting as these phenomena are, they form but a small part of the proper business of an observatory. The *regular* changes (both periodic and secular) are no less important than the irregular; and they are certainly those by which a patient inductive inquirer would seek to ascend to general laws. Even the empirical expression of these laws cannot fail to be of the utmost value, as furnishing a correction to the absolute values of the magnetic elements, and thereby reducing them to their mean amount.

The hourly changes of the *declination* have been frequently and attentively observed; but with respect to the periodical variations of the other two elements, our information is as yet very scanty. The determination of these varations will form an important part of the duty of the magnetic observatories; and from the accuracy of which the observations are susceptible, and the extent which it is proposed to give them, there can be no doubt that a very exact knowledge of the empirical laws will be the result.

With respect to the *secular* variations, it might perhaps be doubted whether the limited time during which the observatories will be in operation is adequate to their determination. But it should be kept in mind that the monthly mean corresponding to each hour of observation will furnish a separate result; and that the number and accuracy of the results thus obtained may be such, as fully to compensate for the shortness of the interval through which they are followed. A beautiful example of such a result, deduced from three years' observation of the declination, is to be found in the first volume of Gauss's magnetical work, of which a translation is published in the fifth number of Taylor's Scientific Memoirs.*

It remains to say a few words of the instrumental means which have been adopted for the attainment of these ends.

The magnetic instruments belonging to each observatory and in constant use, are, 1. a declination instrument; 2. a horizontal force magnetometer; 3. a vertical force magnetometer. These instruments are constructed after the plan adopted by Professor Lloyd in the Magnetic Observatory of Dublin. The magnet, in the two former, is a heavy bar, fifteen inches long, and upwards of a pound in weight. In the declination instrument the magnet rests in the magnetic meridian, being suspended by fibres of silk without torsion. In the horizontal force magnetometer, the magnet is supported by two parallel wires, and maintained in a position at right angles to the magnetic meridian by the torsion of their upper extremities. In both instruments the changes of position of the magnet are read off by means of an attached collimater having a divided scale in its focus. The magnetometer for the vertical force is a bar resting by knife edges on agate planes, and capable of motion therefore in the vertical plane only. This bar is loaded, so as to rest in the horizontal position in the mean state of the force; and the deviations from that position are read off by micrometers near the two extremities of the bar.

In addition to these instruments, each observatory is furnished with a dip circle, a transit with an azimuth circle, and two chronometers. Each vessel also is supplied with a similar equipment. Should therefore the ships be under the necessity of wintering in the ice,—and generally, on every occasion when the nature of the service may render it necessary to pass a considerable interval of time in any port or anchorage,—the magnetometers should be established, and observations made with all the regularity of one of the fixed observatories, and with strict attention to all the same details.

The selection of proper stations for the erection of the magnetometers, and the extent of time which can be bestowed upon each, must in a great measure depend on circumstances, which can only be appreciated after the Expedition shall have sailed. The observatory at St. Helena (the officers and instruments for which will be landed by Captain Ross,) will in all probability,—and that at the Cape (similarly circumstanced in this respect) may possibly,—be in activity by the time the ships arrive at Kerguelen's Land; which we would recommend as a very interesting station for procuring a complete and as extensive a series of corresponding observations as the necessity of a speedy arrival at Van Diemen's Land for the establishment of the fixed observatory at that point will allow; taking into consideration the possibility of obtaining during the intermediate voyage, a similar series, at some point of the coast discovered by Kemp and Biscoe. In the ulterior prosecution of the voyage, a point of especial interest for the performance of similar observations will be found in New Zealand, which, according to the sketch of the voyage laid before us by Captain Ross, will probably be visited shortly after the establishment of the Van Diemen's Land observatory. The observations there will have especial interest, since, taken in conjunction with those simultaneously making in Van Diemen's Land, they will decide the important question, how far that exact correspondence of the momentary magnetic perturbations which has been observed in Europe, obtains in so remote a region, between places separated by a distance equal to that between the most widely distant European Stations.

In the interval between quitting Van Diemen's Land and returning to it again, opportunities will no doubt occur of performing more than one other series of magnetometer observations, the locality of which may be conveniently left to the judgment of Captain Ross, bearing in mind the advantage of observing at stations as remote as possible from both Van Diemen's Land and New Zealand.

The research for the southern magnetic pole and the exploration of the antarctic seas will afford, it may be presumed, many opportunities of instituting on land hitherto unknown, or on firm ice when the vessel may be for a time blockaded, observations of this description; and in the progress of the circumnavigation, the line of coast observed or supposed to exist under the name of Graham's Land, or those of the islands of that vicinity, South Shetland, Sandwich Land, and finally on the homeward voyage the Island of Tristan d'Acunha will afford stations each of its own particular interest.

A programme will be furnished of the days selected for simultaneous observations at the fixed observatories, and of the details to be attended to in the observations themselves as above alluded to. These days will include the *terms* or stated days of the German Magnetic Association, in which, by arrangements already existing, every European magnetic observatory is sure to be in full activity. These latter days, which occur four times in the year, will be especially interesting, as periods of magnetometrical observations by the Expedition, when the circumstances of the voyage will permit. For the determination of the existence and progress of the diurnal oscillation, in so far as that important element can be ascertained in periods of brief duration, it will be necessary to continue the observations hourly during the twenty-four for not less than one complete week. At every station where the magnetometers are observed, the absolute values of the dip, horizontal direction, and intensity will require to be ascertained.

Sydney, for a station of absolute determinations, would be with great propriety selected, as there can be no doubt of its becoming at no distant period a centre of reference for every species of local determination.

The meteorological particulars to be chiefly attended to, as a part of the magnetic observations, are those of the barometer, thermometer, wind, and especially auroras, if any. In case of the occurrence of the latter indeed, the hourly should at once be exchanged for uninterrupted observation, should that not be actually in operation. The affections of the magnetometers during thunder-storms, if any, should be noticed, though it is at present believed that they have no influence, During an earthquake in Siberia in 1829, the direction of the horizontal needle, carefully watched by M. Erman, was uninfluenced; should a similar opportunity occur, and circumstances permit, it should not be neglected.

Should land or secure ice be found in the neighbourhood of the magnetic pole, every attention will of course be paid to the procuring a complete and extensive series of magnetometric observations, which in such a locality would form one of the most remarkable results of the Expedition.

ELECTROMETERS.

The Council are fully impressed with the high importance, of regular observations on the electrical state of the atmosphere; but they are not prepared to suggest any means of effecting this desirable object, which will at all correspond with the present advanced state of electrical physics. At no distant period they hope to supply a defect which is certainly a reproach to science. In the meantime much valuable information might be acquired by observations of an electroscope, on one of the ordinary constructions connected with a lofty insulated wire.

In erecting such a wire, proper precautions should be taken against accidents by preparing a sufficient conductor in its immediate vicinity, by which a communication could be at once opened with the ground in case of any sudden and dangerous accumulation of the electric fluid.

As a temporary contrivance, a common jointed fishing-rod, having a glass stick well varnished with shell lac, substituted for its smallest joint, may be projected into the atmosphere. To the end of the glass must be fixed a metallic wire terminating in a point, and connected with an electroscope by means of a fine copper wire. If the wire be made to terminate in a spiral wrapped round a piece of cotton dipped in spirits of wine and inflamed, its power of collecting electricity will be sometimes doubled, but great precautions are necessary when this mode is employed. When the electroscope has been charged, the nature of the electricity may be tested in the usual way by excited glass or sealing wax.

The principal electroscopes which are capable of being employed to ascertain the electrical state of the atmosphere, or rather to compare its state at any given elevation with the state of the medium in contact with the instrument, are the following:

1. De Saussure's electrometer, which consists of two fine wires, each terminated by a small pith ball, and adapted to a

small metal rod fixed in the upper part of a square glass cover, upon one of the faces of which a divided scale is marked, in order to measure the angles of deviation of the two balls:

2. Volta's electrometer, formed of two straws about two inches long and ¼th of a line broad, suspended from two small very moveable rings adapted to metal rod: to measure the deviation of the straws a telescope with a nonius is employed:

3. Singer's electrometer, consisting of two slips of gold leaf suspended from the rod:

4. Bohnenberger's electroscope, formed of a single strip of gold leaf suspended from the conducting rod between two dry piles, the negative pole of one and the positive pole of the other being uppermost; this arrangement has the advantage of indicating the kind of electricity communicated to the conductor.

The observations made with these and similar instruments have demonstrated that in serene weather the electricity of the atmosphere is always positive with regard to that of the earth, and that it becomes more and more positive in proportion to its elevation above the earth's surface; so that if an observer be on a mountain or in a balloon, if his conductor be directed downwards to reach an inferior stratum of air, his electroscope will indicate negative electricity; and if it be sent upwards into a superior stratum, positive electricity will be manifested. Various means have been resorted to in these experiments, such as connecting one of the extremities of the conducting wire to a kite, a small balloon, or the head of an arrow, the other extremity remaining attached to the electroscope.

It has been ascertained by the observations of De Saussure, Schubler, Arago and others, that the positive electricity of the atmosphere is subject to diurnal variations of intensity, there being two maxima and two minima during the twenty-four hours. The first minimum takes place a little before the rising of the sun; as it rises, the intensity, at first gradually and then rapidly, increases, and arrives at its first maximum a few hours after. This excess diminishes at first rapidly and afterwards slowly, and arrives at its minimum some hours before sunset; it re-ascends when the sun approaches the horizon, and attains its second maximum a few hours after, then diminishes till sunrise, and proceeds in the order already indicated. The intensity of the free electricity of the atmosphere has also been found to undergo annual changes, increasing from the month of July to the month of November inclusive, so that the greatest intensity occurs in winter, and the least in summer.

In cloudy weather the free electricity of the atmosphere is still positive. During storms, or when it rains or snows, the electricity is sometimes positive and sometimes negative, and its intensity is always much more considerable than in serene weather. The electroscope will, during the continuance of a storm, frequently indicate several changes, from positive to negative.

The above is a short summary of almost all that is known respecting the laws of atmospheric electricity. It will be highly important to obtain a series of observations equal in accuracy to those made by Schubler at Frankfort in 1811 and 1812, simultaneously with the observations of the hygrometer, barometer, thermometer, &c. Combined observations at a number of different stations cannot fail to give us important information respecting the distribution of the free electricity in the atmosphere, and the extent and nature of the disturbances to which it is subject; but to render the results valuable it will be necessary to have instruments comparable with each other, and this may be a difficult matter to effect.*

Very recently a new method of investigating the electric state of the atmosphere has been proposed, likely to lead hereafter to very certain and valuable results; but it has not been sufficiently put in practice to enable the Council to recommend, at the present moment, the best form of instrument for making simultaneous and comparable observations, or the proper precautions to guide the observer in manipulating it.

For the principle of this instrument we are indebted to Mr. Colladon of Geneva. He found, that if the two ends of the wire of a galvanic multiplier, consisting of very numerous coils well insulated from each other, were brought in contact, one with a body positively, and the other with a body negatively charged, a current of electricity passes through the wire, until equilibrium is restored; the energy and direction of this current is indicated by the deviation of the needle from the zero-point of the scale. This instrument is applied to the purpose of ascertaining and measuring the atmospheric electricity, by communicating one end of the wire with the earth, and allowing the other to extend into the region of the atmosphere, the electrical state of which is intended to be compared.

---

* For a fuller account of what is known respecting atmospheric electricity, and the mode of conducting the observations, see Becquerel's *Traité de l'Electricité*, t. iv. pp. 78—125.

Thunder storms, of course, should be attended to; but it is of consequence also to notice distant lightning not accompanied with thunder audible at the place of observation, especially if it take place many days in succession, and to note the quarter of the horizon where it appears, and the extent which it embraces. In an actual thunder storm, especial notice should be taken of the quantity of rain which falls, and of the fits or intermittances of its fall, as corresponding, or not, to great bursts of lightning, as also of the direction of the wind, and the apparent progress of the storm with or against it.*

REGISTERS.

The Register proposed by the Council may be comprised in two skeleton forms, which have been supplied to the magnetical observatories and to the Expedition.

They are each calculated for one month's observation. *The first form* is for the insertion of observations as they are made in their uncorrected state. It consists of 12 principal divisions, and is ruled across for 31 days, and for the arithmetical convenience of casting up the sums and means of the quantities inserted. At the bottom of the sheet there is also a space provided for the hourly observations of the barometer and thermometers on *the twenty-first day of the month*, which will be more particularly described after the explanation of the principal divisions.

The outside compartments, both on the left and right of the sheet, are for the date of the month and the phases of the moon.

The second compartment is for the height of the barometer, and the temperature of the mercury for the four regular periods of observation.

The third compartment is appropriated to the dew-point hygrometer, and contains also four columns for the four daily observations, each of which is subdivided into three; for the temperature of the air, the dew-point, and the difference between the two.

The fourth compartment is for the wet-bulb hygrometer, and is similarly divided and subdivided for the temperature of the dry-and wet-bulb thermometer, and for their differences.

The fifth compartment is prepared for the maxima and minima of temperature, and is divided into three. In the first division are to be recorded the maxima and minima of thermometers carefully placed in the shade and screened from radiation. In the second, the maxima of a blackened thermometer exposed to the sun, and the minima of a thermometer placed in a metallic mirror, and radiating freely to the clear sky. The third is devoted to occasional observations of the actinometer under favourable circumstances.

---

* On these subjects the Council especially recommend the attentive perusal of Arago's *Notice sur le Tonnerre.*

The sixth compartment is for the temperature of the surface-water of the sea, or of any river in the immediate neighbourhood of the observatory.

The seventh compartment is prepared for observations upon the direction and force of the wind at the four regular hours of registry. In the left-hand column of each division is to be recorded the direction of the vane, and in the right-hand column the height of Lind's gauge, in tenths of an inch of water.

In the eighth compartment the amount of rain is to be registered once in the day; and in the ninth, the electrical state of the atmosphere, if possible, at the four periods, 3 A. M., 9 A. M., 3 P. M., and 9 P. M.

The tenth compartment is appropriated to remarks on the clouds, and weather generally; and in the eleventh is to be noted, at noon, the longitude and latitude at sea.

On a careful review of the month's observations, the maxima and minima results should have the algebraic signs + and — respectively affixed.

The second form is devoted to the corrected results of the observations, and to the optical comparison together of some of them, by their projection upon a scale of equal parts.

The upper half of the sheet is vertically divided into two equal parts, each prepared for half the month's observations, and accordingly ruled across into sixteen spaces for the daily observations, and two for the sums and means of the quantities. Each half is also divided into five compartments.

The first is for the date of the month and the phases of the moon.

The second for the corrected height of the barometer at 32° Fahr.

The third is appropriated to the elastic force of the aqueous vapour corresponding to the dew-point, and which may be taken from Table 5, in the Appendix B.

The fourth is for the maximum and minimum of temperature, and the mean of the two.

And the fifth for occasional remarks.

The lower half of the sheet is also vertically divided into two equal parts, each of which is similarly divided into 31 columns for the daily observations of a month; and these again subdivided into four, for the six-hourly observations of each day. The vertical lines thus formed are divided into 6 inches; and each inch into tenths of an inck, and half-tenths, by horizontal lines.

The left-hand compartment thus ruled, is intended for the projection of curves of temperature; for this purpose each tenth of an inch upon the scale must be reckoned a degree, which will be divided by the faint line into halves.

The value of the degree may be arbitrarily fixed, and inserted in the margin according to convenience. Towards the upper part of the scale the results of the six-hourly observations should each be marked by a dot in its appropriate space, and the dots may be afterwards connected by a line.

The temperatures of the dew-point, or of the wet-bulb thermometer, or the mean temperature, may be compared with this primary result by projecting their curves in a similar way beneath it; and should the observations of these points be less frequent than four times in the day, the daily spaces may easily be divided accordingly.

The right-hand compartment is appropriated to the projection of curves of pressure, and the four daily observations of the barometer are to be marked by dots towards the upper part of the scale of inches, and afterwards connected by a line. Towards the lower part of the scale the elastic force of the vapour is to be noted, and the marks to be similarly connected by a line.

On either the scale of temperature or of pressure, occasional comparisons may be made with results obtained at other stations, which, if judiciously selected, cannot fail to prove of high interest and importance. They should, however, be laid down in pencil, or marked by a fainter line.

At the bottom of the first skeleton form will be found a space prepared for the 24 hourly observations of the *twenty-first day* of the month, both in their uncorrected and their corrected state. It is divided into four compartments for 6 hours each. The instruments which can with most facility be observed in this manner, are the barometer with its attached thermometer, and the dry-and wet-bulb thermometers; and columns are appropriated to each of these. It is desirable that the mean of each 6 hour should be calculated, and spaces have been provided accordingly for the arithmetical operations.

In casting up the sums and calculating the *means*, care should be taken in all cases to verify the results by repetition;

and the Council recommend in every instance, before adding up the columns, to look down each to see that no obvious error of entry (as of an inch in the barometer, a very common error) may remain to vitiate the mean result. The precaution should also be taken of counting the days in each column, so as to make no mistake in the divisor.

The skeleton forms will be interleaved with blank pages, to facilitate computations and comparisons, and to afford space for other observations of atmospheric phenomena, which will perpetually present themselves to those who make it their business or their pleasure to watch the changes of the weather on a judicious plan. The Council, indeed, wish it to be understood, that, in the suggestions which they have offered, they have taken into consideration only such observations as are indispensable for laying the first foundations of meteorological science; some investigations of a more refined character they may, probably, make the subject of a future report.

As soon as the register of a month's observations has been computed, it should be copied, and the copy carefully compared with the original by two persons, one reading aloud from the original, and the other attending to the copy, and then exchanging parts,—a process always advisable whenever great masses of figures are required to be correctly copied.

A copy so verified should be transmitted regularly to such person or public body, as, under the circumstances, may be authorized or best adapted to receive and discuss the observations.

ACCOUNT OF THE MAGNETICAL INSTRUMENTS EMPLOYED AND OF THE MODE OF OBSERVATION TO BE ADOPTED, IN THE MAGNETICAL OBSERVATORIES ABOUT TO BE ESTABLISHED BY HER MAJESTY'S GOVERNMENT.

The elements on which the determination of the earth's magnetic force is usually based are, the *declination*. the *inclination*, and the *intensity*. If a vertical plane be conceived to pass through the direction of the force, that direction will be determined when its inclination to the horizon is given, as well as the angle which the plane itself forms with the meridian; and if, in addition to these quantities, we likewise know the number which expresses the ratio of the intensity of the force to some established unit, it is manifest that the force is completely determined.

For many purposes, however, and especially in the delicate researches connected with the *variations* of the magnetic force, a different system of elements is preferable. The intensity being resolved into two portions in the plane of the magnetic meridian, one of them *horizontal* and the other *vertical*, it is manifest that these two components may be substituted for the total intensity and the inclination; while, at the same time, their changes may be determined with far greater precision. The former variables are connected with the latter by the relations

$$X = R \cos \theta, \quad Y = R \sin \theta;$$

in which R denotes the intensity, X and Y its horizontal and vertical components, and θ the inclination; and the variations of θ and R are expressed in terms of the variations of X and Y by the formulæ:

$$d\theta = \tfrac{1}{2} \sin 2\theta \left(\frac{dY}{Y} - \frac{dX}{X} \right):$$

$$\frac{dR}{R} = \cos^2\theta \frac{dX}{X} + \sin^2\theta \frac{dY}{Y}.$$

As the instruments destined for the observation of these elements (with a set of which each observatory is furnished) are, for the most part, novel in form, it will be useful to give a somewhat detailed account of their construction and various adjustments, before entering on the plan of observation to be pursued.

DECLINATION MAGNETOMETER.

Construction.—The essential part of the declination magnetometer is a magnet bar, suspended by fibres of untwisted silk, and inclosed in a box, to protect it from the agitation of the air. The bar is a rectangular parallelopiped, 15 inches in length, ⅞ths of an inch in breadth, and ¼th of an inch in thickness. In addition to the stirrup by which the bar is suspended it is furnished with two sliding pieces, one near each end. One of these pieces contains an achromatic lens, and the other a finely divided scale of glass; the scale being adjusted to the focus of the lens, it is manifest that the apparatus forms a moving collimator, and that its absolute position at any instant, as well as its changes of position from one instant to another, may be read off by a telescope at a distance. The aperture of the lens of this collimator is 1¼ inch, and its focal length about 12 inches. Each division of the scale is $\frac{1}{385}$th part of an inch; and the corresponding angular quantity is about 43 seconds.

To the suspension thread is attached a small cylindrical bar, the ends of which are of smaller diameter, and support the stirrup which carries the magnet. The apertures in the stirrup, by which it hangs on the cylinder, are of the form of inverted Y's, so that the bearing points are invariable. A second pair of apertures at the other side of the magnet, serves for the purpose of *inversal;* and care has been taken to render the lines connecting the bearing points of each pair of Y's parallel, so that there may be no difference in the amount of torsion of the thread in the two positions of the stirrup. The two pairs of apertures are at different distances from the magnet, in order that the line of collimation may remain nearly at the same height on inversal, and thus it may not be necessary to alter the length of the suspension thread. The stirrup, and the other sliding pieces, are formed of gun metal.

For the purpose of taking out the torsion of the suspension thread, the apparatus is furnished with a *detorsion bar,* which (with its appendages) is of the same weight as the magnet. It is a rectangular bar of gun-metal, furnished with a stirrup and collimator similar to those of the magnet. A rectangular aperture in the middle receives a small magnet, the use of which is to impart a slight directive force to the suspended bar, and without which the final adjustment of detorsion would be tedious and difficult.

The frame-work of the instrument consists of two pillars of copper, 35 inches in height, firmly screwed to a massive marble base. These pillars are connected by two cross pieces of wood, one at the top, and the other 7 inches from the bottom. In the centre of the top piece is the suspension apparatus, and a divided circle used in determining the amount of torsion of the thread. A glass tube (between this and the middle of the lower cross piece) encloses the suspension thread; and a glass cap at top covers the suspension apparatus, and completes the enclosure of the instrument.

The box is cylindrical, its dimensions being 20 inches in diameter by 7 inches in depth. It rests upon the marble slab, and encompasses the pillars; and it is so contrived as to be raised, when necessary, for the purpose of manipulation. There are two apertures in the box, opposite to each other. The aperture in front, used for reading, is covered with a circular piece of parallel glass, attached to a rectangular frame of wood which moves in dovetails; the prismatic error of the glass (if any) is corrected by simply reversing the slider in the dovetails. The opposite aperture is for the illumination of the scale.

In addition to the parts abovementioned, the instrument is provided with a second magnet, of the same dimensions as the first, to be used in measurements of absolute intensity; a thermometer, the bulb of which enters the box, in order to determine the interior temperature; and a copper ring, for the purpose of checking the vibrations.

Adjustment.—The instrument having been placed on its support, the base is to be levelled, and the whole then fixed in its place. The levelling of the base may conveniently be performed by the aid of a plumb-line hanging in the place of the suspension thread; but no great precision is required in this operation, the chief object of which is that the suspension thread may occupy the middle of the tube, and that the magnet may be central with regard to its support. The suspension thread is then to be formed, and attached at one extremity to the roller of the suspension apparatus, and at the other to the small cylinder which is to bear the stirrup and magnet. Sixteen fibres* of untwisted silk are sufficient to bear double the load without breaking, and will be found to form in other respects a convenient suspension.

These preparations being made, the adjustments are the following:

1. The sliders being placed on the magnet, the scale is to be adjusted to the focus of the lens, and in such a manner that the centre of gravity of the sliders may be near the middle of the bar. The adjustment to focus has been already made by the artist, and the corresponding distances of the sliders measured; they will be found in Table 1.

2. The magnet is to be connected with the suspension thread by means of the stirrup, and to be moved in the stirrup until it assumes the horizontal position. This adjustment may be conveniently effected by means of the image of the magnet, reflected from the surface of water or mercury, the object and its reflected image being parallel when the former is horizontal. The stirrup is then fastened by its screws, and the magnet wound up to the desired height. As the thread stretches considerably at first, allowance should be made for this in the height.

3.† The magnet is then removed, and the unmagnetic bar

---

* Not the individual fibre of the silk-worm, but the compound fibre in the state in which it is prepared for spinning.

† It is obvious that this step of the adjustment may precede the 1st and 2nd, where a saving of time is important.

(having its collimator similarly adjusted) is to be attached, without its small magnet, and allowed to swing for several hours. The bar having come to rest, or nearly so, its deviation from the magnetic meridian is to be *estimated*, and the moveable arm of the torsion circle turned through the same angle in an opposite direction. The plane of detorsion then coincides, approximately, with the magnetic meridian.

4. The magnet is then to be substituted for the unmagnetic bar, and the telescope being directed towards the collimator, the point of the scale coinciding with the vertical wire is to be noted when the magnet is in the *direct* and *inverted* positions. Half the sum of these readings is the point of the scale corresponding to the magnetic axis of the magnet bar; and half their difference (converted into angular measure) is the deviation of the line of collimation of the telescope from the magnetic meridian. The telescope should be moved through this angle in the opposite direction.

5. In order to take out the remaining torsion of the thread, the magnet is again to be removed, and the unmagnetic bar (with its small magnet attached) substituted. The deviation of this bar from the magnetic meridian should then be read off on its divided scale, and the moveable arm of the torsion circle turned through a given angle in the opposite direction. The deviation being again read, a simple proportion will give the remaining angle of torsion; and the moveable arm being turned through this angle in the opposite direction, another observation will serve to verify the adjustment. The plane of detorsion then coincides with the magnetic meridian; and the magnet being replaced, the instrument is ready for use.

Observations.—The observations to be made with this instrument are, 1. of the *absolute declination;* 2. of the *variations of the declination;* and 3. of the *absolute intensity.*

For measurements of the *absolute declination* each observatory is furnished with a small transit instrument having an azimuth circle. This instrument being placed in the magnetic meridian of the declination instrument, the point of the scale coinciding with the central wire of the transit telescope is to be observed; the interval between this point and the point* corresponding to the magnetic axis of the bar, converted into angular measure, is the deviation (δ) of the line of collimation of the transit telescope from the magnetic meridian. The verniers of the horizontal circle being then read, the telescope is turned, and its central wire made to bisect a distant mark, whose azimuth (α) has been accurately determined. If a denote the angle read off on the horizontal circle, it is manifest that the angle between the magnetic and the astronomical meridians is

---

* In determining this point by the mean of two readings of the scale with the bar erect and inverted, care must be taken to eliminate the declination changes which may occur in the interval of the two parts of the observation. The horizontal force magnetometer may be applied to the purpose of this elimination. But perhaps the simplest course is to take a *series* of readings as rapidly as possible, alternately in the two positions of the bar, choosing

$$a + \alpha + \delta,$$

α and δ being affected with their proper signs. The angle a is supposed to have been previously determined by the help of the transit instrument.

But instead of referring the transit telescope *directly* to the magnetic meridian by means of the moving collimator, the same result will be obtained, and probably in a better manner, by referring it to the line of collimation of the *fixed telescope*, with which the changes of the declination are regularly observed. For this purpose it is only necessary to employ the latter telescope as a collimator, the telescope being *reversed* in its Y supports, if necessary. A fixed collimator may also be conveniently substituted for the distant mark. This mode of observation has the advantage of connecting the absolute determination directly with the regular series of observations; and it is manifest that it is sufficient, without any other means, to determine whether any, and what changes may have occurred in the position of the fixed telescope.

The fixed telescopes, furnished to each observatory, have an aperture of $1\frac{5}{8}$ inches, and focal length of 14 inches. They should be fixed upon a stone pillar, or upon a firm pedestal of wood resting on solid masonry unconnected with the floor.

In observing the *declination changes* the fixed telescope (above referred to) is alone employed. The observation consists simply in noting the point of the scale coinciding with the vertical wire, at three successive limits of the arc of vibration. The three readings being denoted by a, b, c, the mean point of the scale corresponding to the time of the middle observation is

$$\frac{1}{4}(a + 2b + c).$$

---

for the time of observation a period when the declination changes are slow and regular. By comparing each result with the mean of the preceding and subsequent, and then taking the mean of all these partial means, a very accurate determination may be obtained.

This mode of observation is sufficient where the observer is not limited to a *precise moment* of observation. Otherwise the more exact method pointed out by Gauss is to be preferred.

The changes of position of the scale may be converted into angular measure, the angle corresponding to one division, being known. In general, however, this reduction will only be required in the monthly mean results.

Before the true changes of the declination can be deduced from the observed readings, it is necessary to apply a correction depending upon the force of torsion of the suspension thread. For supposing that the plane of detorsion has been brought (by the adjustments above described) to coincide with the magnetic meridian, it is manifest that on every deviation of the magnet from, that, its mean position, the torsion force will be brought into play; and as this force tends to bring back the magnet to the mean position, the apparent deviations must be less than the true. The ratio of the torsion force, to the magnetic directive force, is experimentally determined by turning the moveable arm of the torsion circle through any given large angle (for example 90°), and observing the corresponding angle through which the magnet is deflected. Let u denote the latter angle, and v the former; then the ratio in question is,

$$\frac{G}{F} = \frac{u}{v - u};$$

in which G is the co-efficient of the torsion force, and F the moment arising from the action of the earth's magnetic force upon the free magnetism of the bar, the direction of the action being supposed to be perpendicular to its magnetic axis. The ratio of the two forces being thus found, the true declination changes are deduced from the apparent, by multiplying them by the co-efficient

$$1 + \frac{G}{F}.$$

In order to obtain an exact result by the mode of experiment above described, it is necessary that the *actual* changes of the declination which may occur in the interval of the two readings, should be eliminated. The obvious method of accomplishing this, is to observe the declination changes *simultaneously* with a second apparatus. If such means, however, should not be at hand, the object may be attained by making a *series* of readings with the vernier of the torsion circle alternately in two fixed positions (for example + 90° and — 90°); the mean result will be independent of the declination changes, provided the progress of these changes has been gradual in the interval of the experiment.

For the purpose of determining the *absolute intensity* of the horizontal component of the earth's magnetic force, the declination instrument is provided with a *deflecting bar*, and a *beam compass* to be used in measuring its distance from the suspended magnet. The mode of observation has been so fully explained by Gauss, in his valuable memoir entitled "*Intensitas vis terrestris ad mensuram absolutam revocata,*" and in the first volume of the "*Resultate,*" that it is unnecessary to enter here into any details.

The following table contains the interval of the sliders of the collimators, corresponding to focal adjustment; and also the arc values of one division of the scale in each instrument, expressed in decimals of a minute.

TABLE 1.

No. of Instrument.	Observatory.	Interval of Sliders.	Arc value of one division.
		inches.	′
I.	H. M. S. Erebus......	11·70	0·7267
II.	Van Diemen's Land	12·01	0·7085
III.	Montreal...............	11·72	0·7208
IV.	Cape of Good Hope	11·18	0·7525
V.	St. Helena......... ...	11·96	0·7408

HORIZONTAL FORCE MAGNETOMETER.

The instrument employed in determining the horizontal component of the earth's magnetic force is similar, in principle, to the "*bifilar magnetometer*" of Gauss. It is a magnet bar, suspended by two equi-distant wires, or (more accurately) by two portions of the same wire, the distance of whose bearing points is the same above and below; by the rotation of the upper extremities of the wire round their middle point, the magnet is maintained in a position at right angles to the magnetic meridian.

It is manifest from the nature of this suspension, that the *weight* of the suspended body will tend to bring it into the position in which the two portions of the wire are in the *same plane* throughout The moment of the directive force is G sin v;—v denoting the angle formed by the lines joining the bearing points above and below, or the deviation from the plane of detorsion; and G being expressed by the formula

$$G = w\frac{a^2}{l};$$

in which w denotes the weight of the suspended body, a half the interval of the wires, and l their length. The earth's *magnetic force*, on the other hand, tends to bring the magnetic axis of the bar into the magnetic meridian with the force F sin u; in which u is the deviation of the magnetic axis from the meridian, and F is the product of the horizontal part the earth's magnetic force into the moment of free magnetism of the bar. The magnet being thus acted on by two forces, will rest in the position in which their moments are equal When the instrument is so adjusted that $u = 90°$, or the magnet at right angles to the magnetic meridian,

$$F = G \sin v;$$

and the ratio of the forces is known, when we know the angle v. But as one of these forces is constant, and the other variable, it is evident that the place of the magnet will vary around its mean position, and that the variations of angle are connected with the variations of the force. This connexion is expressed by the formula

$$d\,F = F \operatorname{cotan} v \,.\, d\,u;$$

the angle $d\,u$ being expressed in parts of radius.

Construction.—The magnet bar is of the same dimensions as that of the declination instrument. The collimator, by which its changes of position are observed, is attached to the stirrup, and has a motion in azimuth. The suspending wire passes round a small grooved wheel, on the axis of which the stirrup rests by inverted Y's; and the instrument is furnished with a series of such wheels, whose diameters increase in arithmetical progression, (the common difference being about $\frac{1}{20}$th of an inch,) for the purpose of varying the interval of the wires. The exact intervals, corresponding to each separate wheel, have been determined by the artist by accurate micrometrical measurements; they are given in Table III. The same interval is altered, at the upper extremity, by means of two screws (one right-handed and the other left-handed) cut in the same cylinder; the wires being lodged in the intervals of the threads, and their distance regulated by a micrometer head. The interval of the threads of this screw (which is precisely the same for all the instruments) is $\frac{2}{77}$ths, or ·02597 of an inch. The micrometer head is divided into 100 parts; and, as one revolution of the head corresponds to *two* threads of the screw, a single division is equivalent to ·0005194, or the $\frac{1}{2000}$th of an inch nearly. The micrometer head has been carefully adjusted by the artist, so that the index is at zero, when the interval of the wires is exactly half an inch.

The collimator, in this instrument, is enclosed in a light tube attached to the stirrup. The aperture of the lens is about $\frac{8}{10}$ths of an inch, and its focal length about 8 inches. The divisions of the scale are the same as in the collimator of the declination magnetometer; the corresponding arc values have been ascertained for each instrument by accurate experiment, and are given in Table II.

The larger parts of this apparatus,—the box, the framework, and the support,—are precisely similar to those of the declination magnetometer. In addition to the parts already described, the instrument is furnished with a spare magnet; a brass weight, required in determining the plane of detorsion of the wires relatively to the magnetic meridian; a thermometer, the bulb of which is within the box, for the purpose of ascertaining the interior temperature; and a copper ring used in checking the vibrations.

Adjustments.—The instrument being placed on its support, the base is to be levelled, and the whole apparatus fixed. Having then selected one of the small grooved wheels, and fixed it, temporarily, with its axis horizontal, the wire is to be passed round it; and the free extremities of the wire being passed through the corresponding holes in the suspension roller, placed beneath, weights are to be attached, and the two portions of the wire allowed to assume their natural position; the extremities may then be *fastened* to the roller, by introducing small wooden plugs in the holes. The parts are then to be inverted, and put in their proper places; the suspension apparatus resting on the divided circle, and the wire hanging down the tube.

The collimator (its scale having been previously adjusted to focus[*]) is to be screwed on to the stirrup, and the latter attached to the axis of the grooved wheel by means of its Y's. The magnet is then introduced into the stirrup and

---

[†] This adjustment has been already made by the artist.

levelled; and the wires wound upon the roller, until the collimator is at the desired height.

These preparations being made, the adjustments are the following:

1, Determine experimentally the angle through which it is necessary to turn the moveable arm of the torsion circle, in order to deflect the magnet from the magnetic meridian to a position at right angles to it, the two positions being merely *estimated.* The cosine of this angle is, approximately, the ratio of the magnetic force to the torsion force, or the value of the fraction $\frac{F}{G}$. The nearer this ratio is to unity, the more delicate will be the instrument; practically, $\frac{9}{10}$ will be found a convenient value. If, on making the foregoing experiment, the ratio should be found to fall below, or to exceed the proper limits, the torson force must be altered by introducing a different wheel, and making the corresponding alteration in the interval of the upper extremities of the wires.

2. The magnetic axis being brought, approximately, into the magnetic meridian, by turning the moveable arm of the torsion circle, the collimator is to be turned, by its independent motion, until some point about the middle of the scale coincides with the vertical wire of the fixed telescope. This point of the scale is to be noted in the usual manner.

3. The magnet is then to be removed, and the brass weight attached. Note the new point of the scale which coincides with the wire of the telescope. Then, if the magnet had been placed (in the previous experiment) in its *direct* position (i. e. north to north) the* error of the plane of detorsion is

$$v\left(\frac{G}{F}+1\right).$$

v being the difference of the two readings, converted into angular measure. If, on the other hand, the magnet had been *reversed* (i. e. north end to south) the error is

$$v'\left(\frac{G}{F}-1\right).$$

The moveable arm of the torsion circle is then to be turned through this angle, in the opposite direction; and the magnetic axis will be in the magnetic meridian.

---

* The difference of the two readings, corresponding to a given error, being much greater in the reversed than in the

direct position of the magnet, it follows that the former affords a much more delicate method of making the desired adjustment.

4. The brass weight remaining attached, turn the moveable arm of the torsion circle through 90°. Then turn back the collimator, until some point about the middle of the scale coincides with the verticle wire of the fixed telescope; and note the reading.

5. Now remove the brass weight, and replace the magnet. The magnetic force of the earth will bring it back towards the magnetic meridian, and the scale will be thrown out of the field of the telescope. Then turn the moveable arm of the torsion circle until the point of the scale last noted is brought to coincide again with the wire of the telescope; the magnetic axis is then in the plane perpendicular to the magnetic meridian, and the adjustment is complete.

Observations.—The observations to be made with this instrument are those of the *absolute* value of the *horizontal intensity*, and its *changes*.

From the explanation of the principle of the instrument, given above, it is manifest that it will serve to determine the moment of the force exerted by the earth upon the free magnetism of the suspended bar. Let X denote (as before) the horizontal part of the earth's magnetic force; m the moment of free magnetism of the bar; then

$$m \mathrm{X} = \mathrm{F},$$

F having the same meaning as before (page 230.) Hence, substituting the values of F and G, we have

$$m \mathrm{X} = w \frac{a^2}{l} \sin v;$$

in which equation all the quantities of the second member may be obtained by direct measurement. The chief difficulty in this method consists in the determination of the quantity a, which should be known to a very small fractional part of its actual value. This difficulty has been overcome by the measuring apparatus connected with the suspension, which (as has been already stated) serves to determine the interval of the wires, at their upper extremity, to the $\frac{1}{2000}$th of an inch. The numbers given in Table III. for the lower interval, may be relied on to the same degree of accuracy. It is scarcely necessary to mention that the length of the wires, l, is to be measured between the points of contact above and below.

The *product* of the earth's magnetic force into the magnetic moment of the bar being thus known, the *ratio* of the same quantities is to be determined by removing the bar from the stirrup, and using it to *deflect* the suspended bar of the declination instrument, according to the known method devised by Gauss. The experiments of deflection may, however, be performed without the aid of the second magnetometer, by operating upon another bar placed in the *reverse* position. This method has even the advantage in point of delicacy; but it labours under the disadvantage of requiring that the value of $\frac{F}{G}$ should be determined for the second bar.

The chief use of this apparatus is in observing the *variations* of the intensity. In these observations it is only necessary to note, at any moment, the point of the scale coinciding with the vertical wire of the fixed telescope, the mode of observing being precisely the same as in the other instrument. Let n be the number of divisions, and parts of a division, by which the reading at any moment differs from its mean value; then the corresponding variation of the angle (in parts of radius) is

$$d u = n a;$$

a denoting the arc value (in parts of radius) corresponding to a single division. Substituting this in the formula of page 459, we have

$$\frac{dF}{F} = n a \cot v = k n;$$

k being the value of the constant co-efficient $a \cot v$. The values of a have been determined for each of the instruments, and are given in Table II.

The quantity F. in the preceding formula, is the product of the earth's magnetic force into the moment of free magnetism of the bar; and as the latter quantity varies with the temperature, it is necessary to apply a correction, before we can infer the true changes of the earth's force. This correction is easily deduced. Since $F = X m$, there is

$$\frac{dF}{F} = \frac{dX}{X} + \frac{dm}{m};$$

so that the correction to be applied, in order to deduce the value of $\frac{dX}{X}$, is $-\frac{dm}{m}$. Let t denote the temperature, in degrees of Fahrenheit; q the relative change of the magnetic moment corresponding to one degree; then

$$\frac{d\,m}{m} = q\,(t - 32).$$

Accordingly, the changes of the earth's force will be expressed by the formula

$$\frac{d\,X}{X} = k\,n + q\,(t - 32).$$

It is not necessary that these reductions should be applied to the individual results, except in cases of marked change, where it is desired to trace the progress of the actual phenomena. The results should be recorded as they are observed, in parts of the scale, and the reductions made in the monthly, or other mean values.

Table II. contains the arc values of one division of the scale, in each instrument, expressed in *decimals of a minute*; as also the same quantities reduced to *radius*, as the unit by multiplying by the number ·0002909.

Table III. contains the intervals of the axis of the wires corresponding to each wheel, in decimals of an inch; the wire used being that designated in commerce as "silver fine 6."

TABLE II.

No. of Instrument.	Observatory.	Arc values of one division.	
		In Minutes.	In parts of Radius.
I.	H.M.S. Erebus	1·075	·0003127
II.	VanDiemen'sLand	1·080	·0003142
III.	Montreal................	1·074	·0003124
IV.	Cape of GoodHope	1·084	·0003153
V.	St. Helena	1·080	·0003142

TABLE III.

No. of Wheel.	I. H.MS. Erebus.	II. Van Diemen's Land.	III. Montreal.	IV. Cape of Good Hope.	V. St. Helena.
1	·2536	·2549	·2529	·2542	·2536
2	·3032	·3058	·3055	·3055	·3065
3	·3529	·3516	·3529	·3497	·3513
4	·4058	·4088	·4078	·4052	·4071
5	·4562	·4555	·4581	·4555	·4545
6	·5055	·5071	·5042	·5055	·5058
7	·5555	·5604	·5588	·5565	·5591
8	·6071	·6071	·6071	·6097	·6081

VERTICAL FORCE MAGNETOMETER.

The instrument used in determining the changes of the *vertical component* of the magnetic force is a magnetic needle resting on agate planes, by knife edges, and brought to the horizontal position by weights. From the changes of position of such a needle, the changes of the vertical force may be inferred, when we know the mean inclination at the place of observation, the azimuth of the plane in which the needle moves, and the angle which the line connecting the centre of gravity and centre of motion makes with the magnetic axis. As, however, the determination of this latter constant would involve the necessity of considerable additions to the apparatus, the plan adopted has been to *adjust* the needle so that the angle in question shall be *nothing*. The centre of gravity being thus brought to some point of the magnetic axis, the changes of the vertical force are connected with the changes of the position of the needle by the formula.

$$\frac{\delta F}{F} = \cos a \,.\, \text{cotan} \,.\, \theta \, d\zeta ;$$

$d\zeta$ denoting the change of angle in parts of radius, a the *azimuth* of the plane in which the needle moves, and θ the *inclination*.

Construction.—The magnetic needle is 12 inches in length. It has a cross of wires at each extremity, attached by means of a small ring of copper; the interval of the crosses being 13 inches. The axis of the needle is formed at one part into a *knife edge*, and at the opposite into a portion of a *cylinder*, having this edge for its axis, the edge being adjusted to pass as nearly as possible through the centre of gravity of the unloaded instrument. The weights by which the other adjustments are effected are small brass screws moving in fixed nuts, one on each arm; the axis of one of the screws being *parallel* to the magnetic axis of the needle, and that of the other *perpendicular* to it.

The agate planes upon which the needle rests are attached to a solid support of copper, which is firmly fixed to a massive marble base. In this support there is a provision for raising the needle off the planes, the contrivance for effecting this object being similar to that employed in the inclination instrument. The whole is covered with an oblong box of mahogany, in one side of which are two small glazed apertures, for the purpose of reading; the opposite side of the box is covered with plate glass. A thermometer, within the box, shows the temperature of the interior air; and a spirit level, attached to the marble base, serves to indicate any change of level which may occur in the instrument.

The position of the needle at any instant is observed by means of two micrometer microscopes, one opposite each end. These miscroscopes are supported on short pillars of copper, attached to the base of the instrument. They are so adjusted that one complete revolution of the micrometer screw corresponds to 5 minutes of arc. The micrometer head is divided into 50 parts; and, consequently, the arc corresponding to a single division is 0″·1.

In addition to these parts, the apparatus is provided with a brass bar of the same length as the magnet, (furnished like it with cross wires at the extremities, and knife-edge bearings,) for the purpose of determining the zero points of the micrometers; a brass scale, divided to 10′, used in adjusting the value of their divisions; and a horizontal needle, to be employed in determining the azimuth of the vertical plane in which the needle moves.

Adjustments.—The following are the adjustments required in this instrument:

1. The instrument being placed on its support, in a suitable position with respect to the other two instruments, the azimuth of the plane in which the needle is to move may be adjusted in the following manner. The plane is made to coincide, in the first instance, with the magnetic meridian, by means of the horizontal needle which moves upon a pivot fixed to the top of the scale. A small theodolite (or other instrument for measuring horizontal angles) is then placed on the base; and its telescope brought to bear on a distant mark. The teles-

cope should then be moved through a horizontal angle equal to the intended azimuth of the instrument, but in an opposite direction. The base of the instrument is next to be turned, without disturbing the theodolite, until the mark is again bisected by the wires of the telescope: it is then in the required azimuth. The base should then be levelled, and permanently fixed.

2. The microscopes should now be adjusted, I. to bring the image of the cross wires of the needle to coincide with the wires of the microscopes; and 2. to make the arc value of the interval of the wires, corresponding to one revolution of the micrometer head, exactly equal to five minutes*. These arrangements have been nearly effected in the first construction of the instrument; for the purpose of completing the adjustment, the microscopes are capable of a double motion, one of the entire body of the instrument, and the other of the object glass alone. It is manifest that these two movements are sufficient to effect both adjustments. The former is attained when the cross of wires is seen distinctly (and without parallax) at the same time that the microscope wires are exactly in the focus of the eye-piece; the latter is accomplished when the moveable wire of the microscope is made to pass over a given number of divisions of the scale, by double the number of *complete* revolutions of the micrometer head.

3. The *fixed* wires of the microscopes are then to be adjusted to the same *horizontal* line. This is effected by means of the brass needle. This needle being placed upon the agate planes, by its knife edges, and allowed to come to rest, it is manifest that the line joining the cross wires will be horizontal, provided it be perpendicular to the line joining the centre of gravity and the axis. To effect this latter adjustment, the needle (a great part of whose weight is disposed below the knife edge) is furnished also with a small moveable weight. The test of the adjustment is similar to that of the corresponding adjustment of the ordinary balance. The moveable wire of one of the microscopes being brought to bisect the cross, if the adjustment is complete, it will bisect the cross at the other extremity upon reversal; if not, the position of the needle will indicate in what manner the weight is to be moved.

---

* This adjustment is by no means a necessary one. It is sufficient for all purposes if the arc value corresponding to one revolution of the micrometer be accurately known.

A horizontal line being thus obtained, the fixed wires of the microscopes are to be adjusted to it, by moving the capstan-headed screws with which they are connected.

4. The last adjustment is that of the magnetic needle itself. This adjustment is twofold: 1. of the needle to the horizontal position; and 2. of the centre of gravity of the needle to the magnetic axis. To effect this double adjustment the needle is furnished with two moving weights, one on each arm. These weights (it has been already stated) are screws moving in fixed nuts, one in a direction parallel to the magnetic axis of the needle, and the other in a direction at right angles to it. By the movement of the former the needle is brought to the horizontal position; and by that of the latter, the centre of gravity is made to coincide with the magnetic axis. The latter part of the adjustment is tested by inverting the needle on its supports; the inclination of the needle should not be altered by this inversion when the adjustment is complete.

Observations.—In observing the variations of the vertical force with this instrument, it is only necessary to bring the moveable wire of each micrometer to bisect the opposite cross of the needle; unless in seasons of disturbance, the needle will be found at each instant to have assumed its position of equilibrium. The interval between the fixed and moveable wires, expressed in angular measure, is the deviation of the needle of the horizontal position; and the changes of the vertical force are thence obtained by multiplying by a constant coefficient.

If n denote the number of minutes, and parts of a minute, in the observed angle of deviation, the changes of the force are expressed (as in the case of the other component) by the formula.

$$\frac{d\,\mathrm{F}}{\mathrm{F}} = k\,n;$$

in which the constant coefficient is

$$k = \cos\alpha \text{ contan } \theta \sin 1'.$$

The quantity F in the preceding formula is the product of the vertical component of the earth's magnetic force multiplied by the moment of free magnetism of the needle; or

$$\mathrm{F} = m\,\mathrm{Y}.$$

Accordingly the results thus deduced require a correction for the effects of temperature upon the quantity m. This correction is similar to that applied to the horizontal intensity; and the corrected expression of the changes of the vertical component is accordingly

$$\frac{dY}{Y} = kn + q\,(t - 32);$$

where t denotes the actual temperature (in degrees of Fahrenheit) at the time of observation, and q the relative change of the magnetic moment of the needle corresponding to one degree. As in the case of the other instruments, however, it is not in general necessary to apply these reductions to the individual results.

TIMES OF OBSERVATION.

The objects of inquiry in terrestrial magnetism may be naturally classed under two heads, according as they relate, 1. to the *absolute* values of the magnetic elements at a given epoch, or their mean values for a given period; or 2. to the *variations* which these elements undergo from one epoch to another. It will be convenient to consider separately the observations relating to these two branches of the subject.

ABSOLUTE DETERMINATIONS.

By the method of observation which has been suggested for the *absolute declination*, every determination of the position of the declination bar is rendered absolute. We have only to consider the varying angle between the magnetic axis of the bar and the line of collimation of the fixed telescope, as a correction to be applied to the constant angle (already determined) between the latter line and the meridian. It is manifest that if the *fixity* of the line of collimation of the telescope could be depended on, a single determination of the latter angle would be sufficient. But this is not to be trusted for any considerable period; and it will be therefore necessary, from time to time, to refer the line of collimation of the telescope to the meridian, by means of the transit instrument. This observation may be repeated *once a month*, or more frequently if any change in the position of the telescope be suspected.

In the case of the *intensity*, there is another source of error, (besides that due to a change in the position of the instruments) which can only be guarded against by a repetition of *absolute* measurements. The magnetic moment of the magnet itself may alter; and the observations of intensity changes afford no means of separating this portion of the effect from that due to a change in the earth's magnetism. This separation can only be effected by means analogous to those employed in the determination of the absolute value of the horizontal intensity; and accordingly one or other (or both) of the methods proposed for this determination should be occasionally resorted to. It is desirable that this observation should be repeated *once in every month*; and more frequently, whenever the changes observed with the horizontal force magnetometer indicate, by their *progressive* character, a change in the magnetic moment of the suspended bar.

It would be easy, in theory, to devise a method by which the vertical force magnetometer might be made to serve in determining the absolute value of the vertical intensity. The means which at present offer themselves appear, however, to be surrounded with practical difficulties; and it seems safer to deduce this result *indirectly*. From the formulæ given in page 452, we have

$$Y = X \tan \theta;$$

so that if the *inclination* θ be known, and the horizontal intensity X determined in absolute measure, the vertical intensity Y is inferred.

For the purpose of observing the element θ, each observatory is furnished with an inclination instrument, the circle of which is $9\frac{1}{2}$ inches in diameter. The observation should be made in an open space, sufficiently remote from the magnets of the observatory, and from other disturbing influences; and a series of measures should be taken *simultaneously* with the two intensity magnetometers, for the purpose of eliminating the *changes of the inclination* which may occur in the course of the observation. As to the mode of observation, the best seems to be the usual one, the plane of the circle coinciding with the magnetic meridian; but for the purpose of testing the axles of the needles, and the divided limb of the instrument, it is desirable that some observations should be made in *various azimuths*,—for example, every 30° of the azimuth circle commencing with the magnetic meridian. The inclination is then inferred, from each pair of corresponding results, by the formula

$$\cot^2 \theta = \cot^2 \zeta + \cot^2 \zeta';$$

ζ and ζ' being the observed angles of inclination in two planes at right angles to one another. Where the inclination is great (as at Montreal,) this method will serve to test only a limited portion of the circumference of the axle and limb. In this case the best course appears to be that pointed out by Major Sabine,* namely, to convert one of the needles, temporarily, into a needle on Mayer's principle, by loading it with sealing-wax; and to deduce the inclination, from the angles of position of the loaded needle, by the known formula of Mayer. The observations here suggested having been very carefully made, and the inclination changes eliminated in the manner above explained, the observed difference between the *mean* and the result obtained in the *magnetic meridian*, should be applied as a correction for the errors of axle and limb to all future observations made in the meridian.

These observations should be made at the same periods as those of the absolute horizontal intensity,

VARIATION OF THE ELEMENTS.

The *variations* of the magnetic elements are, 1. Those variations whose amount is a function of the *hour angle* of the sun, or of his *longitude ;* and which return to their original values at the same hour in successive days, or the same season in successive years. These, from their analogy to the corresponding planetary inequalities, may be denominated *periodical.* 2. The variations, which are either, continually *progressive*, or else return to their former values in long and unknown periods ; these may in like manner be denominated *secular.* 3. The *irregular* variations, whose amount changes from one moment to another, and which observe (apparently) no law.

The *periodical* variations (with the exception of those of the *declination*) have hitherto been little studied ; and, even in the case of the single element just mentioned, the results have scarcely gone beyond a general indication of the hours of maxima and minima, and of the changes of their amount with the season. The subject is nevertheless of the highest importance in a theoretical point of view. The phenomena depend, it is manifest, on the action of solar heat, operating probably through the medium of thermo-electric currents induced on the earth's surface. Beyond this rude guess, however, nothing is as yet known of the physical cause. It is even still a matter of speculation whether the solar influence be a *principal*, or only a *subordinate* cause, in the phenomena of terrestrial magnetism. In the former case, the periodical changes are to be regarded as the effect only of the

---

* *Reports of the British Association*, vol. vii. p. 55.

variations of that influence; in the latter, they must be considered as its entire result, the action in this case only serving to modify the phenomena due to some more potent cause. It may be fairly hoped that a diligent study of this class of phenomena will not only illustrate this and other doubtful points in the physical foundation of the science; but also, whenever that physical cause shall come to be fully known, and be made the basis of a mathematical theory, the results obtained will serve to give to the latter a numerical expression, and to test its truth. Even the knowledge of the empirical laws of the hourly and monthly fluctuations must prove a considerable accession to science; and (as one of its more obvious applications) will enable the observer to reduce his results, as far as this class of changes is concerned, to their *mean* values.

For the complete determination of the hourly and monthly changes of the magnetic elements, a persevering and laborious system of observation is requisite. The *irregular* changes are so frequent, and often so considerable, as (partially at least) to mark the regular; and the observations must be long continued at the same hours, before we can be assured that the irregularities do not sensibly affect the mean results. Again, in a theoretical point of view, the nocturnal branch of the curves by which the periodical changes are represented is quite as important as the diurnal; and it is manifest that nothing can be done towards its determination without the co-operation of a number of observers. At each of the observatories about to be founded by the liberality of Her Majesty's Government, there will be three assistant observers placed under the command of the director; and it is intended that the observations shall be taken *every two hours* throughout the twenty-four. In order that this series of observations, which is especially destined for the determination of the periodical changes, may at the same time cast some light upon the irregular movements, it is proposed that they shall be *simultaneous* at all the observatories. The hours which have been agreed upon are the *even* hours (0, 2, 4, 6, &c.) *Göttingen mean time.* It is likewise intended that *one* observation of the twelve shall be a *triple* observation, the position of the magnets being noted *five minutes before and after* the regular hour. The time of this triple observation will be 2 P.M., Göttingen mean time.

The barometer, and the wet and dry thermometers, will be registered at each of the twelve magnetic hours. No observation will be taken on Sunday.

No distinct series of observations is required for the determination of the *secular* variations. In the case of the *declination*, the yearly change will be obtained by a comparison of the monthly mean results (for the *same month* and *same hour*) in successive years. The observations of two years only will thus furnish 144 separate results, from which both the periodical and the irregular changes are eliminated; so that great precision may be expected in the final result, notwithstanding the limited period of observation. The same mode of reduction will apply to the two components of the *intensity*, provided that no change shall have taken place in the magnetic moment of the bars employed. In the latter event, recourse must be had to the *absolute* determinations for a knowledge of the secular changes.

The subject of the *irregular* movements has acquired a prominent, and almost absorbing interest, from the recent discoveries of Gauss. It has been ascertained that the resultant direction of the forces, by which the horizontal needle is actuated at a given place, is *incessantly* varying, the oscillations being sometimes small, sometimes very considerable:—that similar fluctuations occur at the most distant parts of the earth's surface, at which corresponding observations have been as yet made;—and that the instant of their occurrence is the same every where. The intensity of the horizontal force has been found subject to analogous perturbations.

For the full elucidation of the laws of these most interesting phenomena, it is of the first importance that the stations of observation should be separated as widely as possible over the earth's surface, and that their positions should be chosen near the points of maxima and minima of the magnetic elements. This has been in a great measure accomplished as regards the observatories about to be founded by Her Majesty's Government. The stations are wide asunder in geographical position, and they are in the neighbourhood of points of prominent interest in reference to the isodynamic lines. The results of observation at these stations will soon testify whether the shocks to which the magnetic needle is subject, are of a local or of a universal character as regards the globe; and in either event we may expect that they will furnish information of great value (in reference to a physical cause) as to the magnitude of the phenomena in different places, and the elements on which it depends.

In the observations destined to illustrate these phenomena, it is proposed to follow, as nearly as possible, the plan laid down by Gauss. One day in each month, namely, the *last*

Saturday, will be devoted to simultaneous observations on this systém; the observations commencing at 10 P.M. of the preceding eve (Göttingen mean time,) and continuing through the 24 hours.

LXIX.—*On Electro-Magnetic Forces.* By J. P. JOULE, Esq.

1. About the commencement of last April, I made some experiments in electro-magnetism, which I had the pleasure of communicating to the readers of this excellent work, in two letters to the Editor, dated on the 28th of May, and on the 10th of July. I am desirous of making some additional observations on that subject, especially as subsequent experience has enabled me to place in a more correct view some of the effects I then witnessed.

2. *I have shown*[*] *that* when a current of voltaic-electricity is transmitted through *the* coils of two electro-magnets, their mutual attraction is in the ratio of the squares of the quantities of electric force: and also that the lifting power of the "horse-shoe" electro-magnet is governed by the same law.

3. I have recently made experiments which prove that the attraction of the electro-magnet, for a magnet of constant force, varies in the simple direct ratio of the quantity of electricity passing through the coil of the electro-magnet. (In order to succeed, it is necessary to guard against the effects of induction by a proper arrangement of the apparatus.)

4. Magnetism appears therefore to be excited in soft iron, in the direct ratio of the magnetizing electric force; and electro-magnetic attraction, as well as the attraction of steel magnets, may be considered as proportionate to the product of the intensities of each magnet, or, which is the same thing, to the number of lines which may be drawn between the several magnetic particles of the attracting bodies.

5. This view is illustrated by figs. 1, 2, and 3, Plate XI. where the several attractions of the magnetic particles, viz. 1 to 1, 2 to 1, and 2 to 2 are represented by the number of lines drawn in each instance, 1, 2, and 4.

6. I have recently understood, that the Russian philosophers Jacobi and Lenz, have arrived by their experiments, at some of the same conclusions with regard to the laws of

---

[*] *Vol. 4, page 134—135.*

electro-magnetic attraction. I have not read their papers, but shall be most happy if they shall be found to confirm the results of my observations.

7. Fig. 4, will perhaps illustrate, with a considerable degree of accuracy, the complex action of the forces which constitute the aggregate attraction which exists between two magnets, for instance, *A.* and *B.* The magnetic particles of which six only *a*, *b*, *c*, *d*, *e*, *f*, are drawn, may be conceived to be of an indefinitely large number spread throughout the region of the "poles;" and the several forces are represented by the straight lines drawn been those particles.

8. If this view be correct, it is obvious that the closer the approximation of the magnetic particles in each system, the greater will be the magnetic attraction; for in that case the particle *a* will both be nearer the particle *f*, and the force exerted between them will be in a less oblique direction.

9. It was in consequence of my entertainment of a different hypothesis, that I was led to imagine that I had detected a decrease of power on increasing the length of the electro-magnet; in Vol. 4, page 136 and 137, is a comparison of the powers of three electro-magnets of the several sizes, $\frac{5}{11}$ $\frac{6}{11}$ and $\frac{7}{11}$, of an inch square, with those of two whose sectional areas were respectively, one inch square, and one inch by 2 inches. It is probably in a great measure the consequence of the principle, (7) that the mean power of the latter, was found to be less than that of the former, in the ratio of 7000 to 10646; and this observation is further-corroborated by the fact, that, of the long electro-magnets the less powerful has the more extended "pole." It would, however, be a matter of no difficulty to determine the inflence of length on magnetic conduction.

10. Hence also a correction should be applied to the attractions of the larger electro-magnets* in order fairly to compare their respective powers with those of smaller dimensions. I will not venture to decide its amount, as that will be entirely dependant upon the distances of the polar particles, (7), from the ends of the electro-magnets; if $\frac{1}{3}$ were added to the attractions opposed to Nos. 5, they would I think, be placed in pretty correct comparison with Nos. 1.

11. These corrections are not however of sufficient amount to affect the general conclusion to which I have come, with regard to the laws under which magnetic attraction, (as applicable to the production of motive force,) is developed by electricity, viz.: *That the attraction of two electro-magnets*

---

* Annals Vol. 4, page 133.

towards each other, is in every case represented by the formula $M = W^2 E^2$*, where* M*, denotes the magnetic attraction,* W*, the length of wire, and* E *the quantity of electricity conveyed by that wire in a given period of time;* a formula *modified* merely by the effects, of saturation, of the conducting power of iron, and of the distance of the coils from the surface of the iron.

12. I have observed, that magnetic and electro-magnetic attraction decreases, in certain cases, in the simple ratio of the distances. This was found to be particularly the case when the magnets were long, and the distances between them small. Mr. Harris has observed the same effect, see his "Experimental Inquiries concerning the laws of Magnetic Forces." It may be principally accounted for by the complex action previously illustrated. It is impossible to doubt that the law of magnetic attraction is inversely as the squares of the distances.

13. I shall now in accordance with my promise enter into the detail of some experiments with the electro-magnetic engine described in the "Annals" for October, 1839; and first it will be proper to describe the apparatus I had occasion to use.

14. The galvanometer was constructed on the plan which was described in pages, 131 and 132, of the present volume. The coil is rectangular, 12 inches long and 6 inches broad; the copper wire is $\frac{1}{12}$ of inch thick, and the length of the needle rather less than 4 inches. To make it more extensively available, I have drawn a curve, whose abscissæ are the degrees of the circle, and whose ordinates are the quantity numbers corresponding to those degrees, in this way I can interpolate to any extent the quantity divisions previously obtained by experiment.

15. I recommend this form of the galvanometer with great confidence, because, 1st, The method of tangents is only applicable when the diameter of the coil bears a very large ratio to the length of the needle, and 2nd, Because you can by passing the electric current through 1, 2, 3, 4, &c. coils, increase the delicacy of the instrument accordingly.

16. I have principally made use of Wollaston's 4 inch doubly coppered batteries, with amalgamated zinc plates, and charged with a solution of sulphuric acid. I shall perhaps describe at an early opportunity an expeditious and convenient method of fitting up both this battery, and the admirable instrument of Mr. Grove.

17. In the subsequent experiments, the engine was fitted up with the hard iron, and hard wire, revolving electro-magnets. After a few trials with powerful batteries, finding

it inpracticable to work with the highest intensity arrangement, I soldered the ends of the three wires of each electro-magnet together, and united the combined wires in such a manner, that the electric current passed through 424 yards of threefold conducting wire.

18. In the tables underneath, the first column indicates the quantity of electricity; the second, the differences of those quantities; the third, the velocity of the revolving electro-magnets, in feet, per second; the fourth, the duty, in pounds raised per second of time, to one foot in height; and the fifth, the duty, in pounds raised to the height of one foot by the agency of one pound of zinc.

19. In calculating the amount of duty, I found that in this arrangement, 12·4 of electricity was just sufficient to keep the machine in motion, when the friction, referred to the revolving electro-magnets, was equal to 10 ounces avoirdupoise; the same amount of electricity was, whatever the velocity, always able to overcome exactly the same amount of friction; I therefore felt justified in making it a basis on which to calculate the force due to other quantities of electricity. The duty in the fifth column is calculated on the basis of the decomposition of water effected by a given quantity of electricity; I consider it as an *approximation* to the truth. I may just observe that the friction has been altogether neglected, and that whenever the motive force was not sufficient, mechanical means were resorted to in order to overcome it; this course was adopted, because the friction is not at all to be considered as an element in the subsequent observations.

Table 1.

80 pairs of Wollaston's plates.

(A mean of 3 trials.)

Electricity.	dif.	Velocity.	Duty.	Economical duty.
24·6		0	0	0
	3			
21·6		2	3·8	21960
	2			
19·6		4	6·25	39740
	1·6			
18·0		6	7·89	54800
	1·5			
16·5		8	8 85	66950
	1·5			
15·0		10	9·15	76140

TABLE 2.

40 pairs of Wollaston's plates.

(A mean of 2 trials.)

Electricity.	dif.	Velocity.	Duty.	Economical duty.
11·8		0	0	0
	1·6			
10·2		2	·85	20100
	·8			
9·4		4	1·44	38300
	·8			
8·6		6	1·80	52320
	·6			
8·0		8	2·08	65000

TABLE 3.

10 pairs of Wollaston's plates.

5		0	0	0
	·8			
4·2		2	·14	33300
	.6			
3·6		4	·21	58300
	·3			
3·3		6	·265	80300
	·3			
3		8	·292	97300

TABLE 4.

Grove's battery of 10, 4 inch, plates.

(Not very efficiently charged.)

17·6		0	0	0
	3·3			
14.3		2	16·6	116080
	1·9			
12·4		4	2·5	201600
	1.4			
11·0		6	2·95	268200
	·8			
10·3		8	3·38	331400

20. I now united the conductors in such a manner, that the fluid was divided between each pair of stationary, and revolving, electro magnets; in this case, the electricity passed through 212 yards of six-fold wire.

TABLE 5.

A quantity arrangement of two 40 pairs of Wollaston's plates.

Electricity.	dif.	Velocity.	Duty.	Economical duty.
52		0	0	0
	9			
43		2	3·76	21800
	5			
38		4	5·87	38600
	3·2			
34·8		6	7·38	53100
	2·6			
32·2		8	8·42	65400
	2·4			
29·8		10	9·02	75700

TABLE 6.

A quantity arrangement of two 20 pairs of Wollaston's plates.

28·2		0	0	0
	5			
23·2		2	1·1	23700
	2·5			
20·7		4	1·74	42000
	1·7			
19·0		6	2·205	58000
	1·4			
17·6		8	2·52	71600

TABLE 7.

A quantity arrangement of two 10 pairs of Wollaston's plates.

16·8		0	0	0
	3			
13·8		2	·387	28000
	1·6			
12·2		4	·605	49600
	1·2			
11.0		6	·738	67100
	1·0			
10·0		8	·813	81300

21. The above examples will show pretty clearly the effects of magnetic electrical resistance. This resistance is the prime obstacle to the perfection of the electro-magnetic engine, and in proportion as it is overcome, in the same proportion will the motive force increase; this ought therefore to claim our first attention.

22. On comparing the *differences* with the *velocities* and *electricities* in each table, the general conclusion is, that *the magnetic electrical intensity is directly proportional to the velocity, multiplied by the magnetism*, or, which is the same thing, by the electricity which induces that magnetism. It is the latter part of this law, which makes the *differences* decrease generally, (and as accurately as the nature of the manipulations can lead one to expect,) in the same ratio with the *electricities* opposed to them. It is necessary to observe that the *first difference*, or that which exists beteen 0, and 2, *velocities*, must be neglected, as that is much augmented by the slightest inaccuracy of the commutator.

23. It appears moreover, that this law is *entirely unaffected by the diminution or increase of battery intensity*; for on comparing the tables of either system together, it will be seen that in all cases the *differences* are about one-tenth of the *electricities* opposed to them. I wish to call particular attention to this circumstance, which is owing to the constant resistance of the wires, in each separate system.

24. In the second arrangement the conducting metal was half as long and twice as substantial as it was in the first; hence it is, that half the battery intensity sufficed to pass twice the quantity of electricity, and so to produce the same motive effect. This is seen on comparing table 1, with table 5.

25. Also, on referring to tables 1 and 5, it will be observed, that the *differences* are twice as great in the 2nd arrangement as in the 1st, whilst the magnetic force remained very nearly the same. To understand the reason of this, it will be necessary to observe, 1st; that the magnetic electrical intensity has nothing whatever to do with the thickness of the wire upon which it is induced, but exists *simply in the direct ratio of the length*, consequently that the intensity is only one half as great in the 2nd arrangement, as it is in the 1st; And 2nd, that, as the resistance of the wire to the battery current, in the 2nd arrangement, is only one quarter of that in the 1st, the *same* additional, or extraneous, resistance will produce four times the effect in the former, as in the latter instance. Hence by compounding these two effects, we have the differences of electricity, due to a given increment of

velocity, and the same amount of magnetism, twice as great in the 2nd, as in the 1st arrangement.

26. If the intensity of the voltaic battery do not increase in a less ratio than that of the number of its pairs, there will theoretically, be no variation in economy, whatever the arrangement of the whole conducting metal, or whatever the size of the battery. For, if the battery be doubled in intensity, it must in that case consist of twice the number of pairs, which will cause twice the quantity of electricity to pass, and hence four times the weight of battery materials will be consumed, while the force of the engine is also increased four times, according to the square of the electricity. See the economical duty in the tables 1, 2, 5, and 6.

27. The following are three resources on which to rely, in order to obtain economical power; 1st, the increase of the quantity of conducting wire, which will produce a *variable* degree of advantage, for while it dimishes the resistance of the wire, it produces no effect upon the magnetic electrical resistance; 2nd, the augmentation of the intensity of the elementary battery, which will produce an exactly similar increase of duty: (compare table 3 with table 4.) 3rd, the improvement of the arrangement of the electro-magnets. Had I placed mine in such a position that the *broad* edges of the poles should have acted on each other, I should doubtless have attained a considerable higher amount of duty.

28. I must apologize to the reader, that I have not relieved the tediousness of this paper, by a single brilliant illustration. I have neither propelled vessels, carriages, nor printing presses. My object has been, first to discover correct principles, and then to suggest their practical developement. If I have succeeded in some measure in the first part of that object, my design has been fully realized.

Broom Hill, near Manchester,
March 10th, 1840.

LXX. *Wonderful effects of Voltaic Electricity in restoring Animal life when the sensorial powers have entirely ceased or in other words, when death in the common acceptation of the term has actually occurred.* Extracted from Mr. W. H. HALSE's address to the Newton Society for the attainment and diffusion of Knowledge dated March 3, 1840.

After describing the benefits obtained by a study of the sciences generally, he thus proceeds to show the powers of galvanism on the animal body.

"On Thursday last one of my spaniels whelped, having a litter of thirteen; six of which I took for my experiments. I drowned three of them in *cold* water and kept them immersed for fifteen minutes, at which time I took them from the bucket and placed them in front of a good fire;—*no motion could be perceived in either of them.* I then put the front legs of one of them in a jar containing a warm solution of salt and water and its hind legs in a similar jar, in each of which was inserted one pole of the galvanic battery; the whole was then placed near the fire.

"The position of the dog being now favourable for operating on, without the necessity of making any incisions in the flesh, I passed a very strong shock through its body; it moved its hind legs; I gave it another shock, which caused its tail also to move; I now passed twenty shocks in quick succession through its body: *it moved every limb, its mouth opened and I was inclined to believe that the dog had actually come to life;* but the moment I ceased passing the shocks, the dog was as motionless as it was previous to my commencement. Again I continued the shocks and I noticed that there was more motion in the limbs:—considering that in proportion to the return of sensibility, that these shocks would be too powerful for it, I decreased the *intensity* of them and passed many hundreds in rapid succession; I continued this for about five minutes—the motion of the limbs increasing as the shocks increased in number—I now ceased; *the dog still moved.*—IT WAS RESTORED TO LIFE.—I placed it on a warm flannel in front of the fire and in a very short time it appeared as well as it was previously to its being drowned; it crawled on the flannel and made the noise peculiar to young dogs. I now examined the two other dogs which were drowned and taken from the water at the same time that this one was.—THEY WERE BOTH DEAD—*a plain proof that it was entirely owing to the galvanic fluid that life was restored.*

"The other three dogs I drowned in *warm water and kept them immersed for forty minutes,* at which time all motion had ceased; two of them I laid in front of the fire and the remaining one I placed in the jars as in the preceding experiment. I now passed a few shocks of weak intensity through the body, but no motion was perceptible; I therefore increased the intensity of them considerably and gave the shocks in quick succession.—*Every limb moved, the belly protruded and again collapsed, and the head was raised*—at this period I stopped passing the shocks in order to see if there were any motion in the dog when not under the galvanic influence;—there was none; I again proceed with the shocks and having noticed that the limbs moved more rapidly than before, I considered it necessary to decrease the intensity and increase the *quantity* of electric fluid, which I did so much as just to be enabled to perceive a slight tremor in the dog; I continued in this manner for about five minutes at which time I removed it from the jars and placed it on the table.—IT WAS ALIVE,—In a quarter of an hour it appeared to be perfectly recovered. The other two dogs (which were not allowed to get cold during the whole of the experiment) were now examined; *no motion whatever could be perceived.* I tried the the effect of galvanism on one of these; I was successful. In one hour after this I operated on the other dog also; *but 'twas in vain—there was no vigour remaining in the vital powers;—life had fled.*

"Remarks:—Having stated that I restored the dogs to life, it will be necessary for me to explain in what light this is to be understood. Strictly speaking, life was not extinct in either of the dogs previously to my operating, for if it had they would certainly have remained dead; *it was merely a cessation of the sensorial functions, whilst there was a degree of vigour still remaining in the vital organs which combined with the nervous influence (or a substitute for it if you please) I supplied by the powers of galvanism; were sufficient to restore these functions to their former state of activity;* nevertheless, the dogs were in the common acceptation of the term—dead; but not properly so, for death cannot be considered to have actually arrived until the sensorial, the muscular and the nervous functions all cease to act—at that moment the animal dies and not before. We therefore see that although the dogs were not strictly speaking—dead, previous to my operations, yet by the fact of the others being dead when the sensorial of those three were restored, it must be evident that the process of dying had commenced, *and would have been perfected had not the powers of galvanism been resorted to;*—therefore when I say the dog was restored to life, I must not be considered to mean that I brought the dead to life but rather that I arrested the process of dying, by restoring the sensorial functions—which functions had before entirely ceased to act.

"The nature of the above experiments must be very familiar to every physiologist, but when we consider the astonishing powers of galvanism on the human frame, in supplying the nervous fluid (or a substitute for it) and the ignorance of this fact by a large proportion of the medical profession, perhaps I shall be excused for introducing this subject to your notice; and as the apparatus necessary for the purpose, when constructed on my principle is quite unexpensive (one guinea, see No. 23 of Sturgeon's Annals of Electricity, Magnetism, and Chemistry,) *I hope there will not be found many medical practitioners who will object to add the powers of voltaism to their other modes of resuscitation from the first stage of death caused by drowning, or from that caused by suffocation through noxious gases.*

"I have refrained from introducing many technical terms, as I wish my subject to be generally understood. It will be perceived, that in passing the shocks through the bodies of the dogs, no cruelty was practised, for when the powerful shocks were passed, they posessed no feeling whatever, and in proportion as the sensorial powers and consequently the feeling returned, the intensity of the shocks were reduced; and when it is also considered that the dogs would have been drowned had these experiments not been made, I trust I shall not be accused of having had recourse to cruel methods, for the purpose of putting the powers of the voltaic electricity on the animal body, to the test of experiment."

WILLIAM H. HALSE.

Brent, near Ashburton.

LXXI. *On Lightning Conductors, and the effects of Lightning on Her Majesty's Ship Rodney and certain other Ships of the British Navy; being a further examination of* Mr. Sturgeon's *Memoir on Marine Lightning Conductors. By* W. Snow Harris, *Esq. F.R.S., &c.*

To the Editors of the Philosophical Magazine & Journal.

Gentlemen,

1. In my former communication (L. and E. Phil. Mag. vol. xiv. p. 461.) I considered the nature of a well-known phenomenon in electricity, termed by Cavallo, Priestley, and others the lateral explosion, and shewed that it did not apply to the state of a metallic rod in the act of transmitting a vanishing electrical accumulation between two opposed electrified surfaces, as insisted on by Mr. Sturgeon in a recent number of his Annals of Electricity. I will now proceed to examine the general character and effect of ordinary electrical dis-

charges, whether produced on the great scale of nature, or artificially, with a view of further showing, that such lateral explosions do not occur at the instant of the passing of a shock of lightning through a metallic conductor, as also with a view of meeting certain other objections which have been advanced at different times to the use of lightning rods in ships.

2. I should not have felt myself called upon to notice further Mr. Sturgeon's memoir, did I not consider the statements it contains, although superficial and inconclusive, likely to mislead the public upon many important points, connected with the effectual protection of shipping, against the destructive effects of lightning, and convey false views of the nature of electrical action. Under these impressions I have little hesitation in noticing what he has advanced under the following heads:—

1st. Examination of the observed effects produced on shipping by lightning.

2nd. A comparison of the observed effects of lightning and the probable effects which lightning would produce by the application of Mr. Harris's conductors to shipping.

3. The first contains an excellent, and I have no doubt, an accurate statement, by an intelligent officer of the Rodney, of the destructive effects of lightning lately experienced in that ship, together with notices of two cases in which ships fitted with my conductors were struck by lightning without any attendant ill consequence. In the second, it is the author's object to prove, from the effects of lightning in the Rodney, that my system is inadmissible; since the discharge of lightning, he observes, which struck the Rodney, "would have been powerful enough to have rendered even the thickest part of Mr. Harris's conductors sufficiently hot to ignite gunpowder.

Considering the boldness of this assertion, and the high pretension of the memoir, we should expect, on examining the author's researches, to find him in possession of a copious induction of facts from well-authenticated cases of damage by lightning on ship-board, illustrating clearly the views he so strenuously insists on,—cases in which continuous or other metallic conductors have been from any cause placed along the masts or rigging, and in which the electric agency found its way through the hull to the sea. We should further expect from him, something like an examination of the general nature and effects of electrical discharges, since it is clear before any accurate estimate can be arrived at, of the relative quantity of electricity likely to be discharged from a thunder-cloud, and the probable effects of metallic rods, or other conductors set up with a view of directing it in any given course, such information is quite indispensable.

4. Now it is to be particularly observed, that Mr. Sturgeon's memoir is really deficient in such information; a few clumsy experiments in illustration of a well-known fact in electricity, deceptively associated, by means of a vague hypothesis, with some of the ordinary effects of lightning, on a ship *not having* any regular conductor, and with some every-day phenomena of the electrical kite, is virtually the amount of all that the author has advanced, under the imposing title of "Theoretical and Experimental Researches.

5. In illustration of the careless way in which he meets this question, it may not be out of place to notice the following specimen of his inductive philosophy,—being the very outset of the comparison he has proposed, of the observed effects of lightning, and the probable effects on my conductors*.

In the account given of the damage recently sustained by H.M. Ship Rodney, it appears, that the shock of lightning which shivered the top-gallant-mast, damaged the top-mast, &c., &c., fell on a small brass sheave in the truck for signal halliards, and *slightly* fused it. This sheave weighed about four ounces; it was only about an inch and a half diameter, hollowed except at the centre and rim, where it was somewhere about half an inch in thickness. The lightning also fell on the copper funnel for top-gallant rigging, being a hollow cylindar of sixteen inches in length, 10 inches in diameter, and not quite a quarter of an inch thick. This funnel was not anywhere fused. It fell also on other metallic masses, such as the iron-bound tie-block, on the top-sail-yard, &c.,&c., the iron hoops of the masts, &c., on which no calorific effect was apparent.

6, Now we have here something like evidence what was really the *actual power* of the charge. We see, for example, that it *did not* fuse a copper funnel, 16 inches long, 10 inches in diameter, and about ¼th of an inch thick. In the face of which fact Mr. Sturgeon insists, that had the charge fallen on my conductor, the thickest part of it would have become red-hot. His reasoning, in fact, amounts to this; an explosion of lightning having *slightly* fused a small brass sheave, weighing 4 ounces, and having failed to fuse a short copper funnel, therefore had it fallen on a rod of copper of one inch in diameter, and 200 feet long*, that rod would have been rendered *red*-hot.

---

* Sturgeon's Memoir, sec. 204.

This, it must be allowed, is a somewhat amusing kind of special pleading, quite unprecedented, I believe, in any paper on science.

7. The author wishes to strengthen his deduction, such as it is, by adverting in a foot-note to the case of a small brig struck by lightning, in which some part of a chain conductor is *supposed* to have been fused; how much is not known, "as the lower part fell overboard." The statement is given without any quoted authority, and is altogether deficient in the very information most required, viz. the *size of the chain, and how much of it was fused.* Let us, however, take it upon the author's own ground, and suppose the conductor to have been such as is commonly used in the merchant service,—that is to say, links of iron wire about one-fourth of an inch in diameter, united by rings, a kind of conductor very easily disjointed and fused at the points of junction by lightning; —the reasoning then stands thus: because a shock of lightning fused and disjointed some unknown portion of a lightning chain in a merchant brig, therefore the same shock, had it fallen on a solid copper rod of one inch in diameter and 100 feet long, would have rendered that rod *red*-hot.

8. The fallacy and entire worthlessness of such reasoning, seems not altogether to have escaped Mr. Sturgeon's notice, as appears by his amplification of the above effects; thus on entering upon the comparison of the effects of lightning, he resorts to a sort of wholesale dealing, and leads the reader to conclude that the *entire* sheave in the Rodney and *all* the brigs' conductor underwent fusion. But even if it were so, no such conclusion as that above mentioned is admissible †, especially in reference to a continuous and massive conductor

---

* This is the equivalent of my conductor on the main-mast of such a ship as the Rodney, taking it at its least value.

† "Were there no other data than those of the *fusion* of *the* metallic sheave in the Rodney and the fusion of the *chain conductor* in the brig Jane," &c-&c.

"The impressions which these facts convey to the mind are too definite to be easily understood; they clearly imply that either of the discharges which struck the Rodney or Jane would have rendered the thickest part of Mr. Harris's conductors sufficiently hot to ignite gunpowder," &c. &c.—Sturgeon's Memoir, sec. 204.

terminating in a point, and equalizing with inconceivable rapidity the disturbed electrical state of the sea and clouds.

9. The manifest deficiency of sound practical information in Mr. Sturgeon's memoir, imposes upon me the necessity of adverting to the general character and operation of common electrical discharges, whether produced by artificial means or on the great scale of nature. In doing this I have no desire to excuse myself, in case I should not have written clearly and explicitly on the subject, since in no department of physics is the field of observation so fertile, and the path of experiment so sure and easy. We have before us the experience of nearly a century, during which time lightning-rods have been employed; a great number of instances have occurred of shocks of lightning falling on ships under a variety of different circumstances, in some cases where lightning conductors have been present, in others where absent; in many instances where ships have been near each other and exposed to the same storm, some *having* conductors, others *not*. The general laws of the discharge are traceable in them all, and the effects on metallic bodies distinctly shown. On the other hand, we can on a minor scale, imitate successfully the great operations of nature, and examine experimentally every possible contingency attendant on the operation of a shock of lightning in a ship. It is our own fault, therefore, if we do not treat the subject scientifically, and arrive at complete practical solutions of such questions as these: Is a lightning conductor desirable in a ship? Will it cause by attraction a shock of lightning to fall on a ship when otherwise such would not take place? If so, can it cause damage by its inability to get rid of the lightning which falls on it? What is the *best* form and dimensions of a lightning conductor for a ship? What is the greatest probable force of lightning to which it may become exposed? Is it liable to cause damage by any lateral operation of the charge passing through it? I say, if such questions as these cannot now be reasonably determined they probably never can; and, therefore, any one who writes or reasons obscurely about them, and without due regard to a good induction of facts, can have no claim to be considered as a sound reasoner in experimental science; for, as beautifully observed by Lord Bacon "Man, who is the servant of nature, can act and understand no further than he has, either in operation or in contemplation, observed of the method and order of nature." Under these impressions I proceed to examine the general character and effects of electrical discharges as exhibited artificially, and on the great scale of nature.

10. Although some theoretical differences may have arisen concerning the precise nature of electricity, yet the following explanation runs sufficiently parallel with facts to entitle it to our confidence, and put us in possession of one of the great advantages of *every* theory, viz. a classification and connexion of observed effects; the province of human knowledge, being, as justly observed by a most intellectual and accomplished writer, "to observe facts, and trace what their relations are."*

General principles:—

11. There is an invisible agency in the material world intimately associated with common matter, termed electricity.

12. Lightning, thunder, and a variety of analogous phenomena of a minor kind, artificially produced, result from discharges of this agency between bodies differently affected by it.

13. In every case of electrical discharge there are two surfaces of action; one existing on some substance eager to throw off redundant electricity, being, according to Dr. Franklin, overcharged with it; the other existing in some other substance eager to receive electricity, being, according to the same philosopher, deficient of it, or undercharged.

14. Two opposed bodies, when placed in these opposite electrical states, have a sort of exclusive action on each other, either *directly* through any intervening substance, whether a conductor of the electrical principle or not, or otherwise *indirectly* through any lateral circuit.

Thus two metallic surfaces A B (fig. 1, plate X.) pasted on the opposite sides of a square of glass *c d*, have, when the square is said to be charged, an exclusive action on each other, either through the intervening glass, or otherwise through any conductor, A o B, connecting them.

Now we have only to suppose these planes placed further apart, as in fig. 2, to have a discharging conductor, *m n*, of greater or less extent between them, to be greatly increased in size, to be separated by air instead of glass, and to consist of free vapour or water, and we have a pretty faithful representation of the conditions, under which a discharge of lightning takes place, when passing partly through the air, and partly through a discharging conductor, *m n*, or any other body, *c d*, placed on the plane B.†

---

* Abercrombie on the Intellectual Powers.

† The thickness of the intervening air, and the amount of free electricity in the clouds, has led Professor Henry to question in some measure, the

15. Any continuous metallic rod or other body, *m n*, (fig. 2,) connected with the lower plane, must be considered merely as a passive way of access for the charge so far as it goes; the electrical agency being observed to seize upon substances best adapted and in a position to facilitate its progress, or otherwise to fall with destructive effect upon such as resist it.

16. It is easy to perceive here, that the presence of a conducting rod, *m n* (fig. 2,) or other conducting body, has nothing whatever to do with the great natural action set up between the planes A B. It is in fact to be considered merely as a point in one of them. The original accumulation of electricity and subsequent discharge, would necessarily go on whether such a rod were present or not, as is completely shown by experience. When present, its operation is confined to the transmission, so far as it extends, of that portion of the charge which happens to fall upon it; and since it is quite impossible to avoid the presence of conducting bodies in the construction of ships, it is the more important to understand clearly in what way damage by lightning occurs to the general mass, and how it may be best avoided.

17. When discharges of lightning fall upon a ship in the way above stated, as being a heterogeneous mass fortuitously placed between the charged surfaces A B (fig. 3.), the course of the discharge is always determined through a certain line or lines, which upon the whole least resist its progress. The interposed air between the ship and the clouds first gives way in some particular point, probably the weakest,—suppose at

---

perfect analogy of a discharge of lightning, with that of a Leyden jar; but I think upon *mature* consideration this circumstance will not be found in any way subversive of the general principle. Thus whether electricity be accumulated on thick glass or on thin, the result is the same; it is merely the intensity as indicated by the electrometer which changes.

Now the term free electricity, applies to the greater or lesser influence of the opposed coating in respect of other bodies. In the case of the opposed surfaces of the clouds and earth, all the charge is necessarily free electricity, since there exists no other point upon which it can tend to discharge. In the same way the electricity of the jar, when the coatings are very near, is nearly all redundant, or free electricity, in respect of the action between them, although latent in respect of other bodies. Hence with a moderate accumulation, the electrometer exhibits but a small intensity, if any. The only difference at the time of the discharge, is in the position of the discharging circuit, which in the case of the clouds and sea, is directly in the interval of separation; and as we find the principal of induction always active in cases of lightning, the thickness of the stratum has evidently no influence on the conditions of the accumulation, especially when we consider the great extent of the opposed surfaces, which may possibly he 20,000 or more square acres. Dr. Faraday has shewn that no distance excludes the the inductive action.

A, fig. 3;—the electrical agency then meeting with continued resistance from the non-conducting particles of air; is often turned into a tortuous course. Suppose it arrives in this way at some point, *m*, in the vicinity of a ship at *k*, the question whether it would strike upon the mast at *y* would be determined by the resistance in the direction of *m y k*, as compared with that in any other direction *m*, B; whether, in fact, it would be easier to break down the remaining air in the direction M B, or otherwise the air in the direction *m y*, supposing the ship's mast to facilitate the progress in that direction.

18. Let the charge however strike in the direction *m y*, and so fall upon the mast,—then in proceeding to its ultimate destination, viz. the plane of the sea B, its course is still determined by the same general principles; that is to say, it seizes upon all those bodies which tend to assist its progress, and which at the same time happen to be placed in certain relative positions, *and upon no others*, falling with destructive effect upon intervening bad conductors, and exhibiting in non-conducting intervals all the effects of a powerful expansive force. If we examine carefully the course of discharges of lightning on ships in some hundred instances in which damage has ensued, we shall find this effect invariable. The damage has always occurred where good conductors cease to be continued, and the destructive consequences most apparent are those usually produced by expansion. The calorific effects, except as depending on this cause, are really inconsiderable; there are comparatively few instances in which metallic bodies have been fused, and no instance in which a bolt or chain of any considerable magnitude has been even much heated.

The following experimental and natural illustrations of these facts will be found conclusive and interesting.

Exp. 1. Lay some small detached pieces of leaf-gold *a*, *b*, *c*, *d*, &c. on a piece of paper, as represented in fig. 4; pass a dense shock of electricity over these, from the commencement at A to the termination at B, so as to destroy the gold; the line which the discharge has taken will be thus shown by the blackened parts; the result will be as in fig. 5, in which we perceive the course of the discharge has been in the dotted line *a*, *b*, *d*, *e*, *f*, *g*, *h*, *l*, being the least resisting line; and it is particularly worthy of remark, that not only are the pieces *c*, *k*, untouched, being from their positions of no use in facilitating the progress of the charge, but even portions of other pieces, which have so operated, are left perfect, as in the transverse piece *i* and portions of *a*, *b*, *d*, *e*, and *f*; so little is there any tendency to a lateral discharge, even up to the point of dispersion of the metallic circuit in which the charge has proceeded; indeed, so completely is the effect confined to the line of least resistance, that percussion powder may be placed with impunity in the interval between the portions *c, d*. Now the separate pieces of leaf-gold thus placed, may be taken to represent detached conducting masses fortuitously placed along the mast and hull of a ship.

Exp. 2. Let a thin continuous line, *m, n*, be passed through the separated pieces, and a dense accumulation discharged over the whole, as in the preceeding case. The effect will be as represented in fig. 6.; the discharge will be confined to the line of least resistance; and we may perceive in this, as in the former case, that those pieces, or parts of pieces, out of the track of the discharge, are not affected; thus a part only of the piece *g* is destroyed, also of the piece *i*, whilst other pieces, *b, d, e, f, l*, which in the former case, where the continuous line, *a, b*, was not present, were blackened by the discharge, remain here perfect.

Exp. 3. If the continuous line A, B (figs. 7, 8) be assisted by other comparatively short collateral branches, as *d e, d c*, having one common connexion at B, then a discharge which would destroy the line A, B, will divide upon these auxiliary lines, and the part *d*, B will either escape, or the whole will suffer together.

Exp. 4. Pass a discharge over a strip of gold-leaf, as A, fig. 2; every part of it, as indicated by the last experiment, will participate in the shock; and if it be of uniform density and thickness it will be everywhere equally affected, so that one portion will not be destroyed without the whole. This result will be readily distinguished from that represented at *d* and *i*, fig. 5, where the masses lie across the track of the discharge.

The diagrams here referred to, are copied from the actual effects of the electrical discharge in the way above mentioned.

19. These experiments are instructive. They evidently prove, that an electrical explosion will not leave a good conductor, constituting an efficient line of action, to fall upon bodies out of that line. Mr. Sturgeon's assertion that a conductor on a ship's mast would operate on the magazine is therefore quite unwarranted. Besides, we have many instances of the masts having been shivered by lightning into the step, whilst acting as partial conductors, without any such consequence; as happened in the Mignonne in the West Indies, the Thetis at Rio, the London, Gibralter, Goliath, and many others. Instead, therefore, of a conductor on the mast being dangerous, it is absolutely requisite as a source of safety to the

ship, by confining the discharge to a given line and leading it to the sea.

20. It was from a careful consideration of the common effects of lightning, and from such experimental facts as those above mentioned, that I was led to suggest the propriety of fitting continuous conductors of lightning of great capacity in the masts of ships, linking them by efficient communications, together with the principal detached metallic bodies in the hull, into one general continuous system, and finally connecting the whole with the sea. These conductors consist of two laminæ of copper-sheet, varying from one inch and a half to five inches wide, and being together nearly one-fourth of an inch thick; they are inlaid so as to be fair with the surface of the mast, and form a series of shut-joints; they are otherwise so constructed as to present an uninterrupted line of action from the highest point to the sea. The method has been partially used in the British navy for several years, and has been proved in every way efficient. In no case has any of the vessels fitted with them received the slightest damage, although frequently exposed to severe thunderstorms, and in some instances actually struck by heavy discharges similar to that which fell on the Rodney in December, 1838.*

21. If we consider attentively the effects of this shock, we shall find them in complete accordance with the principles just stated. The attendant phenomena were of the simplest kind, and such as have always occurred in cases of ships struck by lightning not having a continuous conductor: e. g. the electrical discharge, in forcing its way between the sea and clouds, over resisting intervals, and between discontinuous metallic masses, was productive of a violent expansive effect in these intervals; causing at the same time a considerable evolution of heat. There was really nothing particularly remarkable in this instance; the course of the discharge was a very simple affair, being, according to the law of electrical action just exemplified (Exp. 2,) in the line or lines of least resistance from the highest point to the sea: thus the course of the discharge was, as represented in fig. 6, plate XI, along the masts and rigging, upon the general mass of the hull and sea. The vane-spindle *a*, upon which the accumulation was first concentrated, was of course severely dealt with. From this, being probably assisted by the moisture on the surface of the wood, it glanced over the royal pole to the head of the top-

---

* See a letter in the Annals for January last, by Lieutenant Sullivan, R. N., who witnessed these effects.

gallant mast at *b*, where it found intermediate metallic assistance in the copper funnel for the top-gallant rigging: from this, the resistance in the mass of the wood appears to have been less than that on its surface, probably from the long interval of air between the funnel and conducting bodies about the cap below, the mast was therefore split open as far as the cap at *c*. Here again it was enabled to strike over the surface of the mast, upon the metals about the parrel of the top-sail-yard at *d*, where the accumulation became again concentrated, producing a powerful expansion and heating effect so far as the lower cap at *e*; and thus it passed along *per saltum* over the lower mast *m*. from one metallic mass to another, until within a striking distance *s* of the sea and hull, it divided upon the hull and sea in convenient directions *s n*, *s o*, *s p*. In this course, as indicated by the waving black line *a*, *b*, *c*, *d*, &c., it evidently sought assistance from all the conducting matter it could seize upon; such as the wet ropes, the copper funnel for top-gallant rigging at *b*, the iron work and other bodies about the topmast cap at *c*, as also the men in the top-gallant crosstrees at *c*. The charge evidently divided upon them in proportion to the assistance each could afford as a small auxiliary circuit, as Exp. 3; the men nearest the mast would be necessarily in the more direct course of the discharge, the others would be more or less so according to their respective positions; that these poor fellows who were killed suffered in this way as being conductors to parts of the charge is evident from the appearance of the bodies. Mr. Sturgeon calls especial attention to the circumstance of the men being thrown in opposite directions, and thinks it remarkable: but how could it be otherwise? the intervening air being caused to expand violently from a central point, would necessarily operate as a central force; surely there is nothing very new in this.* About the parrel of the topsail-yard at *d*, we should expect again power effects; for here again the charge became concentrated, and set the sail, &c., on fire. This is quite in accordance with the known laws of electrical action; thus we find the points of ingress and egress of an artificial charge, when caused to fall on a slip of gold-leaf or other matter, are always those in which the most powerful effect arises; and when we desire to fire inflammable matter by electricity we place it directly between detached metallic points.

22. The circumstance of the lightning striking over portions of the wet mast without damage, is precisely the same effect as observed in certain cases of artificial electrical discharges.

---

* Certainly nothing new, merely an instance of the effects of *lateral* explosion of the first kind. Edit.

Thus a very slight film of moisture will allow a jar intensely charged to discharge a luminous ball over a long strip of glass. Dr. Franklin found he could destroy a dry rat by an electrical shock when he failed to hurt a wet one. If we continue to follow the discharge we find similar expansive and destructive effects; such as the bursting of the hoops on the mast, &c., &c., which will sometimes occur and sometimes not.

23. There is really nothing in all this to call for especial remark, except we may observe, as shown by the experiments already described, that if a good capacious conductor had been incorporated with the mast from the truck to the metallic masses in the hull and to the sea, then these *expansive* and destructive effects could not possibly have occurred; since the interrupted circuit would have been avoided, and the intense electrical action have vanished, or nearly so, at the mast-head, for it would have no longer been driven to force its way in a dense explosive form to the hull and sea; of this we have the most complete evidence from experience, particularly in the cases of the ships struck by lightning having such conductors as those just alluded to, curiously enough quoted by Mr. Sturgeon as evidence to the contrary. It seems a strange way of disproving a fact to quote those who, having been eye-witnesses, insist upon its truth. That the electric matter finally distributed itself upon the hull as *well* as on the sea, is evident from the circumstance of the casing of Hearle's pump at *t*, which led through the side under water being shivered; from the vivid electrical sparks below, and from the usual smell of sulphur in the well, and appearance of smoke in the orlop-deck.

24. The interrupted circuit therefore to be traced here, is first from the vane-spindle to the copper funnel of top-gallant rigging; 2nd, from this to the conducting bodies at the heel of the top-gallant mast; 3rd, thence to the metallic masses about the parrel of topsail-yard; 4th, between this and the metallic bodies about the head of lower mast; 5th, from this over the detached metallic bodies on lower mast; finally, from lower mast to the hull and sea. The effect of this shock of lightning appears to have been somewhat palliated by heavy rain.

Although Mr. Sturgeon has gone far out of his way to twist these phenomena into an accordance with certain theoretical views, and sets them up as being of an extraordinary kind, they are nevertheless of a very simple character, and are merely illustrative of a few well-known laws of electrical action.

(To be Continued)

LXXII. *Mr. Sturgeon's fourth Letter to W. Snow Harris, Esq. on the subject of Marine Lightning Conductors.*

Sir,

I had expected that the fury of your wrath against the exposé, contained in my fourth memoir, of the probable danger and unnecessary expense consequent upon your plan of lightning conductors being established in the royal navy, had been totally vented in your first unprecedented volley of abuse; but in this expectation, as well as in that of your being a scientific reasoner, I have been sadly disappointed; for instead of keeping "close quarters," and observing that strict candour which ought to be held sacred in scientific discussion, and especially on a topic of such high national importance as that of marine lightning conductors, you still keep raving on, as if determined, by your coarse bullying language, to crush every attempt to scrutinize your plan of conductors, or any notice that may be taken of those errors into which you have obviously fallen. Such asperous domineering may probably be suitable enough in your hands, as it is the principal weapon you employ. But as I am not in possession of any of the kind, nor of any desire to *shine* in a contest of such an ignoble character, I most willingly acknowledge *you* as master of that part of the field.

That point settled, I must now solicit your attention to a few particulars of a somewhat more important character, and first of all to your own confession of your own ignorance of certain points of electric action. *Occasionally* you deny the existence of *lateral* discharges in toto; and, *occasionally*, you admit some kinds of lateral discharges and deny others. Perhaps you will acknowledge that the mere denial of a fact is no proof of its non-existence; and that it may as possibly be grounded on the mere ignorance of the party denying it. Moreover, your denying a fact at one time and acknowledging it at another, is no sure indication of your accuracy, in either case, emanating from a sound judgment.

You have admitted, however, in page 317 of this volume that there is such a phenomenon as a *lateral explosion*, and you have admitted also, that this lateral explosion produces mechanical action, hence, you have, *indirectly*, admitted all that I have advanced concerning lateral discharges of the *first kind*.* For, although you seem to have no idea at all of an *electrical wave*, you ought to have known that such a wave must necessarily be produced by the expansive force of the explosion; and had you been that *practical* man, that I have all along expected you were, you would have known that a gold leaf electroscope properly exposed would indicate an electrical *wave* during the occurrence of a flash of lightning; and that a similar wave is produced by artificial discharges. Such facts, however, appearing to be quite unknown to you I shall not, here, trouble you more about them.

---

* See paragraph 193 of my fourth Memoir, page 171 of this volume.

I cannot but admire your mode of attack in the fourth paragraph of your second production on this troublesome memoir of mine. You seem to be labouring under some uneasiness about my few "*clumsy experiments*," which notwithstanding your self-sufficient strong position in the scientific world, seem, by paragraph 2, to have produced an apprehension that they may possibly open the eyes of those whom *your elegant experiments* before the Navy Board, and *highly scientific illustrations* of the effects of lightning on ships' masts before the British Association, the members of the United Service museum, and other bodies, have so long been blinding.

Now, it so happens, that those few "clumsy experiments" of mine, "with some every-day phenomena of the electrical kite," are the very facts which an inventor of a lightning rod ought to be perfectly acquainted with; and I verily believe that, had *you* been sufficiently familiar with the "every-day phenomena," as you are pleased to call them, you could never have been led to the persuasion that the effects experienced on board the Beagle and the Dryad were any indications of those ships being struck by lightning. Every sailor knows well that ships are severely shaken by a peal of vicinal thunder, though no lightning strikes the vessel; and if you would condescend to repeat some of my "clumsy experiments" with the electrical kite, during a lightning storm, you would soon learn that the "hissing noise" and other phenomena, witnessed on board the Beagle and the Dryad; are the constant productions of electric waves in the atmosphere; sometimes from flashes of lightning and sometimes from the mere transit of a cloud over the kite. Moreover, a true indication of the Beagle being *not struck* by the primative discharge is, that neither her compasses nor her chronometers were magnetized by the event: for had the main-masts' conductor transmitted a flash of lightning as has been supposed, no circumstance hitherto brought to notice, could possibly have prevented the magnetization of the steel in the chronometer which was placed so near to that conductor.

Again, whatever may be your opinion of those "every-day phenomena of the electric kite," I have always considered that some of those which I have recorded are very far from being deserving of that epithet you have given them. They are obviously such as you never saw, and, I believe, they are such phenomena as you are unable to shew recorded by any other person. My electric kite experiments have probably been more extensive than those of any other person of the present day; and, to me, they have been more productive of correct views of electric action generally, than any series of experiments I ever before pursued; and led me to other investigations which otherwise I might not have thought of.

By the copious discharges occasionally exhibited at my kite-string, I gained a knowledge of atmospheric electrical waves, and of the causes of their production.* By an attention to the motions of the balls of a Cavallo's electro-scope, I have gained a knowledge of the variableness of the density of atmospheric electricity in windy weather. By prosecuting my kite experiments at all seasons of the year, for about six successive years, I have been enabled to foretel at what season of the year, and under what circumstances of weather, the atmosphere would be most powerfully electric with respect to the ground.

By making my experiments in places remote from each other, upon lofty mountains and in low valleys, I have been enabled to understand that an unclouded atmosphere is *constantly electro-positive* with reference to the earth. By studying this fact in connexion with electric waves, I have been led to a knowledge of the cause of the ground being *sometimes* electro-positive with respect to the air immediately above it. And by these and other fluctuations of the atmospheric electrical pressure, arising from hygrometrical, and thermometrical changes, &c., I have been enabled to understand the cause of the ever-varying electrical condition of bodies composing the surface of the earth.

By employing several kites at the same time, at different altitudes, at different seasons of the year, I gained a knowledge of the different electric conditions of atmospheric strata at those altitudes: and from a knowledge of the atmosphere being differently electric at different altitudes, I was led to infer that an exceedingly thin stratum of air would be differently electric on its upper and lower surface.—Keeping this idea in view whilst repeating some of the beautiful experiments described in Sir Humphrey Davy's Bakersian Lecture for 1826, I was led to suspect that thin strata, or films, of metallic bodies might possibly exhibit different electric action on their opposite surfaces, which I found to be the case, and in the year 1827, I constructed my dry electric column, having one metal only; each piece having a *relatively* positive and negative surface.*

---

* See my fourth Memoir, page 181 of this volume.

From my success with the dry electric column, I was led to a "few clumsy experiments" in galvanism; by means of which I shewed that *metallic contact* is not essentially necessary to the production of galvanic action.† This discovery was thought of sufficient importance by Dr. Faraday, to deserve a place in the Transactions of the Royal Society, and to select it from my book as a fit subject for the theme of his 8th Series; forgetting, however, to associate the name of the discoverer with the fact. It was by contemplating the electrical character of the same kind of metal under different states of polish, texture, &c., that I was led to the discovery of making active galvanic combinations with one kind of metal only, and of shewing that *cast* and *rolled* zinc are in different electric conditions. It was in consequence of this discovery that I was enabled to shew that rolled, or hammered zinc in combination with copper, made more powerful galvanic batteries, than *cast* zinc, not so treated, would make in combination with that metal.‡ This discovery was also honoured with a place in the Philosophical Transactions of the Royal Society through the courtesy of Dr. Faraday, who, considering it sufficiently important to form a prominent feature of his own dexterity in the tactics of transplantation, very politely handed it to the Council of the Royal Society as a discovery of his own, placing it very conspicuously in his 10th Series.

The "every day phenomena at the electrical kite," which gave me the first idea of thin strata being differently electrical on their opposite sides, led me to the supposition that the thinnest films which constitute metallic crystalline groups might also be in different electrical conditions. This idea led me to an extensive series of "clumsy experiment" which were perfectly successful in shewing that each separate metal is susceptible of exhibiting thermo-electric currents, and that each group of the crystalline films, is, in fact, an electrical pile, as decidedly as any electric pile formed of two or more distinct kinds of metal.*

---

* See my Experimental Researches in Electro-Magnetism, Galvanism, &c. p. 64. Published in 1830, by Sherwood, Gilbert and Piper.

† ibid page 21, 83, and 84.

‡ See my Experimental Researches, &c. page 65—71.

These few specimens of the consequences of *some* of my "clumsy experiments" and "every-day phenomena at the electric kite," may probably afford you an idea of their having been viewed, by Dr. Faraday and others, under a very different aspect to that which would fain place them in. And, indeed, from the tenour of your first letter to me,† I have every reason to think that, your present disingenuity and want of candour are the mere effects of the lamentable *Electrophobia* under the torments of which my fourth memoir has so unhappily placed you: and that, as the ardour of the fever abates, I am in hopes your mind will gradually be restored to its usual healthy tone of vigour and conscientiousness; and resume its capability of appreciating the labours of those who, even under inexpressible disadvantages, have so long been working with you in the same field of science. Hence it is that, notwithstanding the violations of courtesy and candour which you have manifested during the impulse of those fervent paroxysms under which you have been unhappily labouring, I most willingly and sincerely exonerate you from all blame in this temporary misunderstanding; and you may rest perfectly assured, that, whenever liberality and candour again emanate from your pen, they will be accompanied by my best wishes for your welfare and success.

I have the honour to be

Sir,

Your obedient Servant,

WILLIAM STURGEON.

To W. Snow Harris, Esq.

P.S.—The state of the controversy will be seen on the next page.

---

* Philosophical Magazine.

† See page 325 of this volume.

THE ANNALS
OF
ELECTRICITY, MAGNETISM,
AND CHEMISTRY;
AND
Guardian of Experimental Science.

SEPTEMBER, 1840.

XXIII.—*Experimental Researches in Electricity.—Twelfth Series.*

By MICHAEL FARADAY, Esq. D. C. L. F. R. S. Fullerian Prof. Chem. Royal Institution, Cor. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburg, Florence, Copenhagen, Berlin, &c. &c.

Received January 11,—Read February 8, 1838.

¶ ix. *Disruptive discharge continued—Insulation—Spark—Brush—Difference of discharge at the positive and negative surfaces of conductors.*

1392. When a spark had passed at either interval, then, generally, more tended to appear at the *same* interval, as if a preparation had been made for the passing of the latter spark. So also on continuing to work the machine quickly the sparks generally followed at the same place. This effect is probably due in part to the warmth of the air heated by the preceding spark, in part to dust, and I suspect in part to something unperceived as yet in the circumstances of discharge.

1393. A very remarkable difference, which is *constant* in its direction, occurs when the electricity communicated to the balls *s* and S is changed from positive to negative, or in the contrary direction. It is that the range of variation is always greater when the small balls are positive than when they are negative. This is exhibited in the following Table, drawn from the former experiments.

	Pos.	Neg.
In Air the range was	0·19	0·09
Oxygen	0·19	0·02
Nitrogen	0·13	0·11
Hydrogen	0·14	0·05
Carbonic acid	0·16	0·02
Olefiant gas	0·22	0·08
Coal gas	0·24	0·12
Muriatic acid	0·43	0·08

I have no doubt these numbers require considerable correction, but the general result is striking, and the differences in several cases very great.

1394. Though, in consequence of the variation of the striking distance (1386.), the interval in air fails to be a measure, as yet, of the insulating or resisting power of the gas in the vessel, yet we may for present purposes take the mean interval as representing in some degree that power. On examining these mean intervals as they are given in the third column (1388.), it will be very evident, that gases, when employed as dielectrics, have peculiar electrical relations to insulation, and therefore to induction, very distinct from such as might be supposed to depend upon their mere physical qualities of specific gravity or pressure.

1395. First, it is clear that at the *same pressure* they are not alike, the difference being as great as 37 and 110. When the small balls are charged positively, and with the same surfaces and the same pressure, muriatic acid gas has three times the insulating or restraining power (1362.) of hydrogen gas, and nearly twice that of oxygen, nitrogen, or air.

1396. Yet it is evident that the difference is not due to specific gravity, for though hydrogen is the lowest, and therefore lower than oxygen, oxygen is much beneath nitrogen, or than olefiant gas, and carbonic acid gas, though considerably heavier than olefiant gas or muriatic gas, is lower than either. Oxygen as a heavy, and olefiant as a light gas, are in strong contrast with each other; and if we may reason of olefiant gas from Harris's results with air (1365.), then it might be rarefied to two-thirds its usual density, or to a specific gravity of 9·3 (hydrogen being 1), and having neither the same density, nor pressure as oxygen, would have equal insulating powers with it, or equal tendency to resist discharge.

1397. Experiments have already been described (1291.

1292.) which shew that the gases are sensibly alike in their inductive capacity. This result is not in contradiction with the existence of great differences in their restraining power. The same point has been observed already in regard to dense and rare air (1375.).

1398. Hence arises a new argument proving that it cannot be mere pressure of the atmosphere which prevents or governs discharge (1377. 1378.) but a specific electric quality or relation of the gaseous medium. Hence also additional argument for the theory of molecular inductive action.

1399. Other specific differences amongst the gases may be drawn from the preceding series of experiments, rough and hasty as they are. Thus the positive and negative series of mean intervals do not give the same differences. It has been already noticed that the negative numbers are lower than the positive (1393.), but besides that, the *order* of the positive and negative results is not the same. Thus on comparing the mean numbers (which represent for the present insulating tensions) it appears that in air, hydrogen, carbonic acid, olefiant gas, and muriatic acid, the tension rose higher when the smaller ball was made positive than when rendered negative, whilst in oxygen, nitrogen, and coal gas, the reverse was the case. Now though the numbers cannot be trusted as exact, and though air, oxygen, and nitrogen should probably be on the same side, yet some of the results, as, for instance, those with muriatic acid, fully shew a peculiar relation and difference among gases in this respect. This was further proved by making the interval in air 0·8 of an inch whilst muriatic acid gas was in the vessel *a*; for on charging the small balls *s* and S positively, *all* the discharge took place through the *air*; but on charging them negatively, *all* the discharge took place through the *muriatic acid gas.*

1400. So also, when the conductor *n* was connected *only* with the muriatic acid gas apparatus, it was found that the discharge was more facile when the small ball *s* was negative than when positive; for in the latter case, much of the electricity passed off as brush discharge through the air from the connecting wire *p*; but in the former case, it all seemed to go through the muriatic acid.

1401. The consideration, however, of positive and negative discharge across air and other gases will be resumed in the further part of this, or in the next paper.

1402. Here for the present I must leave this part of the subject, which had for its object only to observe how far gases agreed or differed as to their power of retaining a charge on bodies acting by induction through them. All the results conspire to shew that Induction is an action of contiguous molecules (1295. &c.); but besides confirming this, the first principle placed for proof in the present inquiry, they greatly assist in developing the specific properties of each gaseous dielectric, at the same time shewing that further and extensive experimental investigation is necessary, and holding out the promise of new discovery as the reward of the labour required.

1403. When we pass from the consideration of dielectrics like the gases to that of bodies having the liquid and solid condition, then our reasonings in the present state of the subject assume much more of the character of mere supposition. Still I do not perceive anything adverse to the theory in the phenomena which such bodies present. If we take three insulating dielectrics, as air, oil of turpentine and shell-lac and use the same balls or conductors at the same intervals in these three substances, increasing the intensity of the induction until discharge take place, we shall find that it must be raised much higher in the fluid than for the gas, and higher still in the solid than for the fluid. Nor is this inconsistent with the theory; for with the liquid, though its molecules are free to move almost as easily as those of the gas, there are many more particles introduced into the given interval; and as respects the latter circumstance, the same is the case when the solid body is employed. Besides that, the cohesive force of the body used will produce some effect; for though the production of the polarized states in the particle of a solid may not be obstructed, but, on the contrary, may in some cases be even favored (1164. 1344.) by its solidity or other circumstances, yet solidity may well exert an influence on the point of its final subversion, (just as it prevents discharge in an electrolyte,) and so enable inductive intensity to rise to a much higher degree.

1404. In the cases of solids and liquids too, bodies may, and most probably do, possess specific differences as to their ability of assuming the polarized state, and also as to the extent to which that polarity must rise before discharge occurs. An analogous difference exists in the specific inductive capacities already pointed out in a few substances (1278.) in the last paper. Such a difference might even account for the various degrees of insulating and conducting power possessed by different bodies, and, if it should be found to exist, would add further strength to the argument in favor of the molecular theory of inductive action.

1405. Having considered these various cases of sustained insulation in non-conducting dielectrics up to the highest point which they can attain, we find that they terminate at last in *disruptive discharge;* the peculiar condition of the molecules of the dielectric which was necessary to the continuous induction, being equally essential to the occurrence of that effect which closes all the phenomena. This discharge is not only in its appearance and condition different to the former modes by which the lowering of the powers was effected (1320. 1343.), but, whilst really the same in principle, varies much from itself in certain characters, and thus presents us with the forms of *spark, brush* and *glow* (1359.). I will first consider *the spark,* limiting it for the present to the case of discharge between two oppositely electrified conducting surfaces.

The electric spark or flash.

1406. The *spark* is a discharge or lowering of the polarized inductive state of many dielectric particles, by a particular action of a few of the particles occupying a very small and limited space; all the previously polarized particles returning to their first or normal condition in the inverse order in which they left it, and uniting their powers meanwhile to produce, or rather to continue, (1417 and 1436.) the discharge effect in the place where the subversion of force first occurred. My impression is, that the few particles situated where discharge occurs are not merely pushed apart, but assume a peculiar state, a highly exalted condition for the time, i. e. have thrown upon them all the surrounding forces in succession, and rising up to a proportionate intensity of condition, perhaps equal to that of chemically combining atoms, discharge the powers, possibly in the same manner as they do theirs, by some operation at present unknown to us; and so the end of the whole. The ultimate effect is exactly as if a metallic wire had been put into the place of the discharging particles; and it does not seem impossible that the principles of action in both cases may, hereafter, prove to be the same.

1407. The *path of the spark,* or of the discharge, depends on the degree of tension acquired by the particles in the line of discharge, circumstances, which in every common case are very evident and by the theory easy to understand, rendering it higher in them than in their neighbours, and, by exalting them first to the requisite condition, causing them to determine on the course of the discharge. Hence the selection of the path, and the solution of the wonder which Harris has so well described* as existing under the old theory. All is prepared amongst the molecules beforehand, by the prior induction, for the path either of the electric spark* or of lightning itself.

1408. The same difficulty is expressed as a principle by Nobili for voltaic electricity, almost in Mr. Harris's words, namely,† "electricity directs itself towards the point where it can most easily discharge itself," and the results of this as a principle he has well wrought out for the case of voltaic currents. But the *solution* of the difficulty, or the proximate cause of the effects, is the same: induction brings the particles up to or towards a certain state (1370.); and by those which first attain it, is the discharge first and most efficiently performed.

1409. The *moment* of discharge is probably determined by that molecule of the dielectric which, from the circumstances, has its tension most quickly raised up to the maximum intensity. In all cases where the discharge passes from conductor to conductor this molecule must be on the surface of one of them; but when it passes between a conductor and a non-conductor, it is, perhaps, not always so (1453.). When this particle has acquired its maximum tension, then the whole barrier of resistance is broken down in the line or lines of inductive action originating at it, and disruptive discharge occurs (1370.): and such an inference, drawn as it is from the theory, seems to me in accordance with Mr. Harris's facts and conclusions respecting the resistance of the atmosphere, namely, that it is not really greater at any one discharging distance than another.‡

1410. It seems probable, that the tension of a particle of the same dielectric, as air, which is requisite to produce discharge, is a *constant quantity*, whatever the shape of the part of the conductor with which it is in contact, whether ball or point; whatever the thickness or depth of dielectric throughout which induction is exerted; perhaps, even, whatever the state, as to rarefaction or condensation of the dielectric; and whatever the nature of the conductor, good or bad, with which the particle is for the moment associated. In saying so much, I do not mean to exclude small differences which may be caused by the reaction of neighbouring particles on the deciding particle, and indeed, it is evident that the intensity required in a particle must be related to the condition of those which are contiguous. But if the expectation should be found to approximate to truth, what a generality of character it presents! and, in the definiteness of the power possessed by a particular molecule, may we not hope to find an immediate relation to the force which, being electrical, is equally definite and constitutes chemical affinity?

---

* Nautical Magazine, 1834, p. 229.

† Bibliotheque Universelle, 1835 lix. 275.

‡ Philosophical Transactions, 1834. pp. 227, 229.

1411. Theoretically it would seem that, at the moment of discharge by the spark in one line of inductive force, not merely would all the other lines throw their forces into this one (1406.), but the lateral effect, equivalent to a repulsion of these lines (1224. 1297.), would be relieved and, perhaps, followed by something equivalent to a contrary action, amounting to a collapse or attraction of these parts. Having long sought for some transverse force in statical electricity, which should be the equivalent to magnetism or the transverse force of current electricity, and conceiving that it might be connected with the transverse action of the lines of inductive force already described (1297.), I was desirous, by various experiments, of bringing out the effect of such a force, and making it bear upon the phenomena of electro-magnetism and magneto-electricity.

1412. Amongst other results, I expected and sought for the mutual affection, or even the lateral coalition of two similar sparks, if they could be obtained simultaneously side by side, and sufficiently near to each other. For this purpose, two similar Leyden jars were supplied with rods of copper projecting from their balls in a horizontal direction, the rods being about 0·2 of an inch thick, and rounded at the ends. The jars were placed upon a sheet of tinfoil, and so adjusted that their rods, *a* and *b*, were near together, in the position represented in plan at fig. 2. *c* and *d* were two brass balls connected by a brass rod and insulated; *e* was also a brass ball connected, by a wire, with the ground and with the tinfoil upon which the Leyden jars were placed. By laying an insulated metal rod across from *a* to *b*, charging the jars, and removing the rod, both the jars could be brought up to the same intensity of charge (1370.). Then, making the ball *e* approach the ball *d*, at the moment the spark passed there, two sparks passed between the rods *n*, *o*, and the ball *c*; and as far as the eye could judge, or the conditions determine, they were simultaneous.

1413. Under these circumstances two modes of discharge took place; either each end had its own, particular spark to the ball, or else one end only was associated by a spark with the ball, but was at the same time related to the other end by a spark between the two.

1414. When the ball *c* was about an inch in diameter, the ends, *n* and *o*, about half an inch from it, and about 0·4 of an inch from each other, the two sparks to the ball could be obtained. When, for the purpose of bringing the sparks nearer together, the ends, *n* and *o* were brought closer to each other, then, unless very carefully adjusted, only one end had a spark with the ball, the other having a spark to it; and the least variation of position would cause either *n* or *o* to be the end which, giving the direct spark to the ball, was also the one through, or by means of which, the other discharged its electricity.

1415. On making the ball *c* smaller, I found that then it was needful to make the interval between the *n* and *o* larger in propotion to the distance between them and the ball *c*. On making *c* larger, I found I could diminish the interval, and so bring the two simultaneous separate sparks closer together, until, at last, the distance between them was not more at the widest part than 0.6 of their whole length.

1416. Numerous sparks were then passed and carefully observed. They were very rarely straight, but either curved or bent irregularly. In the average of cases they were I think, decidedly convex towards each other; perhaps two thirds presented more or less of this effect, the rest bulging more or less outwards. I was never able, however, to obtain sparks which, separately leaving the ends of the wires *n* and *o*, conjoined into one spark before they reached or communicated with the ball *c*. At present, therefore, though I think I saw a tendency in the sparks to unite, I cannot assert it as a fact.

1417. But there is one very interesting effect here analogous to, and it may be in part the same with, that I was searching for; I mean the increased facility of discharge where the spark passes. For instance, in the cases where one end, as *n*, discharged the electricity of both ends to the ball *c*, fig. 2., the electricity of the other end *o*, had to pass through an interval of air 1·5 times as great as that which it might have taken, by its direct passage between the end and the ball itself. In such cases, the eye could not distinguish, even by the use of WHEATSTONE's means*, that the spark from the end *n* which contained both portions of the electricity, was a double spark. It could not have consisted of two sparks taking separate courses, for such an effect would have been visible to the eye; but it is just possible, that the spark of the first end *n* and its jar, passing at the smallest interval of time before that of the other *o*, had heated and expanded the air in its course, and made it so much more favorable to discharge, that the electricity of the end *o* preferred leading across to it and taking a very circuitous route, rather than the more direct one to the ball. It must, however, be remarked, in answer to this supposition, that the one spark between *d* and *e* would, by its influence, tend to produce simultaneous discharges at *n* and *o*, and certainly did so, when no preponderance was given to one wire over the other, as to the previous inductive effect (1414.).

---

* Philosophical Transactions, 1834, pp. 584, 585.

1418. The fact, however, is, that disruptive discharge is favorable to itself. It is at the outset a case of tottering equilibrium: and if *time* be an element in discharge, in however minute a proportion (1436.), then the commencement of the act at any point favors its continuance and increase there, and portions of power will be discharged by a course which they would not otherwise have taken.

1419. The mere heat and expansion of the air itself by the the first portion of electricity which passes, must have a great influence in producing this result.

1420. As to the result itself, we see its influence in every spark that passes; for it is not the whole quantity which passes that determines the discharge, but merely that small portion of force which brings the deciding molecule (1370.) up to its maximum tension; then when its forces are subverted and discharge begins, all the rest passes by the same course, from the influence of the favoring circumstances just referred to; and whether it be the electricity on a square inch, or a thousand square inches of charged glass, the discharge is complete. Hereafter we shall find the influence of this effect in the formation of brushes (1435.); and it is not impossible that we may trace it producing the jagged spark and the forked lightning.

1421. The characters of the electric spark in *different gases* vary, and the variation *may* be due simply to the effect of the heat evolved at the moment. But it may also be due to that specific relation of the particles and the electric forces which I have assumed as the basis of a theory of induction; the facts do not oppose such a view; and in that view, the variation strengthens the argument for molecular action, as it would seem to shew the influence of the latter in every part of the electrical effect (1423, 1454).

1422. The appearances of the sparks in different gases have often been observed and recorded,* but I think it not out of place to notice briefly the following results; they were obtained with balls of brass, (platina surfaces would have been better,) and at common pressures. In *air*, the sparks have that intense light and bluish colour which are so well known, and often have faint or dark parts in their course, when the quantity of electricity passing is not great. In *nitrogen*, they are very beautiful, having the same general appearance as in air, but have decidedly more colour of a bluish or purple character, and I thought were remarkably sonorous. In *oxygen*, the sparks were whiter than in air or nitrogen, and I think not so brilliant. In *hydrogen*, they had a very fine crimson colour, not due to its rarity, for the character passed away as the atmosphere was rarefied (1459.).* Very little sound was produced in this gas; but that is a consequence of its physical condition.† In *carbonic acid gas*, the colour was similar to that of the spark in air, but with a little green in it: the sparks were remarkably irregular in form, more so than in common air: they could also, under similar circumstances as to size of ball, &c. be obtained much longer than in air, the gas shewing a singular readiness to pass the discharge in the form of spark. In *muriatic acid gas*, the spark was nearly white: it was always bright throughout, never presenting those dark parts which happen in air, nitrogen, and some other gases. The gas was dry, and during the whole experiment the surface of the glass globe within remained quite dry and bright. In *coal gas*, the spark was sometimes green, sometimes red, and occasionally one part was green and another red. Black parts also occur very suddenly in the line of the spark, i. e. they are not connected by any dull part with bright portions, but the two seem to join directly one with the other.

---

* See Van Marum's description of the Teylerian Machine, vol. i. p. 112. and vol. ii. p. 196.; also Ency. Britan. vol. vi., Article Electricity, pp. 505. 507.

1423. These varieties of character impress my mind with a feeling, that they are due to a direct relation of the electric powers to the particles of the dielectric through which the discharge occurs, and are not the mere results of a casual ignition or a secondary kind of action of the electricity, upon the particles which it finds in its course and thrusts aside in its passage (1454.).

1424. The spark may be obtained in media which are far denser than air, as in oil of turpentine, olive oil, resin, glass, &c.: it may also be obtained in bodies which being denser likewise approximate to the condition of conductors, as spermaceti, water, &c. But in these cases, nothing occurs which, as far as I can perceive, is at all hostile to the general views I have endeavored to advocate.

---

* Van Marum, says they are about four times as large in hydrogen as in air, vol. i. p. 122.

† Leslie.

The electrical brush.

1425. The *brush* is the next form of disruptive discharge which I will consider. There are many ways of obtaining it, or rather of exalting its characters; and all these ways illustrate the principles upon which it is produced. If an insulated conductor, connected with the positive conductor of an electrical machine, have a metal rod 0·3 of an inch in diameter, projecting from it outwards from the machine, and terminating by a rounded end or a small ball, it will generally give good brushes; or, if the machine be not in good action, then many ways of assisting the formation of the brush can be resorted to; thus, the hand, or any *large* conducting surface may be approached towards the termination to increase inductive force (1374.): or the termination may be smaller and of badly conducting matter, as wood: or sparks may be taken between the prime conductor of the machine and the secondary conductor to which the termination giving brushes belongs: or, which gives to the brushes exceedingly fine characters and great magnitude, the air around the termination may be rarefied more or less, either by heat or the air pump; the former favorable circumstances being also continued.

1426. The brush when obtained by a powerful machine on a ball about 0·7 of an inch in diameter, at the end of a long brass rod attached to the positive prime conductor, had the general appearance as to form represented in fig. 3.: a short conical bright part or root appeared at the middle part of the ball projecting directly from it, which at a little distance from the ball, broke out suddenly into a wide brush of pale ramifications having a quivering motion, and being accompanied at the same time with a low dull chattering sound.

1427. At first the brush seems continuous, but Professor Wheatstone has shewn that the whole phenomenon consists of successive intermitting discharges.* If the eye be passed rapidly, not by a motion of the head, but of the eyeball itself, across the direction of the brush, by first looking steadfastly about 10° or 15° above, and then instantly as much below it, the general brush will be resolved into a number of individual brushes, standing in a row upon the line which the eye passed over; each elementary brush being the result of a single discharge, and the space between them representing both the time during which the eye was passing over that space, and that which elapsed between one discharge and another.

---

* Philosophical Transactions, 1834, p. 586.

1428. The single brushes could easily be separated to eight or ten times their own width, but were not at the same time extended, i. e. they did not become more indefinite in shape, but, on the contrary, less so, each being more distinct in form, ramification, and character, because of its separation from the from the others, in its effects upon the eye. Each, therefore, was instantaneous in its existence (1436.). Each had the conical root complete (1426.).

1429. On using a smaller ball, the general brush was smaller, and the sound, though weaker, more continuous. On resolving the brush into its elementary parts, as before, these were found to occur at much shorter intervals than in the former case, but still the discharge was intermitting.

1430. Employing a wire with a round end, the brush was still smaller, but, as before, separable into successive discharges. The sound, though feebler, was higher in pitch, being a distinct musical note.

1431. The sound is, in fact, due to the recurrence of the noise of each separate discharge, and these, happening at intervals nearly equal under ordinary circumstances, cause a definite note to be heard, which rising in pitch with the increased rapidity and regularity of the intermitting discharges, gives a ready and accurate measure of the intervals, and so may be used in any case when the discharge is heard, even though the appearances may not be seen, to determine the element of *time.* So, also, when, by bringing the hand towards a projecting rod or ball, the pitch of the tone produced by a brushy discharge increases, the effect informs us that we have increased the induction (1374.), and by that means increased the rapidity of the alternations of charge and discharge.

1432. By using wires with finer terminations, smaller brushes were obtained, until they could hardly be distinguished as brushes; but as long as *sound* was heard, the discharge could be ascertained by the eye to be intermitting; and when the sound ceased, the light became *continuous* as a glow (1359. 1405.).

1433. To those not accustomed to use the eye in the manner I have described, or, in cases where the recurrence is too quick for any unassisted eye, the beautiful revolving mirror of Professor Wheatstone* will be useful for such developments of condition as those mentioned above. Another excellent process is to produce the brush or other luminous phenomenon on the end of a rod held in the hand opposite to a charged positive or negative conductor, and then move the rod rapidly from side to side whilst the eye remains still. The successive discharges occur of course in different places, and the state of things before, at, and after a single coruscation or brush can be exceeding well separated.

---

* Philosophical Transactions, 1834, pp. 584, 585.

1434. The *brush* is in reality a discharge between a bad or a non-conductor and either a conductor or another non-conductor. Under common circumstances, the brush is a discharge between a conductor and air, and I conceive it to take place in some thing like the following manner. When the end of an electrified rod projects into the middle of a room induction takes place between it and the walls of the room, across the dielectric, air; and the lines of inductive force accumulate upon the end in greater quantity than elsewhere, or the particles of air at the end of the rod are more highly polarized than those at any other part of the rod, for the reasons already given (1374.). The particles of air situated in sections across these lines of force are least polarized in sections towards the walls, and most polarized in those nearer to the end of the wires (1369.): thus, it may well happen, that a particle at the end of the wire is at a tension that will immediately terminate in discharge, whilst in those even only a few inches off, the tension is still beneath that point. But suppose the rod to be charged positively, a particle of air A, *fig.* 4. next it, being polarized, and having of course its negative force directed towards the rod and its positive force outwards; the instant that discharge takes place between the positive force of the particle of the rod opposite the air and the negative force of the particle of air towards the rod, the whole particle of air becomes positively electrified; and when, the next instant, the discharged part of the rod resumes its positive state, by conduction from the surface of metal behind, it not only acts on the particles beyond A, by throwing A into a polarized state again, but A itself, because of its charged state, exerts a distinct inductive act towards these further particles, and the tension is consequently so much exalted between A and B, that discharge takes place there also, as well as again between the metal and A.

1435. In addition to this effect, it has been shewn, that, the act of discharge having once commenced, the whole operation, like a case of unstable equilibrium, is hastened to a conclusion (1370. 1418.), the rest of the act being facilitated in its occurrence, and other electricity than that which caused the first necessary tension hurrying to the spot. When, therefore, disruptive discharge has once commenced at the root of a brush, the electric force which has been accumulating in the conductor attached to the rod, finds a more ready discharge there than elsewhere, and will at once follow the course marked out as it were for it, thus leaving the conductor in a partially discharged state, and the air about the end of the wire in a charged condition; and the time necessary for restoring the full charge of the conductor, and the dispersion of the charged air in a greater or smaller degree, by the joint forces of repulsion from the conductor and attraction towards the walls of the room, to which its inductive action is directed is just that time which forms the interval between brush and brush (1420. 1427. 1431.)

1436. The words of this description are long, but there is nothing in the act or the forces on which it depends to prevent its being *instantaneous*, as far as we can estimate and measure it. The consideration of *time* is, however, important in several points of view (1418.), and in reference to disruptive discharge, it seemed from theory far more probable that it might be detected in a brush than in a spark, for in a brush, the particles in the line through which the discharge passes are in very different states as to intensity, and the discharge is already complete in its act at the root of the brush, before the particles at the extremity of the ramifications have yet attained their maximum intensity.

1437. I consider brush discharge as, probably, a successive effect in this way. Discharge begins at the root (1426.), and, extending itself in succession to all parts of the single brush, continues to go on at the root and the previously formed parts until the whole brush is complete; then, by the fall in intensity and power at the conductor, it ceases at once in all parts, to be renewed, when that power has risen again to a sufficient degree. But in a spark, the particles in the line of discharge being, from the circumstances, nearly alike in their intensity of polarization, suffer discharge so nearly at the same moment as to make the time quite insensible to us.

1438. Mr. Wheatstone has already made experiments which fully illustrate this point. He found that the brush generally had a sensible duration, but that with his highest capabilities he could not detect any such effect in the spark.* I repeated his experiment on the the brush, though with more imperfect means, to ascertain whether I could distinguish a longer duration in the stem or root of the brush than in the extremities, and the appearances were such as to make me think an effect of this kind was produced.

---

* Philosophical Transactions, 1836. pp 586, 590.

1439. That the discharge breaks into several ramifications, and by them passes through portions of air alike, or nearly alike, as to polarization and the degree of tension the particles there have acquired, is a very natural result of the previous state of things, and sooner to be expected than that the discharge should continue to go straight out into space in a single line amongst those particles which, being at a distance from the end of the rod, are in a lower state of tension than those which are near: and whilst we cannot but conclude, that those parts where the branches of a single brush appear, are more favorably circumstanced for discharge than the darker parts between the ramifications, we may also conclude, that in those parts where the light of concomitant discharge is equal, there the circumstances are nearly equal also. The single brushes are by no means of the same particular shape even when they are observed without displacement of the rod or surrounding objects (1427. 1433.), and the successive discharges may be considered as taking place into the mass of air around, through different roads at each brush, according as minute circumstances, as dnst, &c. (1391. 1392.) may have favored the course by one set of particles rather than another.

1440. Brush discharge does not essentially require any current of the medium in which the brush appears: the current almost always occurs, but is a consequence of the brush, and will be considered hereafter. On holding a blunt point positively charged towards uninsulated water, a star or glow appeared on the point, a current of air passed from it, and the surface of the water was depressed; but on bringing the point so near that sonorous brushes passed, then the current of air instantly ceased, and the surface of the water became level.

1441. The discharge by a brush is not to all the particles of air that are near the electrified conductor from which the brush issues; only those parts where the ramifications pass are electrified: the air in the central dark parts between them receives no charge, and, in fact, at the time of discharge, has its electric and inductive tension considerably lowered. For consider fig. 14. to represent a single positive brush;—the induction before the discharge is from the end of the rod outwards, in diverging lines towards the distant conductors, as the walls of the room, &c., and a particle at *a* has polarity of a certain degree of tension, and tends with a certain force to become charged; but at the moment of discharge, the air in the ramifications *b* and *d*, acquiring also a positive state, opposes its influence to that of the positive conductor on *a*, and the tension of the particle at *a* is therefore diminished rather than increased. The charged particles at *b* and *d* are now inductive bodies, but their lines of inductive action are still outwards towards the walls of the room ; the direction of the polarity and the tendency of other particles to charge from these, being governed by, or in conformity with, these lines of force.

1442. The particles that are charged are probably very highly charged, but, the medium being a non-conductor, they cannot communicate that state to their neighbours. They travel, therefore, under the influence of the repulsive and attractive forces, from the charged conductor towards the nearest uninsulated conductor, or the nearest body in a different state to themselves, just as charged particles of dust would travel, and are then discharged ; each particle acting, in its course, as a centre of inductive force upon any bodies near which it may come.

1443. The travelling of these charged particles when they are numerous, causes wind and currents, but these will come into consideration under *carrying discharge* (1319.). When air is said to be electrified, and it frequently assumes this state near electrical machines, it consists, according to my view, of a mixture of electrified and un-electrified particles, the latter being in very large proportion to the former. When we gather electricity from air by a flame or by wires, it is either by the actual discharge of these particles, or by effects dependent on their inductive action, a case of either kind being produceable a pleasure. That the law of equality between the two forces or forms of force in inductive action is as strictly preserved in these as in other cases, is fully shewn by the fact, formerly stated (1173. 1174.), that, however strongly air in a vessel might be charged positively, there was an exactly equal amount of negative force on the inner surface of the vessel itself, for no residual portion of either the one or the other electricity could be obtained.

1444. I have nowhere said, nor does it follow, that the air is charged only where the luminous brush appears. The charging may extend beyond those parts which are visible, i. e. particles to the right or left of the lines of light may receive electricity, the parts which are luminous being so only because much electricity is passing by them to other parts (1437.) ; just as in a spark discharge the light is greater as more electricity passes, though it has no necessary relation to the quantity required to commence discharge (1370. 1420.).

Hence the form we see in a brush may by no means represent the whole quantity of air electrified; for an invisible portion, clothing the visible form to a certain depth, may, at the same time, receive its charge.

1445. Several effects which I have met with in muriatic acid gas tend to make me believe, that gaseous body allows of a dark discharge. At the same time, it is quite clear from theory, that in some gases, the reverse of this may occur, i. e. that the charging of the air may not extend even so far as the light. We do not know as yet enough of the electric light to be able to state on what it depends, and it is very possible that, when electricity bursts forth into air, all the particles of which are in a state of tension, light may be evolved by such as, being very near to, are not of, those which actually receive a charge at the time.

1446. The further a brush extends in a gas, the further no doubt is the charge or discharge carried forward; but this may vary between different gases, and yet the intensity required for the first moment of discharge not vary in the same, but in some other proportion. Thus with respect to nitrogen and muriatic acid gases, the former, as far as my experiments have proceeded, produces far finer and larger brushes than the latter (1458. 1462.), but the intensity required to commence discharge is much higher for the latter than the former (1395.). Here again, therefore, as in many other qualities, specific differences are presented by different gaseous dielectrics, and so prove the special relation of the latter to the act and the phenomena of induction.

1447. To sum up these considerations respecting the character and condition of the brush, I may state that it is a spark to air; a diffusion of electric force to matter, not by conduction, but disruptive discharge; a dilute spark which, passing to very badly conducting matter, frequently discharges but a small portion of the power stored up in the conductor; for as the air charged reacts on the conductor, whilst the conductor, by loss of electricity, sinks in its force, the discharge quickly ceases, until by the dispersion of the charged air and the renewal of the excited conditions of the conductor, circumstances have risen up to their first effective condition, again to cause discharge, and again to fall and rise.

1448. The brush and spark gradually pass into one another. Making a small ball positive by a good electrical machine with a large prime conductor, and approaching a large uninsulated discharging ball towards it, very beautiful variations from the spark to the brush may be obtained. The drawings of long and powerful sparks, given by

Van Marum,[*] Harris,[†] and others, also indicate the same phenomena. As far as I have observed, whenever the spark has been brushy in air of common pressures the whole of the electricity has not been discharged, but only portions of it, more or less according to circumstances: whereas, whenever the effect has been a distinct spark throughout the whole of its course, the discharge has been perfect, provided no interruption had been made to it elsewhere, in the discharging circuit, than where the spark occurred.

1449. When an electrical brush from an inch to six inches in length or more is issuing into free air, it has the form given, fig. 3. But if the hand, a ball, or any knobbed conductor be brought near, the extremities of the coruscations turn towards it and each other, and the whole assumes various forms according to circumstances, as in figs. 5, 6, and 7. The influence of the circumstances in each case is easily traced, and I might describe it here, but that I should be ashamed to occupy the time of the Society in things so evident. But how beautifully does the curvature of the ramifications illustrate the curved form of the lines of inductive force existing previous to the discharge! for the former are consequences of the latter, and take their course, in each discharge, where the previous inductive tension had been raised to the proper degree. They represent these curves just as well as iron filings represent mignetic curves, the visible effects in both cases being the consequences of the action of the forces in *the places where* the effects appear. The phenomena, therefore, constitute additional and powerful testimony (1216. 1230.) to that already given in favor both of induction through dielectrics in curved lines (1231.), and of the lateral relation of these lines, by an effect equivalent to a repulsion producing divergence, or, as in the cases figured, the bulging form.

1450. In reference to the theory of molecular inductive action, I may also add here, the proof deducible from the long brushy ramifying spark which may be obtained between a small ball on the positive conductor of an electrical machine, and a larger one at a distance (1448.). What a fine illustration that spark affords of the previous condition of *all* the particles of the dielectric between the surfaces of discharge, and how unlike the appearances are to any which would be deduced from the theory which assumes inductive action to be action at a distance, in straight lines only; and charge, as being electricity retained upon the surface of conductors by the mere pressure of the atmosphere!

---

[*] Description of the Teylerian machine, vol. i. pp. 28. 32.; vol. ii. p. 226. &c.

[†] Philosophical Transactions, 1834. p. 243.

1451. When the brush is obtained in rarefied air, the appearances vary greatly, according to circumstances, and are exceedingly beautiful. Sometimes a brush may be formed of only six or seven branches, these being broad and highly luminous, of a purple colour, and in some parts an inch or more apart:—by a spark discharge at the prime conductor (1455.) single brushes may be obtained at pleasure. Discharge in the form of a brush is favored by rarefaction of the air, in the same manner and for the same reason as discharge in the form of a spark (1375.); but in every case there is previous induction and charge through the dielectric, and polarity of its particles (1437.), the induction being, as in any other instance, alternately raised by the machine and lowered by the discharge. In certain experiments the rarefaction was increased to the utmost degree, and the opposed conducting surfaces brought as near together as possible without producing the glow: the brushes then contracted in their lateral dimensions, and recurred so rapidly as to form an apparently continuous arc of light from metal to metal. Still the discharge could be observed to intermit (1427.), so that even under these high conditions, induction preceded each single brush, and the tense polarized condition of the contiguous particles was a necessary preparation for the discharge itself.

1452. The brush form of disruptive discharge may be obtained not only in air and gases, but also in much denser media. I procured it in oil of turpentine from the end of a wire going through a glass tube into the fluid contained in a metal vessel. The brush was small and very difficult to obtain; the ramifications were simple, and stretched out from each other diverging very much. The light was exceedingly feeble, a perfectly dark room being required for its observation. When a few solid particles, as of dust or silk, were in the liquid, the brush was produced with much greater facility.

1453. The running together or coalescence of different lines of discharge (1412.) is very beautifully shewn in the brush in air. This point may present a little difficulty to those who are not accustomed to see in every discharge an equal exertion of power in opposite directions, a positive brush being considered by such (perhaps in consequence of the common phrase *direction of a current*) as indicating a breaking forth in different directions of the original force, rather than a tendency to convergence and union in one line of passage. But the ordinary case of the brush may be compared, for its illustration, with that in which, by holding the knuckle opposite to highly excited glass, a discharge occurs, the ramifications of a brush then leading from the glass and converging into a spark on the knuckle. Though a difficult experiment to make, it is possible to obtain discharge between highly excited shell-lac and the excited glass of a machine: when the discharge passes, it is, from the nature of the charged bodies, brush at each end and spark in the middle, beautifully illustrating that tendency of discharge to facilitate like action, which, I have described in a former page (1418.).

1454. The brush has *specific characters* in different gases, indicating a relation to the particles of these bodies even in a stronger degree than the spark (1422. 1423.). This effect is in strong contrast with the non-variation caused by the use of different substances as *conductors* from which the brushes are to originate. Thus, using such bodies as wood, card, charcoal, nitre, citric acid, oxalic acid, oxide of lead, chloride of lead, carbonate of potassa, potassa fuso, strong solution of potash, oil of vitriol, sulphur, sulphuret of antimony, and hæmatite, no variation in the character of the brushes was obtained, except that (dependent upon their effect as better or worse conductors) of causing discharge with more or less readiness and quickness from the machine.*

1455. The following are a few of the effects I observed in different gases at the positively charged surfaces, and with atmospheres varying in their pressure. The general effect of rarefaction was the same for all the gases: at first, sparks passed; these gradually were converted into brushes, which became larger and more distinct in their ramifications, until, upon further rarefaction, the latter began to collapse and draw in upon each other, till they formed a stream across from conductor to conductor: then a few lateral streams shot out towards the glass of the vessel from the conductors; these became thick, flossy, and soft in appearance, and were succeeded by the full constant glow which covered the discharging wire. The phenomena varied with the size of the vessel (1477.), the degree of rarefaction, and the discharge of electricity from the machine. When the latter was in successive sparks, they were most beautiful, the effect of a spark from a small machine being equal to, and often surpassing, that produced by the *constant* discharge of a far more powerful one.

---

* Exception must, of course, be made of those cases where the root of the brush, becoming a spark, causes a little diffusion or even decomposition of the matter there, and so gains more or less of a particular colour at that part.

1456. *Air.*—Fine positive brushes are easily obtained in air at common pressures, and possess the well-known purplish light. When the air is rarefied, the ramifications are very long, filling the globe (1477.), the light is greatly increased, and is of a beautiful purple colour, with an occasional rose tint in it.

1457. *Oxygen.*—At common pressures, the brush is very close and compressed, and of a dull whitish colour somewhat purplish, but all the characters very poor compared to those in air.

1458. *Nitrogen* gives brushes with great facility at the positive surface, far beyond any other gas I have tried: they are almost always fine in form, light, and colour, and in rarefied nitrogen are magnificent. They surpass the discharges in any other gas as to the quantity of light evolved.

1459. *Hydrogen*, at common pressures, gave a better brush than oxygen, but did not equal nitrogen; the colour was greenish grey. In rarefied hydrogen, the ramifications were very fine in form and distinctness, but pale in colour, with a soft and velvety appearance, and not at all equal to those in nitrogen. In the rarest state of the gas, the colour of the light was a pale grey green.

1460. *Coal gas.*—The brushes were rather difficult to produce, the contrast with nitrogen being great in this respect. They were short and strong, generally of a greenish colour, and possessing much of the spark character: for, occurring on both the positive and negative terminations, often when there was a dark interval of some length between the two brushes, still the quick, sharp sound of the spark was produced, as if the discharge had been sudden through this gas, and partaking, in that respect, of the character of a spark. In rare coal gas, the forms were better, but the light very poor and the colour grey.

1461. *Carbonic acid gas* produces a very poor brush at common pressures, as regards either size, light, or colour; and this is probably connected with the tendency which this gas has to discharge the electricity as a spark (1422.). In rarefied carbonic acid, the brush is better in form, but weak as to light, being of a dull greenish or purplish hue, varying with the pressure and other circumstances.

1462. *Muriatic acid gas.*—It is very difficult to obtain the brush in this gas at common pressures. On gradually increasing the distance of the rounded ends, the sparks suddenly ceased when the interval was about an inch, and the discharge, which was still through the gas in the globe, was silent and dark. Occasionally a very short brush could for a few moments be obtained, but it quickly disappeared again. Even when the intermitting spark current (1455.) from the machine was used, still I could only with difficulty obtain a brush, and that very short, though I used rods with rounded terminations (about 0·25 of an inch in diameter) which had before given them most freely in air and nitrogen. During the time of this difficulty with the muriatic gas, magnificent brushes were passing off from different parts of the machine into the surrounding air. On rarefying the gas, the formation of the brush was facilitated, but it was generally of a low squat form, very poor in light, and very similar on both the positive and negative surfaces. On rarefying the gas still more, a few large ramifications were obtained of a pale bluish colour, utterly unlike those in nitrogen.

1463. In all the gases, the different forms of disruptive discharge may be linked together and gradually traced from one extreme to the other, i. e. from the spark to the glow (1405.), or, it may be, to a still further condition to be called dark discharge; but it is, nevertheless, very surprising to see what a specific character each keeps whilst under the predominence of the general law. Thus, in muriatic acid, the brush is very difficult to obtain, and there comes in its place almost a dark discharge, partaking of the readiness of the spark action. Moreover, in muriatic acid, I have *never* observed the spark with any dark interval in it. In nitrogen, the spark readily changes its character into that of brush. In carbonic acid gas, there seems to be a facility to occasion spark discharge, whilst yet that gas is unlike nitrogen in the facility of the latter to form brushes, and unlike muriatic acid in its own facility to continue the spark. These differences add further force, first to the observations already made respecting the spark in various gases (1422. 1423.), and then, to the proofs deducible from it, of the relation of the electrical forces to the particles of matter.

1464. The peculiar characters of nitrogen in relation to the electric discharge (1422. 1458.) must, evidently, have an important influence over the form and even the occurrence of lightning. Being that gas which most readily produces coruscations, and, by them, extends discharge to a greater distance than any other gas tried, it is also that which constitutes four fifths of our atmosphere; and as, in atmospheric electrical phenomena, one, and sometimes both the inductive forces are resident on the particles of the air, which, though probably affected as to conducting power by the aqueous particles in it, cannot be considered as a good conductor so, the peculiar power possessed by nitrogen, to originate and effect discharge in the form of a brush or of ramifications, has, probably, an important relation to its electrical service in nature, as it most seriously affects the character and condition of the discharge when made. The whole subject of discharge from and through gases is a most important one to science, and, if only in reference to atmospheric electricity, deserves extensive and close experimental investigation.

Difference of discharge at the positive and negative conducting surfaces.

1465. I have avoided speaking of this well-known phenomenon more than was quite necessary, that I might bring together here what I have to say on the subject. When the brush discharge is observed in air at the positive and negative surfaces, there is a very striking difference, the true and full comprehension of which would, no doubt, be of the utmost importance to the physics of electricity; it would throw great light on our present subject, i. e. the molecular action of dielectrics under induction, and its consequences, and seems very open to, and accessible by, experimental inquiry.

1466. The difference in question used to be expressed in former times by saying, that a point charged positively gave brushes into the air, whilst the same point charged negatively gave a star. This is true only of bad conductors, or of metallic conductors charged intermittingly, or otherwise controlled by collateral induction. If metallic points project *freely* into the air, the positive and negative light upon them differ very little in appearance, and the difference can be observed only upon close examination.

1467. The effect varies exceedingly under different circumstances, but, as we must set out from some position, may perhaps be stated thus: if a metallic wire with a rounded termination in free air be used to produce the brushy discharge, then the brushes obtained when the wire is charged negatively are very poor and small, by comparison with those produced when the charge is positive. Or if a large metal ball connected with the electrical machine be charged *positively*, and a fine uninsulated point be gradually brought towards it, a star appears on the point when at a considerable distance, which though it becomes brighter, does not change its form of a star until it is close up to the ball: whereas, if the ball be charged negatively, the point at a considerable distance has a star on it as before; but when brought nearer, (in my case to the distance of $1\frac{1}{2}$ inch,) a brush formed on it, extending to the negative ball; and when still nearer, (at $\frac{1}{8}$ of an inch distance,) the brush ceased, and bright sparks passed. These variations, I believe, include the whole series of differences, and they seem to shew at once, that thenegative surface tends to retain its discharging character unchanged, whilst the positive surface, under similar circumstances, permits of great variation.

1468. There are several points in the character of the negative discharge to air which it is important to observe. A metal rod, 0·3 of an inch in diameter, with a rounded end projecting into the air, was charged negatively, and gave a short noisy brush (fig. 8). It was ascertained both by sight (1427. 1433.) and sound (1431.), that the successive discharges were very rapid in their recurrence, being seven or eight times more numerous in the same period, than those produced when the rod was charged positively to an equal degree. When the rod was positive, it was easy, by working the machine a little quicker, to replace the brush by a glow (1405. 1463.), but when it was negative no efforts could produce this change. Even by bringing the hand opposite the wire, the only effect was to increase the number of brush discharges in a given period, raising at the same time the sound to a higher pitch.

1469. A point opposite the negative brush exhibited a star, and as it was approximated caused the size and sound of the negative brush to diminish. and, at last, to cease, leaving the negative end silent and dark, yet effective as to discharge.

1470. When the round end of a smaller wire (fig. 9.) was advanced towards the negative brush, it (becoming positive by induction) exhibited the quiet glow at 8 inches distance, the negative brush continuing. When nearer, the pitch of the sound of the negative brush rose, indicating quicker intermittences (1431.); still nearer, the positive end threw off ramifications and distinct brushes; at the same time, the negative brush contracted in its lateral directions and collected together, giving a peculiar narrow longish brush, in shape like a hair pencil, the two brushes existing at once, but very different in their form and appearance, and especially in the more rapid recurrence of the negative discharges than of the positive. On using a smaller positive wire for the same experiment, the glow first appeared on it, and then the brush, the negative brush being affected at the same time; and the two at one distance became exceedingly alike in appearance, and the sounds, I thought, were in unison; at all events they were in harmony, so that the intermissions of discharge were either isochronous, or a simple ratio existed between the intervals. With a higher action of the machine, the wires being retained unaltered, the negative surface would become dark and silent, and a glow appear on the positive one. A still higher action changed the latter into a spark. Finer positive wires gave other variations of these effects, which I must not allow myself to go into here.

1471. A thinner rod was now connected with the negative conductor in place of the larger one (1468.), its termination being gradually diminished to a blunt point, as in fig. 10, ; and it was beautiful to observe that, notwithstanding the variation of the brush, the same general order of effects was produced. The end gave a small sonorous negative brush, which the approach of the hand or of a large conducting surface did not alter, until it was so near as to produce a spark. A fine point opposite to it was luminous at a distance ; being nearer it did not destroy the light and sound of the negative brush, but only tended to have a brush produced on itself, which, at a still nearer distance, passed into a spark joining the two surfaces.

1472. When the distinct negative and positive brushes are produced simultaneously in relation to each other in air, the former almost always has a contracted form, as in fig. 11., very much indeed resembling the figure which the positive brush itself has when influenced by the lateral vicinity of positive parts acting by induction. Thus a brush issuing from a point in the re-entering angle of a positive conductor has the same compressed form (fig. 12.).

1473. The character of the negative brush is not affected by the chemical nature of the substances of the conductors (1454.), but only by their possession of the conducting power in a greater or smaller degree.

1474. Rarefaction of common air about a negative ball or blunt point facilitated the development of the negative brush, the effect being, I think, greater than on a positive brush, though great on both. Extensive ramifications could be obtained from a ball or end electrified negatively to the plate of the air-pump on which the jar containing it stood.

1475. A very important variation of the relative forms and conditions of the positive and negative brush takes place on varying the dielectric in which they are produced. The difference is so very great that it points to a specific relation of this form of discharge to the particular gas in which it takes place, and opposes the idea that gases are but obstructions to the discharge, acting one like another and merely in proportion to their pressure (1377).

1476. In *air*, the superiority of the positive brush is well known (1467. 1472.). In *nitrogen*, it is as great or even greater than in air (1458.). In *hydrogen*, the positive brush loses a part of its superiority, not being so good as in nitrogen or air; whilst the negative brush does not seem injured (1459.). In *oxygen*, the positive brush is compressed and poor (1457.); whilst the negative did not sink in character: the two were so alike that the eye frequently could not tell the one from the other, and this similarity continued when the oxygen was gradually rarefied. In *coal gas*, the brushes are difficult of production as compared to nitrogen (1460.), and the positive not much superior to the negative in its character, either at common or low pressures. In *carbonic acid gas*, this approximation of character also occurred. In *muriatic acid gas* the positive brush was very little better than the negative, and both difficult to produce (1462.) as compared with the facility in nitrogen or air.

1477. These experiments were made with rods of brass about a quarter of an inch thick having rounded ends, the ends being opposed in a glass globe 7 inches in diameter, containing the gas to be experimented with. The electric machine was used to communicate directly, sometimes the positive, and sometimes the negative, state, to the rod in connexion with it.

1478. Thus we see that, notwithstanding there is a general difference in favor of the superiority of the positive brush over the negative, that difference is at its maximum in nitrogen and air; whilst in carbonic acid, muriatic acid, coal gas, and oxygen it diminishes, and at last becomes almost nothing. So that in this particular effect, as in all others yet examined, the evidence is in favor of that view which refers the results to a direct relation of the electric forces with the molecules of the matter concerned in the action (1421. 1423. 1463.). Even when special phenomena arise under the operation of the general law, the theory adopted seems fully competent to meet the case.

1479. Before I proceed further in tracing the probable cause of the difference between the positive and negative brush discharge, I wish to know the results of a few experiments which are in course of preparation: and thinking this Series of Researches long enough, I shall here close it with the expectation of being able in a few weeks to renew the inquiry, and entirely redeem my pledge (1306.).

Royal Institution, Dec. 23rd, 1837.

XXIV.—*On Electro-Magnetic Forces.* By J. P. Joule, Esq.

29. In resuming the relation of my researches, I shall dismiss for the present the investigation of electro-magnetic forces as applied to the movement of machines, and consider the laws which govern that peculiar condition which is assumed on the completion of the ferruginous circuit—the lifting or sustaining power of the electro-magnet.

30. Although this wonderful property is known to all, and a variety of forms has been given to the electro-magnet both as regards the bulk and shape of its iron, and the length and number of its magnetizing spirals, I am not aware that any general rules have been laid down for its manufacture, which is a circumstance the more to be regretted, as it has led some to imagine that the different capabilities of various arrangements, are the consequence of causes too many and too recondite to be unravelled. I shall attempt in this paper to throw some light upon this subject, and shall describe a construction attended by far greater results than have hitherto been produced.—It was my desire to make my experiments as exact as possible, and as I wish the relation of them to be clear and definite, I shall begin with some observations on the *measure* of current electricity indicated by the galvanometer, an instrument not only useful but absolutely essential in an inquiry of this nature.

31. The great difficulty, if not the absolute impossibility, of understanding experiments such as these and comparing them with one another, arises in general from incomplete descriptions of apparatus, and in particular from the arbitrary and vague numbers which are used in characterizing electric currents. Such a practice might be tolerated in the infancy of the science, but in its present state of advancement greater precision and propriety are imperatively demanded. I have therefore determined for my own part to abandon my old quantity numbers and to express my results on the basis of an *unit* which shall be at once scientific and convenient.

32. That proposed by Dr. Faraday is, I believe, the only standard of this kind that has been suggested. His discovery of the definite quantity of electricity associated with the atoms or chemical equivalents of bodies, has induced him to use the *voltameter* as a measurer, and to propose that the hundredth part of a cubic inch of the mixed gases should constitute the *degree.** There can be no doubt that this system offers superior advantages to the experimenter in some circumstances,

---

* Experimental Researches, Series vii. (736).

and when the above instrument is employed. However, as I am not aware that it has been used in the researches of any electrician, not excepting those of Faraday himself, I have not hesitated to advance what I think to be more appropriate as well as more generally advantageous. It is thus simply stated.

33. 1. *A degree of* static *electricity is that quantity which is just able to decompose nine grains of water.* 2. *A degree of* current *electricity is the same amount propagated during each hour of time;* and 3. Where both time and length of conductor are elements, as in electro-dynamics, *a degree of* electric force, *or of* electro-momentum, *is indicated by that same quantity (a degree of static electricity,) propagated through the space of one foot in one hour of time.* Whenever in future I speak of *degrees*, I shall intend those which I have just defined.

34. As 9 is the atomic weight of water it is obvious how greatly my *degree* will facilitate the calculation of electro-chemical decompositions. I may in this place adduce an illustration from electro-type engraving: here, if a galvanometer graduated according to my scale were included in the circuit, it would only be necessary to multiply the degrees (33,2) of its indication by 32, the equivalent of copper, and this again by the time in hours during which the work has been carried on, to obtain the weight of copper in grains which has been precipitated, and there would therefore be no occasion whatever to disturb the arrangement until calculation had shewn that the proper quantity of copper was cast. For instance, in an experiment of my own, I caused two electrodes of copper to terminate in a solution of the sulphate slightly acidulated by sulphuric acid. The negative electrode, upon which of course the copper was deposited, consisted of a disc an inch and a half in diameter; the positive, of a small coil of wire. A current of the mean quantity ·415 was then passed through the apparatus for one hour and a quarter; hence, according to the rule, $\cdot 415 \times 32 \times 1\cdot 25 = 16\cdot 6$ gr. the weight of copper which should *theoretically* be deposited. The *real* quantity, well washed and dried, was 15·6 gr. The deficiency of one grain was the evident consequence of the consumption of a part of the electricity in the decomposition of *water*, which was plainly indicated by a slight evolution of hydrogen at the negative pole.

35. The galvanometer of which I made use in the last series of experiments* (14,) was connected with an apparatus fur-

---

* "Annals," April, 1840. p. 476.

nished with very fine platinum wires. Voltaic currents of a large variety of intensities were then conducted through both instruments at once, and at the end of one, two, or three minutes the circuit was broken, and the hydrogen measured in a graduated glass tube. The mean of ten trials, none of which differed materially from the rest, added to half its bulk of oxygen, then corrected for temperature, barometrical pressure and force of vapour, and reduced to weight, gave ·76 gr. of water decomposed in one hour by electricity indicated by each unit of my former quantity numbers; hence 11·8 of these last is equal to one *degree* (33,2) of my present scale.—The dimensions of the single coil of the above galvanometer are 12 inches by 6, and the deviation of its needle for one *degree* (33,2) is 34° of the graduated card. From these data it is easy to calculate with considerable accuracy the value of the indications of any similar instrument, bearing in mind that the electro-dynamic force produced by a constant quantity of electricity is directly as the number of coils and inversely as their linear dimensions.

36. The quantities of electricity which were brought into play in the subsequent experiments, were frequently so great that the needle of my galvanometer (14) was brought to an almost rectangular position when subject to their influence. I have, therefore, devised a new measurer, which I flatter myself will prove of greater service in some cases than the arrangement proposed for the same purpose by Mr. Iremonger.* Fig. 1, plate 4, is the plan of my instrument: *c*, *c*, is a rod of copper bent and fastened firmly to a strong wooden frame: *m*, is a magnetized cylindrical bar of steel, one foot long, and half an inch in diameter, supported slightly above the centre of gravity, (like the ordinary balance beam,) by knife-edges resting on hard concave surfaces of steel; a scale *s*, is attached to the nearer end of the magnet, for the purpose of receiving the weights by which the intensity of electricity is measured. Lastly, *r*, *r*, is a rest, the under surface of which, the magnet just touches when at zero.

37. In using this apparatus, it is merely necessary to adjust the magnet to zero, either by means of screws, weights, or (perhaps the most convenient in practice) by the attraction or repulsion of a steel magnet kept for the purpose. Then, on making the necessary battery communications at *c*, *c*, the scale *s* will rise with a force estimated by the weight, in grains, tenths, &c., which is required to reduce it again to zero. In my instrument, I have found that one degree (33,2) is indicated by .69 gr.

---

* "Annals," vol. iii. pp. 413, 414.

38. The value of this new galvanometer, (the sensibility of which may be increased at pleasure by multiplying the number of coils,) besides its usefulness in measuring copious currents, consists chiefly in its perfect independence of the terrestrial, as well as any other ordinary magnetic influence. In every possible situation, provided that the intensity of the balance bar is constant, and that no interference is induced *after* the adjustment to zero, the transmitted current is exactly proportional to the weight lifted by the scale, and I should have as much confidence in working with it on an iron steam-boat as if every particle of iron were removed entirely away.

39. I proceed now to describe my electro-magnets, which I had occasion to construct of very different sizes in order to develope any curious circumstance which might present itself.—A piece of cylindrical wrought iron, eight inches long, had a hole one inch in diameter, bored the whole length of its axis; one side was then planed until the hole was exposed sufficiently to separate the "poles" $\frac{1}{3}$ of an inch. Another piece of iron, also eight inches long, was then planed, and being secured with its face in contact with the other planed surface, the whole was turned into a cylinder eight inches long, three inches and three quarters in exterior, and one inch in interior, diameter. The larger piece was then covered with calico and wound with four copper wires (covered with silk) each 23 feet long and 1-11th of an inch in diameter, a quantity which was just sufficient to hide the exterior surface and entirely to fill the inside hole. I shall perhaps be better understood on reference to plate 4, fig. 2, where *m*, is the "horse shoe" on which I have drawn some lines to illustrate the position of the conducting wire, *a*, is the armature and *s. s.* &c., are screws with eye holes for the purpose of suspension. This electro-magnet is designated No. 1, and the rest are numbered in the order of their description.

40. The iron which I used in the construction of a second was round, and 2·7 in. long, and half an inch in diameter. It was bent into an almost semicircular shape, and covered with 7 feet of well insulated copper wire 1-20th of an inch thick. The poles were half an inch asunder, and the wire completely filled the space between them.

41. A third electro-magnet was made of a piece of iron, .7 of an inch long, .37 in. broad, and .15 of an inch thick.—Its edges were reduced to such an extent that its transverse section was a perfect ellipsis. This also was bent into a semicircular shape; and was covered with 19 inches of silked copper wire, one-fortieth of an inch in diameter.

42. Anxious to procure a still larger variety, I made what might, from its extreme minuteness, be termed an *elementary electro-magnet.* It was the smallest I believe ever constructed; and consisted of a piece of good iron wire, one quarter of an inch long and 1-25th of an inch in diameter. It was bent into a semicircle, and was covered by three turns of *uninsulated* copper wire 1-40th of an inch in diameter.

43. The system of *levers* which was used in part of the subsequent experiments was found to be so convenient that I am induced to describe it in this place, although it may not involve any thing essentially new.—In fig. 2. *b*, *b*, *b*, *b*, are beams of ash, three inches square and ten feet long, strengthened by strong iron plating. These are fastened together in pairs, by boards nailed to their upper sides. *i. i*, are moveable iron bearings, and *f*, is the fulcrum, also moveable, and armed with iron; *w*, *w*, are strong pieces of wood which bear upon the levers and carry the hooks which are affixed to the electro-magnet No. 1, and its armature.—I subjoin some of the results obtained by this apparatus. The first column contains degrees of current electricity (33,2). The second gives the products of the numbers in the first column and the length in feet of wire wrapped round the magnet; it contains therefore, degrees of electric force (33,3). Lastly, the fourth expresses the weight carried in pounds avordupois.

TABLE I.

Electro-magnet, No. 1. (39)—Weight of iron, 15lbs; length of wire, 23 feet.

Electricity.	Elec. force.	do. (corrected.)	Lifting power.
0.8	18.4	6.5	2.75
1.8	41.4	14.4	10
2.6	59.8	21	23
3.8	87.4	31	45
8.1	186	65	238
10.9	250	88	540
4.3	99.3		670
5.7	132.5		890
8.6	198.7		1060
14.4	331		1400
21.6	497		1800
36	828		2030

44. On one occasion the power necessary to break contact was 2090 lbs. or nearly *nineteen hundred weight*, which is I believe a greater weight than any magnet has hitherto carried, and is certainly vastly superior to the performance of any of the same weight; and I can shew (45. 50.) that this power great as it is, is not *so much* as is due to its peculiar shape.

45. The latter part of the above table was obtained experimentally before the first part, and in the mean time the proper insulation of the coils from the iron was destroyed by an accident, and not having the opportunity of refitting the electro-magnet, I have been obliged to supply the corrections of electric-force seen in the third column and calculated on the basis of the power obtained when the insulation was good. I can place great confidence in these corrections, but must confess that I cannot give that which I suspect to be necessary in (44), I have therefore related that experiment without mentioning the electric force. As however this uncertainty will not materially affect the subsequent observations, and only induces the suspicion that the maximum power of this electro-magnet is not yet attained, I have thought it best to relate that experiment (44) in the absence of the more complete data which I hope to advance in my next communication.

TABLE II.

Electro-magnet, *No.* 2, (40.) *Weight* 1057 *gr.*; *length of wire* 7 *ft.*

Electricity.	Electric-force.	Weight carried.
0·51	3·57	20
1·53	10·7	38·5
6·1	42·7	49

TABLE III.

Electro-magnet, *No.* 3, (41.) *Weight* 65·3 *gr.*; *length of wire* 1·58 *ft.*

0·42	0·66	5·5
1·0	1·58	9
2·0	3·16	11

46. With great care this small electro-magnet supported in one instance twelve pounds, or 1286 times its own weight.

47. No. 4, (42.) the weight of which was only half a grain, carried in one instance 1417 grains, or 2834 *times its own weight.*

48. It required great patience to work with an arrangement so minute as this last, and it is on this account that the above weight is not nearly so great as it ought to have been, the relative power however which I obtained with it is far greater than any that I had hitherto seen, and is more than eleven times that of the celebrated steel magnet of Sir Isaac Newton.

49. It is well known that the steel magnet should necessarily have a much greater length than breadth or thickness, and that the contrary shape is attended by the confusion of the

poles and a general diminution of virtue, and Mr. Scoresby has found that if a large number of straight steel magnets are bundled together, the power of each when separated and examined is greatly deteriorated.* All this is easily understood, and finds its cause in the attempt of each part of the system to induce upon the other part a contrary magnetism to its own. Still there is no reason why the principle should be extended from the *common* to the *electro* magnet, especially as in the latter case a great and *commanding* inductive power is brought into play to *sustain* what the former has to support by its own unassisted retentive property. All the preceding experiments support this position and I shall give a table in proof of its obvious and necessary consequence,—that *the maximum power of the electro-magnet is directly proportional to its least transverse sectional area.* The first column contains the least sectional areas in square inches of the whole magnetic circuits. The maximum powers in pounds avoirdupois are recorded in the second; and these reduced to one square inch constitute the third, under the title of *specific powers.*

TABLE IV.

		Least Sec Area.	Max. Power.	Spec. Power.
My own Electro-magnets.	No. 1	10	2090	209
	No. 2	0.196	49	250
	No. 3	0.0436	12	275
	No. 4	0.0012	0.202	162
Electro-Magnet at the "Manchester Victoria Gallery," made by Mr. Nesbit: length around the curve about three feet, diameter of iron 2¾ inches; sectional area, 5.7 inches; do. of armature, 4.5; weight of iron, about 50lbs.		4.5	1428	317
Prof. Henry's†, of iron, 2-inch square; its sharp edges rounded; weight 21lbs.; length 20 inches round the curve.		3.94	750	190
Mr Sturgeon's (one of the first exhibited in this country); length about one foot; diameter half an inch.		.196	50	255

50. These results are, I think, sufficient to prove the rule, if we make an allowance for various sources of error. No. 1, is unfortunately made of a piece of unsound iron, and moreover is suspected not to have been saturated (45.), otherwise I have no doubt that its power per square inch would have approached 300, or, that the whole would have been 6 or 7 hundred weight greater. Again, the specific power of No. 4,

---

* Magnetical Investigations, pp. 37, 38.

† Silliman's Journal, vol. xix. p. 404.

is smaller than the mean simply because of the extreme difficulty of making a good experiment with it (48.). With regard to Mr. Nesbit's electro-magnet* the battery used was so powerful (19 of Daniell's two feet cells) and the quantity of conducting wire so very large (14 lengths of wire each 70 feet long and about 1-14th of an inch thick), that its magnetism must have been brought to the utmost possible pitch of intensity, which therefore excelled the mean. On the other hand Professor Henry's for the opposite reason exhibits a specific power much *below* the mean.†

51. The mean of the specific powers of No 2, No. 3, and of that at the "Royal Victoria Gallery" may I think be fairly taken for the expression of the maximum magnetic force of iron under ordinary circumstances, which is simply stated by the formula $x=280a$ where a is the least sectional area in square inches of the magnetic circuit (49.).

52. Since the element of *length* has no place in the above formula and has in fact only a secondary influence playing the part of an active *resistance* (55) which it requires a large additional force to overcome; it is obvious that in the direct ratio of its reduction, will the attractions relative to weight of iron increase. Hence the large power, in this respect, of my short electro-magnets. Hence, also, I have no doubt that a relative power of 10,000 might be attained, and by increasing the sectional area and at the same time diminishing the length, or, what is the same thing and indeed the only means of its performance, by increasing the length and diminishing the diameter of the cylinder (39) of No. 1, that a single pound weight of iron might be made to carry 2 or 3 tons.

53. All this corroborates what I have before stated‡ with regard to the proper construction of the electro magnet for lifting purposes, and it is well illustrated by fig. 4, of plate xi. in the "Annals" for last April: if, in that figure, the line between *b* and *c* (which has been omitted by the engraver) be drawn, it will be evident that in the case of saturation when the magnets A and B are brought into contact, the *oblique* forces will vanish, and the attraction will consequently exist in the simple ratio of the smallest number of magnetic particles opposed to each other.

---

* I have had the pleasure of seeing another electro-magnet of this gentleman's construction It is short and thick, and consequently adapted for lifting a large proportional weight.

† His coils consisted of nine lengths of copper bell wire, each 60 feet long. The battery consisted of a single pair, which was certainly not sufficiently intense to overcome the resistance of the wire, so as adequately to effect the saturation of the iron.

‡ "Annals" vol. iv. p. 60.

54. With respect to the magnetizing coils, I may observe that each particle of space through which a certain quantity of electricity is propagated appears to operate in moving the magnetism of the bar with a force proportionate to the inverse square of its distance from the surface of the iron, and that when the tension or specific magnetism is the same, the thickness of iron on which that particle of conducting space acts, has nothing (apart from resistance and other foreign circumstances) to do with the whole effect. Now it may be mathematically demonstrated that, such being the law; if each particle induce upon a *large surface*, the resulting magnetic force will not vary much with the distance, but be a very constant quantity for any distance which bears a small ratio to the dimensions of that surface. Hence it is that a coil *within* a hollow piece of iron has no power to magnetize it;* in that case its energy is directed in equal quantities towards opposite directions, the nearness of one surface exactly counterbalancing the size of its opposite. And hence also in the case of my large electro-magnet, where the surfaces are large, every particle of conducting wire would perform its full extent of duty even if it were not quite close to the iron.

55. When the interferences arising from tension are reduced to a minimum by completing the magnetic circuit and making use of a very small electric force, (33,3) the resistance from *length* becomes a very sensible quantity,† varying probably in the direct ratio of that element. Some idea of its character may be formed from the following table, where I have compared half the maximum powers of each electro-magnet with the electric forces (33,3) that produced them; and, by dividing the former by the latter, I have a third column which, under the title of *specific power*, contains the quantity of lifting power (of that degree of tension) due to an unit of electric force.

TABLE V.

	Elec. forces.	½ max. power.	Specific power.
No. 1.	200°	1060lbs.	5.3lbs.
No. 2.	4.5	25	5.5
No. 3.	.66	5.5	9.2

56. The electric force against No. 2 is rather larger than the truth, on account of the greater relative distance of its coils from the iron: if we make on this account a slight addition to its *specific power*, we shall find that the results are in character with the observations in 54, 55, and that the

---

* "Scientific Memoirs," Part V. p. 14.

† "Annals," vol. iv. p. 59.

specific powers are the same for each, after allowance has been made for the *resistance of length.*

57. It is well known that when the galvanic circuit is broken, the armature is retained in its place with very considerable force. I was anxious to try the capability of my cylinder, No. 1, in this respect, and have arranged my results in a table, the first column of which contains the *degrees* (33,3) which were cut off; the second, the lifting powers due to these quantities of electricity; and the third, the power left after the current was broken.

TABLE VI.

Elec. force.	lifting power.	retentive power.
88°	540	33
29	40	16
14.5	10	10

58. There is considerable difficulty in making a good experiment with so powerful an electro-magnet as No. 1, when very small forces are measured. Nevertheless it is certainly the case that the *retentive is very nearly equal to the lifting power* with small quantities of electricity. Another curious circumstance presents itself in the very inferior retentive power of my electro magnet compared with those of considerable length. It is the natural consequence of its peculiar shape (49).

59. When the whole current is not cut off, but merely reduced by the interposition of a bad conductor, a surprising quantity of magnetism may be *supported* by a very small electric force. I subjected No 1 to 90° (33,3) a quantity adequate to bring its power up to 560 lbs., and then reduced the electricity to different degrees of intensity. Here are the results. The first column contains the degrees of electric force (33,3) to which the superior current of 90° was reduced; the second expresses the weight which is simply due to those quantities; and the third gives the lifting power which the same quantities could *support.*

TABLE VII.

Elec. force.	Lifting power.	Supported power.
31°	45lbs.	294lbs
21	23	210
14.5	10	112
6.2	26	63
4.1	11	56

60. A battery of the size of a common thimble is quite sufficient to produce 31° of electric force, and consequently

to sustain a magnetic power of about 300 pounds, and it is easy to perceive that, by increasing the size of the electro-magnet and the quantity of its conducting wire, the same minute source could support a magnetic virtue of an indefinite amount.

61. I must now conclude my remarks for the present. I intend, however, shortly to construct an instrument of a still larger amount, both of absolute and relative power, than that described in 39; and I will only add that the form I have now given to the electro-magnet is the only one which will permit an unlimited increase of size without diminution of relative power.

Note on Voltaic Batteries.

62. Having had occasion about a year ago to construct a battery of great intensity, it became a great object with me to devise such an arrangement of the elements as should be both convenient in use, and when destroyed, easily refitted. After trying and rejecting two or three systems, I succeeded in producing one which answered my immediate purpose very well; but as I was aware that experience was the only strict test of its value, I have hitherto refrained from presenting it to public notice. Now, however, that I have worked with it during nine or ten months, and have found it to possess every quality that can be desired; I hope in describing it to give the same facilities to others which I possess myself.

63. I have represented a series of three elements in fig. 3. A, B, is the common divided Wollaston's trough with the front side removed in order to shew the inside. The black lines within the cells are rectangular pieces of strong sheet copper, bent on a gauge to the shape seen in the figure. Within these, *z*, *z*, *z*, represent plates of sheet zinc amalgamated in those parts which are in contact with the dilute sulphuric acid, with which I always charge my batteries, and fixed in their places by pieces of hard wood furnished with grooves and extending the whole breadth of the zinc. Lastly, *a*, *a*, *a*, *a*, *a*, are pieces of square wood with holes in their centres to admit the screw bolt *s*, *s*, which secures the whole.

64. When the battery is worn out, empty its trough and place it therein; then unscrew the bolt and remove it and the pieces of wood; change the old zinc plates for new ones, taking care in the mean time to see that those parts of the copper which touch the zinc are bright; then replace the pieces of wood *a*, *a*, &c. pass the bolt through their centres and screw the whole tightly together. In this way I can easily refit three batteries, each consisting of ten pairs, (including the

amalgamation of fresh zinc plates,) in three quarters of an hour.

65. Of course Mr. Smee's battery may be conveniently fitted up on my plan. I prefer however for ordinary use an electro-negative element of *sheet iron* before either copper or platinized silver. In using sheet iron it is well to *tin* that part which is to touch the zinc in order to keep its surface bright.

66. I have lately constructed a large battery on Mr. Sturgeon's plan, and from my experience with it I am convinced that it presents a very superior arrangement of voltaic elements. It consists of eleven cast iron cells each one foot square, and 1½ in. in interior diameter. With eight pairs, arranged in a series of four, I can raise to a full red heat 18 inches of copper wire one tenth of an inch thick.

Broom Hill, near Manchester, 21st August, 1810.

XXV.—Professor Van Kobell *on a new kind of Electrotype Engraving, by which, without the use of previously engraved plates, impressions in the manner of Indian-ink Drawings are produced.—From the Gelehrte Anzeigen der k. bayer. Akademie d. Wissensch. Nos.* 88 *and* 89—1840.

It was the great and, indeed, remarkable advantages that practical science has already reaped from Jacobi's application of the galvanic precipitation of copper that first induced me to make the following experiments, and which, to the best of my belief, are new.*

I allude to the precipitation of a plate of copper into the surface of a painting or drawing in the indian-ink manner—the plate thus formed being capable of having impressions thrown off from it in the usual way.

It was easy enough to see that if we could succeed in giving the surface of the colour a conducting power, there might, of course, be formed on it a coating of copper whose minutest details would correspond with our drawing. The kind of drawing in question however, that is to say, sketching on a polished surface makes it necessary to work up the colours employed with some oily or resinous substance, and this does away with its conducting power. Neither can we apply a coat of black lead or other similar conducting substance, inasmuch as the most delicate tints and shades of the drawing would suffer from the use of the brush.

---

* Most of the plates used by our English calico printers, &c. &c. are, I understand, now produced by galvanic precipitation.—*Translator.*

I, therefore, set about trying to throw down a coating of copper into a sketch made on a plate of silver without employing any such expedients, for as the copper precipitated by this process is thrown down in a crystalline form, and the aggregation of individual crystals in pure malleable metals readily assumes the forms of plates, (inasmuch as their tesseral forms, when in thin films, so unite together as to form such) it struck me that it would be a mere matter of time to cause depositions on non-conducting places *when interspersed and surrounded with good conductors.*

The experiment bore out my expectations; and drawings in wax, varnish, copying-ink, &c. &c. were covered with a deposit without any conducting power being imparted to them, and this not unfrequently in a very short time. I had frequently occasion to remark how little nodules of copper began forming at the centre of the non-conducting surface with which the lower plate was there entirely coated, these nodules gradually running into each other, and forming lines and threads by subsequent aggregation. As it always requires from four to five days to obtain a plate thick enough to print from, it is the less necessary to impart a conducting power to the colour for the most delicate shades, that is to say, the thinnest films of the paint are, for the most part, completely coated over by the second day, leaving but a few patches free, the closing up of which may be hastened by the application of a coat of good conducting black lead, laid on with a paint-brush, for the drawing in the state in which it then is, is not injured by so doing. Before having thus recourse to the brush, the plate is to be dried with bibilous paper.

With regard to the method of forming the picture we wish to copy, the first thing is, that it should be painted on a bright plate of silver or copper.* The painting is to be executed with a *single colour*, which should be laid on with the clammy oil used in painting on china, and which is the residuum obtained from the evaporation of oil of turpentine. By way of colour, we may use the red ochre of the porcelain painters. A solution of Demerara gum in oil of turpentine, duly thickened by an admixture of red ochre, mineral black, or some such substance, furnishes also a colour that is pleasant to work, and which dries rapidly. The colour is to be so applied that the polished surface of the metal, where left bare, gives the brightest lights, while those parts that are more or less coated furnish the shadows. It is right to observe, that it is noways necessary that the colour should be laid on in a thick coat, *on the contrary, the more delicate and the finer is the execution of the drawing,* the sharper is its re-production in the copper-plate, and the sooner is this completed.

---

* Copper may be grounded with a coat of whiting, end on this, without difficulty, we may draw with a fine pen, using a solution of sulphuret of potassium (with the maximum of sulphur) by way of ink. The black lines thereby obtained may be removed while yet moist by washing, the drawing being, nevertheless, visibly impressed on the copper, owing to a kind of corrosive action.

The colour, when dry, should adhere firmly to the plate, otherwise a thin film of copper, which nothing but nitric acid can remove, gets in beneath it. The surface of the colour is not, however, to be quite smooth; it must be fine-grained, otherwise the copper-plate precipitated into it will not take the printing ink.

In some of the experiments I mixed up formate of silver with the colour, and exposed the plate to a gentle heat. Conducting points of silver were thus generated on the surface, whereby the covering of the whole was hastened, but no such addition is, as before remarked, necessary.

With regard to the precipitation of the copper, we may, for that purpose, use Jacobi's apparatus, or we may employ a copper trough with a parchment frame, an arrangement which Professor Steinheil—following up Daniell's plan—has introduced, or recourse may be had to Spencer's arrangement.

The employment of Jacobi's arrangement has this disadvantage, namely, that when the action has been going on for some time, the edges of the plate become too thick, forming rough borders, especially towards the corners; besides, without frequently changing its position, the copper is not precipitated of equal thickness over the whole surface, and it requires a certain degree of practice to prevent the metal running out into lines and branches upon the plate. The use of the copper trough has, it is true, its advantages; by frequent use, however, it becomes so coated with copper that it is necessary to give it a new bottom on account of the wavy form the old one assumes, besides there is more copper precipitated by its use than the operatiou requires. The apparatus that I have employed, and which I find answers the purpose very well, is composed of a flat-bottomed vessel of china or glass, and whose sides are two or three inches in height. A plate of copper is laid on the bottom of this vessel, having a strip of the same metal an inch and a half in width rivetted to it at right angles by way of conductor. This metal band, with the exception of its upper end, is insulated throughout by a coat of wax.

The dimensions of this bottom plate must be such that it should project about half an inch all round the plate on which the drawing is made, the latter being placed thereon during the operation. At first I connected the conducting strip of metal directly with the painted plate, but the edges of the plate thus obtained were too ragged, and this is avoided by the above modification of the apparatus. Above these plates, and resting upon feet about a quarter of an inch high, there is fixed a frame with parchment stretched across it, or, in other words, a tambourine. In this there are laid a couple of small glass rods, and on them a plate of amalgamated zinc, the metal being thereby prevented from coming into contact with the diaphragm. To establish the connexion, I make use of a copper plate somewhat smaller than the zinc plate, and resting on it, and furnished with a ribbon of copper about an inch and a half wide. This strip of metal either dips down into a channel filled with quicksilver adapted to the strip in connexion with the lower plate, or the two bands are connected by a binding screw. The employment of mercury, in making the connexion, requires care, for if any of it gets thrown into the lower plate as it lies during the operation, a thing likely enough to occur in inserting or withdrawing these strips, there is formed an amalgam with the copper to the destruction of the plate. It does not answer equally well to employ a wire in lieu of the broad connecting strip of metal, for on doing so we find the precipitation considerably weakened. The glass vessel up to the spot to which the frame when inserted comes, is to be filled with a concentrated solution of sulphate of copper, and water moderately acidulated with sulphuric acid is to be poured on to the zinc plate to the depth of a few lines. To keep up the precipitating action of the fluid, crystals of sulphate of copper should be scattered round the copper plate. From time to time I renewed the upper fluid, and replaced the zinc plate when considerably eaten away. Inconsiderable deposits of copper on the parchment may be scratched off, but if they increase to any extent, a new membrane must be used. Instead of such a tambourine, it may be mentioned, we may employ a trough of half-burnt clay, porous enough to allow of the percolation of the fluids, but in this case the precipitation is by no means so rapid.

By following the plan I have recommended, I have, in from four to six days, obtained plates four inches square, and above a line in thickness, and tolerably even throughout. When the surface is waved and uneven I withdraw the plate, and having dried it with bibulous paper, I file it down till it is of uniform thickness. I then replace it and allow the operation to proceed as before. Occasionally, also, I have covered particular spots with wax to allow others that were lower to increase to the height of the former, and the plate has then been filed smooth. It is advisable to direct one's attention from time to time to the thickness of the metal so as to turn round the thinnest edges of the plate under the parts of the diaphragm where the action is the strongest. To endure a rapid and compact precipitation, it is above all things requisite that the solution of copper should be constantly maintained at the point of saturation. The bubbles of air that adhere to the plate on its first immersion may be removed with a camel-hair pencil. It is only at the commencement of the process, that is to say, till the picture is coated over, that the operation demands our attention.

When the plate has attained the desired thickness, the edge all the way round is to be filed away, upon which, generally speaking, the two plates separate without difficulty. To render the plate we thus obtain fit for furnishing impressions, all we have to do is to clean off with æther any particles of colour adhering to it.

The impressions have the appearance of Indian-ink drawings, and the tone of colouring is extremely delicate, a fact the painter ought not to overlook.

I think that the accompanying specimens will* bear me out in the idea that this modification of the electrotype process is the more deserving of the attention of artists as it enables them, and that without much previous knowledge on the subject, to throw off copper-plate impressions of any sketch or picture. It need scarcely be remarked, that the graver may be subsequently applied to a plate thus produced, supposing we wish to heighten the effect of any particular part of the engraving. From what has been said, it will be seen that the process is by no means an expensive one.

Translated by
W. G. LETTSOM, Esq.

---

* We have several beautiful specimens of printing from this style of electro-type, which were sent to us with this paper; also one electro-type plate, with the picture of a tree, from which we give a copy in the electro-type plate.—EDIT.

XXVI.—*On the Analysis of Limestones, especially the Magnesian kind, and a method of completely separating Lime from Magnesia, when both are present in large quantity.* By ROBERT E. ROGERS, M. D. and MARTIN H. BOYÉ.*

Carbonate of lime, associated with more or less carbonate of magnesia, forms the principal ingredient of limestones. In some varieties the latter substance appears only as a trace, while in others, it amounts to nearly 50 per cent. of the mass. When the proportion of the carbonate of magnesia is very considerable, the rock is termed magnesian limestone, or dolomite, the latter name being mostly applied to the crystalline varieties. Variable quantities of other substances, as silica, alumina, and the oxides of iron, and manganese, are generally associated to some extent with the above principal constituents.

The Silica is usually either in the free state, in the form of small transparent grains of quartzose sand, sometimes impalpably minute, or in chemical combination with the alumina and iron, (clays, &c.).

The extensive use made of limestones in the arts and agriculture, as mortars, cements, fluxes and manures, renders it a matter of great importance to procure a certain and expeditious process for their analysis, especially as there exists great diversity of opinion respecting the relative efficiency of the several constituents.

We proceed to describe a mode of analysing calcareous carbonates, which we have found in practice both certain and expeditious, and, therefore, preferable, we conceive, to the methods generally in use, which demand extreme care and considerable time to furnish accurate results. The method here proposed, we have adopted with success in an extensive series of analyses performed for the geological survey of the state.

The limestone is first finely powdered, when a given weight, about 30 grains, is digested in chlorohydric acid, in the ordinary way, evaporated to dryness, moistened with chlorohydric acid, and re-dissolved and filtered. The silica and a large part of the other adventitious substances are thus left upon the filter. They are then calcined and weighed, a correction being made for the weight of the ashes of the filter. These steps give the amount of the *insoluble matter.*

The filtered solution, containing besides the lime and magnesia, portions of alumine and oxides of iron and manganese,

---

* **Journal of the Franklin Institute.**

is neutralised with ammonia, avoiding an excess, and then precipitated with sulphhydrate of ammonium, a small quantity of which will usually suffice. When the precipitate has subsided, it is filtered, the funnel being covered with a glass plate, so as to exclude the atmosphere, and then washed with water containing a few drops of the sulphhydrate of ammonium. The filter, with its contents, is then removed, pressed between bibulous paper, dried and calcined. The alumina, and oxides of iron and manganese are thus obtained together. When their quantity is such as to require them to be separately estimated, it can be done in the ordinary way.

In determining the lime and magnesia, a fresh equal portion of the powdered mineral is employed, which is decomposed by a sufficient quantity of dilute sulphuric acid, with the aid of heat. Water is then added so as to fill the vessel up to a given mark, after which alcohol of known strength is introduced in such proportion as to make the whole solution contain 40 or 41 per cent., (estimated by volume) of alcohol. The alcoholic solution of this strength * precipitates entirely the *sulphate of lime* along with the insoluble matters. When the precipitate is settled, it is filtered under cover of a glass plate, and repeatedly washed with dilute alcohol of the same strength, as that previonsly employed, until a barytic solution indicates no trace of sulphuric acid. The whole is now calcined, and the weight of the insoluble matters as already ascertained, being deducted, we obtain the amount of *sulphate of lime*, from which we compute that of the *carbonate*.

The filtered solution now contains the sulphate of magnesia, and an inconsiderable portion of the sulphates of alumina, iron, and manganese, besides an excess of sulphuric acid.—It is to be evaporated until all the alcohol is dispelled, and then precipitated by pure carbonate of potassa, with the precautions usually prescribed. The magnesia, alumina, and oxides of iron and manganese, thus precipitated, are filtered, washed and calcined. Subtracting from the weight of the whole, that of the three latter previously ascertained, we find the amount of the magnesia, which is to be estimated as carbonate.

The separation of the lime in the form of sulphate from magnesia, by an alcoholic solution, is so complete as to make it unnecessary to estimate directly the magnesia, except when we desire to check one result by the other. The above process, it need hardly be said, will apply equally to the analysis of other substances than limestones, in which lime and magnesia abound, for we have only to precipitate these earths as carbonates, convert them into sulphates, and then treat them with dilute alcohol after the manner described.

---

* Alcohol of this strength has a *specific gravity* of 0.951 to 0.949 at 60° Far and marks between 17° and 18° Baume. Alcohol of the shops (alcohol rectificatus Lond. Phar.) marking 54½ Pennsylvania proof—has a specific gravity of 0.835. Five volumes of this, and 6 a 6½ volumes of water, will give a very suitable mixture for the above purpose of analysis.

We present the following analyses by way of illustration :—

1. A white crystalline dolomite, from the neighbourhood of Montville, New Jersey. *Specific gravity*—2.853

A portion, 1.469 grammes, was raised to a dull red heat, and the water, which was received in a tube containing chloride of calcium, was found to weigh .007 grm. or .48 per cent, This small amount of water is not expelled at the temperature of boiling water.

Two other portions of the powdered mineral, treated after the manner described, gave these results :—

	Per cent.
Insoluble matter	.04
Alumina, ox. iron, and ox. manganese	.15
Sulphate of lime, and insol. matter	76.09
Magnesia, alumina, and ox. of iron and manganese	20.70

By subtracting the insoluble matter .04 from the joint weight of the soluble matter and sulphate of lime 76.09, we get 76.05, and subtracting the alumina and oxides of iron and manganese from the joint weight of these and the magnesia, we have for the magnesia 20.55

A reference to the annexed table, the use of which will be explained, shews that 76.05 per cent of sulphate of lime is equivalent to 31.54 per cent. of lime, or to 56.11 per cent. of carbonate of lime.

It also appears that 20.55 per cent. of magnesia is equivalent to 42.54 of carbonate of magnesia. The result will, therefore, stand thus :—

Carbonate of lime	56.11
Carbonate of magnesia	42.54
Alumina and oxides of iron and manganese	0.15
Insoluble matter	0.04
Water	0.48
	99.32

Were we to estimate the magnesia in this case by the loss, it would be 43.22 carbonate of magn. equivalent to 20.88 magnesia, or 0.33 per cent. more than the amount found by direct estimation.

With a view further to shew that the *whole* of the lime is procured by the above method, and therefore, that we may safely estimate the magnesia by subtracting the carbonate of lime and other ingredients directly got from the weight of the mass, we subjoin the following example of a specimen found to contain no magnesia.

2. A white crystalline, imperfectly saccharoidal limestone, from near the mouth of Yellow Breeches Creek, Susquehana River, Pa.

From one portion of the powdered mineral, treated with chlorohydric acid, we obtained

Insoluble matter	2.3 per cent.
Alumina..	1.2 ,,
Ox. of iron and manganese..................	*none*

Another portion treated with sulphuric acid and diluted alcohol of the proper strength, gave

Insoluble matter and sulph. lime.	133.19

Table for calculating lime and carbonate of lime from the magnesia from magnesia or its sulphate[*]

		1	2
Sulphate of lime	Lime	0.41532	0.83064
Sulphate of lime	Carbonate of lime	0 73780	0.47561
Magnesia	Carbonate of magnesia	2.07002	4 14004
Sulphate of magnesia	Magnesia	0.34015	0.68030

The first vertical column of the table contains the names of the substances, from a known weight of which we wish to compute the weight of the substances embraced in the second column. The figures in the horizontal lines represent the quantities of the substances named in the second vertical column corresponding to those quantities of the substances in the first column, which are signified by the numbers at the head of each vertical division of the table. An example will render the mode of using the table sufficiently plain.

In the first analysis, the amount of sulphate of lime was 76.05. To ascertain from the table the quantity of carbonate of lime equivalent to this amount of sulphate, we find on the horizontal line appropriated to the carbonate, the quantity due to seven parts of the sulphate—namely, 5,16463, then the quantity due to six parts, namely, 4.42682, and then that equivalent to five parts or 3.68902. By arranging these in their proper decimal order, so as to impart to the several

---

[*] This table is taken partly from H. Rose's Analytical Chemistry, vol. ii., and partly calculated for the present purpose. The principle of this method of calculating analytical results was first set forth by Poggendorf, in his Annals, vol. xxi., and has since been extensively carried out by H. Rose in his work just mentioned.

[†] The Table in the middle is to be read through both pages.—Edit.

Subtracting the insoluble matter, 2.3, from the sulphate of lime and insoluble matter, we have sulphate of lime 130.89 per cent., which is equivalent, as the table will shew, to 96.3 per cent. of carbonate of lime.

The amount of water as derived from a third portion was 0.2 per cent. Our analysis therefore stands thus:—

Composition in 100 parts—

Carbonate of lime	96,3
Carbonate of magnesia	none
Alumina	1.2
Insoluble matter	2.3
Water	0.2
	100.0

---

sulphate of lime, and also for calculating the carbonate of

3	4	5	6	7	8	9
1.24596	1.66128	2.07660	2.49102	2 90721	3 32256	3.73788
2.21311	2.95121	3.68902	4.42682	5.16462	5.90242	6.61023
621006	8.28009	10.35011	12.42013	14.49015	16.56017	18.63019
1.02045	1.36060	1 70075	2.04009	2.38105	32.72120	3.06135

amounts taken from the table, the value they are intended to have as units, tenths, hundreths, &c., and then performing a simple addition, we get the amount of carbonate corresponding to the whole quantity of the sulphate.

The figures will stand thus:—

Sulphate of lime 76.05

51.6462
4.42682
.000000
368902

Carbonate of lime 56.1099102

In the same manner, 20.55 of magnesia will be found to be equivalent to 42.54 of carbonate of magnesia—thus:—

Magnesia 20.55

41.400
00.000
1.035
1.03

Carbonate of magnesia 42.538

As it may be sometimes convenient to evaporate the magnesian solution to dryness, ignite it, and from the sulphate of magnesia thus procured, compute the magnesia or its carbonate—we have introduced into the table a column to facilitate the calculations.

XXVII.—*Extract of a Letter from* W. Snow Harris, Esq. F.R.S., *to* Mr. W. Sturgeon.

The simplicity and convenience of my plan of fixed conductors having been in the year 1820 generally admitted, the Navy Board were led to institute some further inquiries into the general effects of lightning on ship-board, and I was called upon to shew that the connexion of my conductors *with the sea* through the metallic masses in the hull was in no way detrimental to their action, or liable to objection as involving any danger to the vessel—the electrical discharges might as safely become dispersed this way as by a lightning chain hung in the rigging, perhaps more so, considering that these conductors were massive and continuous, and linked with the various metallic masses in the hull and sea into one general whole.

I was further called upon to explain what had been the ordinary course of lightning on ship-board, and what would, in all probability be the effects of electrical discharges upon my conductors.

In order to meet the views of the officers of the Board, as made known to me at that time, I naturally enough resorted to such practical experiments and observations as were within my reach, and calculated to bear immediately upon the points in question: I cited numerous instances of ships struck by lightning, in which heavy discharges had been safely transmitted to the sea through the intervention of the keelson bolts and other metallic bodies passing through the hull, and which were shewn to have been of such frequent occurrence as to lead to a common observation among sailors, recorded in the Philosophical Transactions, that when the lightning had reached the well the danger was over. By way of shewing the operation of *my conductors through the hull*, I resorted to the experiment you first loosely notice—strong charges from twenty-five square feet of coated glass were passed over a vessel's masts, fitted with the conductors, so as to shew the perfect facility with which the charge pervaded the hull and the sea at the same time: the charge was adequate to the fusion of 15 feet of small iron wire—percussion powder was placed over the joints of the conductors on the mast, and the sliding masts were put in motion at the time of the passing of the charge, and placed in different positions at each repetition of the experiment. I believe, any one must perceive, that the experiment shewed—1st, The perfect operation of the conductor through the hull. 2nd, Its continuity. 3rd, Its complete operation, under every possible position of the mast, which was required to be done.

You, however, shut your eyes to these plain deductions, and tell your readers that the experiments prove nothing peculiar to my system of conductors, and serve only to shew that copper is a conductor of electricity, and that detonating powder can be ignited by an electric spark; and this is what you call giving "a fair and candid explanation of my experiments before the Navy Board at Plymouth." Now, I never asserted that any other conductor *would not* convey an electrical charge to the sea. My experiments were never *instituted* under such an impression: they shewed, however, the continuity of the copper plates along the masts in the way I had disposed them; for had detonating powder been placed in a similar way about the *conductors then in use*, it would have inflamed, if exposed to a similar charge. These experiments were subsequently carried out in the Thames, opposite Somerset House, and again at Plymouth, on a very extensive scale, and in various ships of the navy, and were admitted by all who witnessed them to have an important bearing on the question of marine lightning conductors.

You inquire, whether you have not pointed out other experiments with which *I ought* to have made the Navy Board acquainted? Do you, then, really imagine that the simple facts to which you allude, and which are known to every tyro in electricity, were not also well known to the officers of the Board?

Are you serious, when you say, I ought to have shewn the officers that a wire *heated* RED-HOT by electricity *would ignite gunpowder?* and that an interruption in the conductor, by a cut of a saw, would cause an electric spark in the opening? However ignorant you may suppose the officers of the Board to have been of this subject, they certainly understood the matter very much better than you *appear* to do; they did not require such horn-book information: they entered very fully into the merits of the question, and left no point unexplored. They required of me information respecting the relative conducting powers of different metals; their respective resistance to fusion by electricity; the ratio in which they became heated either by the same or different quantities of electricity; the quantity required to heat wires of different diameters to the same degree, &c. &c. Experiments for the perfect elucidation of which, in meeting their views, I was obliged to invent new electrical apparatus, and exhibit the results in a way not before done.

I come now to the experiment you have characterised as shewing nothing more than the effects of gunpowder "blowing asunder two pieces of wood." This experiment was made to meet the inquiries of the Board as to the effect of a conductor incorporated with the mast, in *confining the discharge to its surface*, and preventing it *from entering* the wood.

My first illustrations were confined to small models about four or six inches in length, which could be *splintered* by the force of a heavy battery, and *saved* from damage by the application of metallic leaf along the surface; but being desirous to exhibit the same result on a larger scale, and shew how completely the *surface* conductor directed the charge without entering the interior, I tried the experiment under new and very delicate circumstances. A model of a mast, about ten feet in length, was made in parts, and an interrupted line of metal passed *through* it, percussion powder being placed in the interruptions; a continuous conductor was attached to its *exterior*, and *both* connected at a common point of junction outside the model, so as to give the electrical shock the choice of passing in the direction of *either* or *both*. *In no case* did the heaviest explosion *enter* the mast whilst the *exterior* conductor remained. In fact, it was safe both from a direct, and what you call a "lateral discharge." This result being first exhibited, the *superficial conductor* was removed, and a similar charge passed in order to shew by the explosion of the powder within, that the reason of *its failing to explode* in the former case, *was owing to the presence of the exterior conductor*.

Now this result, bearing so directly on the application of conductors to the masts, and which every one will, I imagine, deem conclusive and fair, you *briefly* carricature as the "*blowing asunder two pieces of wood by gunpowder*," and accuse me of endeavouring to persuade the British Association that it is a "fair representation of the effects of lightning on a ship's mast;" *and this you call also an "explanation" of my experiments*. I can only account for such gross misrepresentation by supposing what, I believe, is after all, not far from the truth, that you are really *ignorant* of the subject on which you have attempted to write.

Thus in sec. 202 of your memoir, you say, "this kind of lateral discharge will always take place when the vicinal bodies are sufficiently capacious and near to the principal conductor which carries the primitive discharge, or *to any of its metallic appendages;*" and in sec. 198 and 199 you say, that with a small jar of a *quart* capacity only, "you can produce lateral discharges, half-inch long, and *at a distance of 50 feet* from the *direct discharge*. That a discharge from such a jar *would* IMITATE a flash of lightning striking a similar conductor on a mast." In my experiment, which you so much abuse, we have actually *all* the *conditions* you yourself point out as essential to the exhibition of your own results, supposing them to be according to the course of nature. How then does it happen that we can pass the heaviest "primitive discharges" along the exterior conductor *without in any way effecting the detonating powder within?* If what you say be true, the model should be blown asunder *in consequence of the discharge passing down the exterior conductor.* However much, therefore, you may choose to detract from this experiment, it is by your own admission *consistent* with the course of nature. What would you have more?

These experiments, to which you have alluded, form only a part of those originally employed—there were a great variety of others, such as the dispersions of strips of leaf gold in certain directions only, when placed in the same relative position as the conductor on the mast—the expansive effects of the charge on various bodies—fusion of wires, and such like. In short, the series was as complete as could be desired: the experiments were examined by a Committee of the Royal Society, by Sir Humphry Davy, Dr. Young, and Dr. Wollaston; the latter entered minutely into the matter, and in a letter to the Comptroller of the Navy, gave them his unqualified approbation. *I suppose Dr. Wollaston's judgment will be considered at least equal to your own.*

You will excuse my entering into *the detail* of my kite experiments. Having been for the last ten years an observer of atmospheric electricity, and having had an atmospheric conductor leading into my study, it would be remarkable, indeed, if I had allowed the common electrical kite to have escaped me.

Do you wish "to persuade yourself" into a belief that *your occasional amusement with this piece of philosophical apparatus* entitles you to become a philosophical dictator?

With whatever self-complacency you may regard your employment of the kite, the effects you mention are *very common-place,* and *are as distinct* from the effects of a concentrated flash of lightning striking a ship, in the way described by Lieutenant Sullivan, of the "Beagle," as it is possible to be.

You ask me to what kind of electrical action I attribute the bursting of the hoops in the "Rodney," &c. To no electrical action at all, *properly so called;* but, as Priestly has already shewn, to the effects of sudden expansion. He says, "the cause of this dispersion, &c. &c. of bodies in the *neighbourhood* of electrical explosions is not their being suddenly charged with electrical matter, but the air being displaced suddenly, gives a concussion to all bodies that may happen to be near." Did you never see such effects illustrated by artificial electricity? Why, even children who use an electrical machine as a toy, are acquainted with the propulsion of a small ball from an ivory mortar, by the expansive force in the surrounding air caused by a dense spark. Surely, the experiments detailed by Cuthbertson, to shew the bursting open of wood and other bodies, by the expansive effects of the electrical explosion, must be known to you.

One would almost imagine by your putting such a question, that you were really uninformed upon some of the commonest experiments in electricity.

Your question, whether I think it "more prudent to lead lightning into a ship or keep it out," is a plain piece of sophistry. If meant as an argument against my method of *equalizing* the electrical action upon the general mass of the hull and sea, is as deficient as any thing can be. It is, like many other similar efforts in your memoir, *a deceptive and sorry appeal to the fears and prejudices of the* IGNORANT, by imposing upon their credulity, and leading them to imagine that my conductors lead lightning in an *explosive* form into the ship, and deposit it there as so much cargo, than which nothing can be more fallacious: almost every one acquainted with electricity knows, that the great use of a lightning conductor is to equalize, in a rapid way, dense electrical discharges, and so rob them of their explosive power by taking down their tension.

So far, therefore, from my conductors leading lightning into the ship in the way *you would have it supposed,* they virtually come under the "prudent part" of your question, and keep off the explosion altogether, by depriving the charge of its mischievous tendency directly it strikes any where upon the conductors aloft. Now, it should never be forgotten as an important feature in this discussion, that whenever we set set up an artificial elevation on the earth's surface we do, in fact, *set up a conductor of electricity,* that is to say, a lightning conductor. The *masts,* themselves, therefore, *are already* lightning conductors, passing necessarily into the body of the vessel, and upon these discharges of lightning will fall, whether detached metallic bodies be present or not, or whether they be furnished with metallic conductors or not: this is proved by experience. The mast of a ship from its position alone, necessarily determines a discharge of lightning upon the hull. Now, by perfecting the conducting power of the masts, and connecting them with all the metallic masses in the hull and with the sea, we so complete the conducting power of the whole, that an instantaneous distribution takes place in all directions, directly the explosion strikes the mast head, and the electricity is changed immediately, from a dense form, into electricity of comparatively little tension.

Now with respect to the actual results of the trials of my conductors. Have not these conductors, been tried in 12 ships of the navy for as many years? have not these ships been in all parts of the world? have they not all been exposed more or less, to severe storms of lightning?

Do not some of the officers who commanded them, and others, experienced men, *insist* on the *fact* of their ships having been struck by lightning in the usual way *without damage?* Convinced of the protection my conductors afford, have not the captains of ships fitting for service, repeatedly applied to the Lords of the admiralty to be furnished with them? *Can you point out any instance*, in which *inconvenience or damage* has arisen in these ships during this lapse of time? The main argument of your queston, therefore, is really answered by the results of experience; if you have *not* any *good fact* to oppose to these, of what avail is any *theoretical* objection to the use of my conductors you may find it CONVENIENT to set up. It must be quite apparent, that my method of defending shipping from lightning is based on admitted principles in *science*, and is, consequently, as free from theoretical objection as any other method. A lightning rod as a defence from lightning, is, under any form, nothing but a means of rendering more efficient the conducting power of the general mass—so as to admit of such intense discharges being readily dispersed, which would otherwise by causing an *explosive expansive force, produce damage.*

According to the Experiments of the learned Mr. Cavendish, the chances of escape from Lightning is in *this way*, increased by at least four hundred million to one, even with a conductor of iron.

The letter in your Annals, sighed W. Pringle Green, is really not worth my notice. His ridiculous queries have been so often before the public so often answered, that I cannot really notice them again. I must decline all intercourse with him in the shape of correspondence, and for this plain reason —I cannot place the slightest confidence in any thing he advances. But, lest I should be thought harsh in making this assertion without apparent truth, I will give a few examples of his respect for accuracy, and I will leave it even to your "candour" to say how far I am right.

1st. In the Mechanic's Magazine, vol. VIII, page 286, and in other places, Lieutenant Green states that the conductor of St. Paul's Cathedral, the *largest* ever put up, was, by a moderate flash of lightning, heated *red hot*, and *therefore judiciously removed as* DANGEROUS and USELESS.

That the conductors of St. Paul's church have been removed is a most shameless assertion. Neither have we good evidence for supposing it to have been made *red hot*—this I have shewn in my papers in the Nautical Magazine—at all events, if it had been, it could not have been from a moderate stroke of lightning, as stated by Mr. Green.

2nd. In the Mechanics' Magazine, vol. viii. page 13, and in a variety of other places, such as the public newspapers—in a pamphlet by himself, Lieutenant Green states, H. M. Ship "Kent" and "Perseverance" were struck by lightning and damaged, although having *conductors at the time.* In the "Kent," he says, three men were killed and several wounded, and the masts much damaged. At this time, he says, "two conductors were up, and there were more than 20 sail of H.M. Ships in company, and *near the* Kent, *without* conductors, *none of which were injured.*" Not finding a word about the conductors in the ship's log, I remonstrated with Lieutenant Green on the subject, when he again repeated the assertion at page 287, where he says, that he named the captains of the ships, and that "more than one hundred officers and a thousand seamen witnessed the fact."

The subject having been investigated by the Admiralty Committee, it appeared by letters from Captain Godfrey of the navy, and Admiral Cardon, the former in the "Kent" at the time, the latter in the "Perseverance," that these statements made by Lieutenant Green *are utterly unfounded.* Captain Godfrey says, that the Kent usually had the conductors up; but having been damaged, they had been laid aside. Admiral Cardon, who was in the Perseverance, also affirms, that they *had not a conductor in the ship.* Lieutenant Green talks of "imposing gross deceptions on the naval service and the public." Pray, what does he call this?

3rd. In his letter in your last Number, page 230, he refers to the Naval Chronicle, vol i. page 201, as evidence to shew, that my plan has been copied from Mr. Marrot. Now in the first place, there is no mention made there of any such person; and secondly, the memoir to be found there by the celebrated

Frenchman Le Roy, does not contain one word about lightning conductors fixed in the masts.

4th. In the same letter, and the same page 330, Lieutenant Green says, "by my representation of these facts, the existing Board of Admiralty, countermanded the Navy Board's order, &c." In other places e. g. in his pamphlet above-mentioned, in the Mechanics' Magazine, 288, he takes credit to himself for having through his influence with the Board of Admiralty, caused my plan to be laid aside. In order to ascertain if such were really the case, I wrote lately an official letter to the Board, referring to Lieutenant Green's assertion in your work, and in other places. The following is the copy of the letter received in reply:—

Admiralty, 6th March, 1840.

SIR,—In reply to your letter of the 3rd instant, I am commanded by the Lords, Commissioners of the Admiralty, to acquaint you that it does not appear by the records of this office, that their Lordships were in any way induced to lay aside your lightning conductors, by any representations of Lieutenant Green, or any other persons, and that that officer is not authorized to make such statements.[*]

I am, sir, your humble servant,

W. SNOW HARRIS, ESQ. Signed, JOHN BARROW.

5th. Lieutenant Green states page 232 of your last number and elsewhere, that my conductors are led through the after magazine[†]—this he has always insisted in, and has given a drawing to that effect. Will any of the thousands who have been at sea in the ships fitted with my conductors say that this is true?

These are a few of the numerous deceptions which appear in Lieutenant Green's productions. I do not think it worth while to cite any more. A brief notice of his style of reasoning and I have done.

In the Mechanic's Magazine, vol. viii. page 14, in order to shew the danger or conductors, he states, that the setting up of certain pointed rods in Lausanne, in 1825, was the cause of a terrible storm which happened there in 1824—that is just one year *after* the storm happened. This logic is based upon extracts from newspapers, in which the dates are given, and by what he calls an explanation in page 285, he makes the storm happen three years *before* the rods were set up, which *he* says *was the cause of it.*

---

[*] See Mechanic's Magazine, vol. VIII, page 237.

[†] Whatever might be the cause for discontinuing Mr. Harris's conductors in several men of war which had been furnished with them, it is a fact that such was the case, as will appear by the appendix to this letter.—EDIT.

In a newspaper called the Nautical Register, in which he wrote against my conductors in 1822, amongst a most luxuriant variety of contradictory matter, he has this remarkable passage: "The following statement will bear me out in what I have advanced, that '*no man ever did, or will exist*, who can *invent anything to guard ships from the direful effects of lightning*." He goes on to say:—"In the year 1801 or 1802, H. M Ship Cleopatra was at anchor about 30 miles from Vera Cruz; early in the evening it commenced to rain, with thunder, &c. The *conductor* was ordered by the captain to be hoisted at the mizen mast head, and from the time of its being hoisted until the morning did *streams of electric fluid continue to run* down it into the sea." Well, was the ship injured? Not in the least. Still this is to prove no man *did*, or ever will *invent anything* to guard ships from the direful effects of lightning!!!

I am, Sir, your obedient servant,
WILLIAM SNOW HARRIS.

Plymouth, March 10, 1840.

APPENDIX.

Copy of a correspondence with Rear Admiral Warren, Admiral Superintendent of the Plymouth Dock Yard.

"Plymouth, 13th March, 1838.

"Sir,

"As several of her majesty's ships fitted with my new lightning conductors have been paid off at Plymouth, and their spars returned to the dock yard. I should be much obliged by your informing me whether the conductors still remain in them? Whether any having the conductors have been re-issued? Whether, in the case of their having been removed from any cause, they have been refitted in another ship, or have been duly set aside for that purpose? as also, whether any spars with the conductors in them are yet remaining in store.

"I am, Sir,
"Your very obedient servant,
W. Snow Harris."

"Plymouth Yard, 16th March, 1838.

"Sir,

"As requested by your letter of the 13th instant, I beg to enclose to you a report of the particulars therein required, respecting the spars in store, fitted with lightning conductors, on the plan suggested by you.

"I am, &c. &c.

(Signed by the Admiral Superintendent.)

Copy of the Report to the Master Shipwright.

"**Plymouth Dock Yard, 15th March, 1838.**

"**Sir,**

"**In reference to the questions contained in Mr. Harris's letter of the 13th instant, I beg to state:—**

"**1st. That the conductors in the spars of ships paid off at this port have been removed, with the exception of five top-gallant masts returned from the Forte, which are now in store, the conductors remaining in them.**

"**2nd. No spars have been re-issued with the conductors remaining fixed.**

"**3rd. The conductors which have been removed, from whatever cause, *have not been refitted to other ships, but returned into store in common with other old copper.*"**

"**Mr. Harris's fourth question appears to be answered in the first paragraph of this memento."**

"**(Signed) J. F. Hawkes.**
J. Shaw."

"**To the Master Shipwright."**

In consequence of this correspondence, I addressed the following letter to Sir J. Barrow, who had previously favored me with an interview on the subject.

"Plymouth, 10th March, 1838."

"My dear Sir,

"I wrote to admiral Warren soon after my return. You will soon see by the copy of the correspondence herewith transmitted, that the new lightning conductors have been, with a few trifling exceptions, all torn out of the masts and thrown by in a somewhat contemptuous way as old copper: thus, the plates which might have been very well replaced in other ships, have not even been taken care of. The correspondence with admiral Warren is very brief, and will not cost you five minutes attention."

"After the explanation you were so good as to favor me with respecting conductors, I cannot but believe you would wish to have me fairly dealt by in this matter; and I should hope that the Board would not, on a review of the facts, treat me ungenerously. Let us then see in *as few words as possible* how the matter stands in relation to the Admiralty, the country, and myself."

1st. It is an admitted fact, that ships may be *burned* and *destroyed* by *lightning*; the logs of the navy shew that this is by no means improbable, and that some missing ships may have perished from this cause. They exhibit a loss of life, of damage, and loss of services of ships at critical periods, not generally appreciated: well then, this subject has been deemed of sufficient consequence to engage the attention of scientific persons for more than half a century, and some steps have been taken to palliate the effects of lightning on ship board. The methods proposed have been *inadequate* in *some way* for the damage has continued up to the present time; notwithstanding that buildings on land have been protected from this source of danger."

"2nd. In the year 1820 I investigated *practically* this question, and shewed how the fixed continuous conductors of Franklin might be rendered available on ship board, and how by a perfect system of conduction throughout the hull, all the protection which could possibly be obtained from admitted scientific priciples, would be arrived at."

"3rd. My proposals were eventually carried into effect in eleven ships of the navy, and the results has been as perfect as could be hoped for. The written testimonies of officers in command of the ships, prove that they have been exposed to heavy thunder storms; that they have been actually struck by the electric fluid, without in any case receiving the slightest damage: thus, not only shewing that the conductors are *unobjectionable*, but actually useful."

"4th. The conductors not only stand upon this, but they are supported by the avowed opinions of some of the most talented men in science the country has to boast of: almost every naval officer, to whom the conductors are known is desirous to have them, and many have applied for that purpose; and this feeling prevails even with the sailors who were at sea in the ships fitted with them, as, for instance, in the Beagle and Dryad."

"4th. In the face of all this how does the matter actually stand at this present instant? why thus, the ships in which the conductors were fitted. have been nearly *all paid off*, the plates, *have been commonly torn out of the masts, and thrown by as old copper*, and no notice taken of it. A great national experiment has been *abandoned*, and the results lost to the country, without any assignable reason, without enquiry. An experiment of great consequence to our commercial and naval prosperity, and one which has occupied the attention of the scientific part of Europe for upwards of 70 years."

"Can the affair possibly rest here; I am sure this could not be the serious intention of the Board; nevertheless, such is the actual state of the question in relation to the Admiralty and the country."

"6th. In respect to myself, I must necessarily feel the circumstances above detailed to be very severe, and uncalled for by any thing on my part: it is always difficult to speak of one's self; there are however some cases in which we are called upon to do so; this appears to be one of them; and if it be done with becoming diffidence, I trust you will excuse it."

"It is well known, that so far as ability has enabled me, I have for many years cultivated with great zeal, experimental science; and have not spared *time*, *toil*, or *money*, in doing so, as I believe my papers in the 'Philosophical Transactions' fully shew; indeed, the Royal Society marked their sense of my contributions to the pages of the 'Transactions,' by awarding me their Copley medal in 1835. Many of my researches in electricity and magnetism have been of practical advantage to the navy; I may claim therefore, at the hands of the Board some little attention."

"Now, in perfecting the application of conductors in ships, I have incurred, not only a very serious responsibility, but a very heavy expense. You cannot but believe, that if any damage had happened, either to the ships fitted with the conductors, or even to the buildings at the Victualling office at Plymouth, (which I should remark, were protected from lightning under my direction,) I must have been the *person* held responsible with the public. Is it right that one who successfully labors to promote the national science, and whose services have been advantageously used for the general good of the navy, should be passed by with coldness and neglect? Here are these conductors, notwithstanding the many documents and facts conclusive of their value, thrown unceremoniously aside as old copper, and no notice taken of it: surely, without any claim I may have the consideration of the Board on the ground of general science, this it must be admitted has the appearance of dealing somewhat unjustly by me. I cannot but believe, that in stating thus freely all I have to say to you, I am appealing to one who has himself done much for the literary honour of our country, and who, anxious for the advancement of natural knowledge, must necessarily feel well-disposed to promote an enquiry into such a case. When we consider the resources of this powerful nation, and how much its interest is involved in its naval and commercial prosperity, it surely cannot be on account of a thousand or two pounds that an invention of practical advantage to the navy is laid aside."

"I trust you will be so good as to bring this matter under the consideration of the Board, and will do me the justice to believe, that I desire nothing which may not come fairly and openly before the country, without any kind of reservation whatever."

"I am dear Sir, &c. &c.

W. SNOW HARRIS.

Sir John Barrow made a courteous reply to this communication: the matter, however, eventually terminated in nothing more than the fitting of the Acteon, without my knowledge, in the way before explained, sec. 19, page 13.

Copy of correspondence with the Admiralty, on the subject of an extract from a report on the new conductors to the Admiralty, from the Officers of the Plymouth Dock Yard.

"Admiralty, 12th December, 1837.

"Sir,

"With reference to former correspondence upon the subject of your lightning conductors, I am commanded by my Lords Commissioners of the Admiralty to transmit to you the accompanying *extract* of a report from Plymouth Dock Yard, relative to the state of the masts of the Caledonia, in which the conductors were fitted.

"I am, &c. &c.,
"C. WOOD."

"W. Snow Harris, Esq."

Extract of a report from the Officers of Plymouth Dock Yard, dated 6th December, 1837.

"We beg to acquaint you that the conductors have been removed from all the spars returned from the Caledonia; that the main-top mast has been converted to a brig's main-maist; the fore and main-top gallant masts have been appropriated to jury gear; and that owing to the scores left in the spars by the removal of the conductors, it will be necessary to reduce them before they be re-issued."

"Plymouth, 16th December, 1837.

"Sir,

"I feel much indebted to the Lords Commissioners of the Admiralty for the extract of the report from the Plymouth Dock Yard, relative to the state of the masts of the Caledonia, fitted with my lightning conductors; and hope to be permitted to offer the following remarks on it, which their lordships will, I trust, take into their candid consideration.

"I find on inquiry, since I was honoured with their lordships' communication, that when the Caledonia was dismantled:—

"1st. That her three working top-masts, having been in the ship for *more* than seven years, were so rubbed in the caps and otherwise worn, that they were not considered fit for further service.

"2nd. That no kind of defect was discovered arising out of the application of lightning conductors; that so far as the conductors were concerned, the masts might *have been again used.*

"We learn, therefore, from these facts, that the conductors remained perfect in the masts up to the time of the masts being *considered* no longer serviceable, and that since the plates of copper were still good, they might, consequently, be re-applied to other masts of the same dimensions; without any new expense except in labour.

"3rd. That the three spare top-masts, at sea in the ship for more than seven years, were returned into the store as serviceable top-masts, and might, if they had been permitted to remain in the same state in which they were returned, have been re-issued, either to the Caledonia or to another ship of her class, without the necessity of any alteration whatever. That for some reason not explained, the plates were taken out of the masts, and, of course, as a *necessary consequence*, the shallow groove in which they were inserted left exposed.

"As these spars were never intended to be used without the conductors, any reduction contingent upon their removal was *a matter of choice*, such removal being *quite uncalled for*.

"I would still, however, respectfully submit to their lordships, that even although the plates should be removed, a reduction of the spar is not absolutely necessary; for if an oak batten was inserted in the grove, in place of the copper, and the whole planed off fair with the round of the mast, I am prepared to shew that the spar would be as serviceable as at first.

"Admitting, however, that the spar must be reduced, it is still not necessary to do more than pair off the small projection of the groove, (which is, after all, very little more than a quarter of an inch in depth,) the diminution of strength by this is really inconsiderable, and the mast might still be re-issued. The Spartiate's jib-boom, for example, was re-issued in this way, and, I believe, answered well.

"4th. That in the conversion of the top-mast to a brig's main-mast, the requisite reduction carried all round the spar *was not so great on account of the groove as was found necessary to bring the spar down to the required size.*

"Should it ever be requisite to convert a top-mast once fitted with my conductors to any other purpose, the necessary reduction is always much more than is contingent upon the groove for the lightning conductors.

"Their lordships will, I am sure, allow, that if after more than seven years, the wood was, on the removal of the copper plates, found so perfect as to admit of the mast being converted into so important a spar as a brig's main-mast, we have not much to complain of on account of the application of the conductor.

"I would in conclusion respectfully call their lordship's attention to the fact, that out of eleven ships fitted with the new conductors, few, I believe, now remain in commission, except the Beagle.* That although on being dismantled, their spars, with the conductors in them, remained perfect, and so far as the conductors were concerned, fit to be re-issued, yet, in several instances which have come to my knowledge, the conductors have been taken out of the masts, and the masts used for various purposes. I have no doubt the mast makers can explain why they have been led to do this in many cases, but why they have done so in others does not immediately appear; as no complaint has ever been made of the conductors so far as the masts were concerned, and that without any additional expense to the country the serviceable masts might still have been applied in the same way, and many ships been furnished with this protection from lightning."

"It is well known to their lordships that the Beagle was full *five* years on service, and that yet she has gone to sea with the same spars and conductors in them, on an equally long voyage, with the exception of new top-gallant masts."

"I cannot but respectfully bespeak their lordships' attention to these facts."

"And remain, Sir. &c. &c.,
WM. SNOW HARRIS."

"To Charles Wood, Esq., M.P., &c., &c."

XXVIII.—Mr. W. Sturgeon's *Letter to* W. Snow Harris, Esq. F. R. S.

Sir,—I hope you will acknowledge that I have given publicity to every part of your letter, that can possibly be useful either to yourself, or to the cause of your marine lightning conductors; to have published the other part of your letter, could have answered no laudable purpose whatever. I can have but very little to say in reply, as the opinions which I have already entertained, and which are already before the public, are not in the least affected by any facts which your letter contains. Your answers to the queries in my letter are partly satisfactory, and partly otherwise. Your explanation of the expansive effects of lightning are perfectly satisfactory, because you necessarily admit that lightning was the *primitive* cause, which admits of the correctness of all I have said respecting a *lateral discharge* of the *first kind.*[*]

---

* That is to say, in which the conductors still remain perfect.

Your explanation of the reasons which led you to proceed in the manner you did with the experiments at Plymouth, before the Navy Board, appear to me anything but satisfactory. That your conductor on board the Louisa cutter, was perfect enough to carry those electric charges which you transmitted through it, there can be no doubt whatever; but, that it shewed any peculiar advantage of action over other conductors I must still deny; although you say that, "*I shut my eyes to these plain deductions, and tell my readers that the experiments prove nothing peculiar to your system of conductors, &c.*" Now his sentence of yours, obviously implies a claim of some peculiar advantage of your system of conductors being demonstrated by those very experiments, and a censure upon me for not telling my readers that such was the case. Had I said anything otherwise than that which I did say respecting the character of the Plymouth experiments, I should have told my readers an untruth; and I think that not only *my* readers, but *your* readers also, will see pretty clearly, that I was perfectly correct, in stating that "those experiments are no more illustative of the efficacy of Mr. Harris's system than of any other ever yet offered to public notice," when I point out to them your own words on this matter, which are the following. "Now I never asserted that any other conductor *would not* convey an electrical charge to the sea. My experiments were never *instituted* under such an impression."[*] I am sure that both of our readers will be much pleased to find that you have so ably set this matter at rest.

With respect to the "horn book"[†] work which you speak of, I have no means of knowing anything further than that which the character of your experiments indicates, which, to an electrician, would not appear very conclusive. And the reason you have given for employing gunpowder to show the effects of lightning on a ship's mast, are quite unsatisfactory.[‡] Had you continued your illustrations on the small model, which for the first time, you now speak of, they would have been perfectly satisfactory. It is an old experiment and quite conclusive; but I must certainly still indulge in the opinion that your *gunpowder* experiment was not only quite out of place, but tended to give a false idea of the nature of the action by which masses of wood are cleft by flashes of lightning.

---

[*] Fourth Memoir, paragraph 139, page 174, vol. iv. of these Annals.

[†] Fourth Memoir, page 166, vol. iv. of these Annals.

[‡] Mr. Harris's letter, page 209. of this Number.

I am of opinion, also, that you are led into error even "under the new and very delicate circumstances" by which you "tried the experiment."* For if the detonating powder *did not* fire in the *interrupted* part of the circuit, by *your* experiment, that can be no very decisive reason why it should be so extremely obstinate in other hands. The "horn book" informs me that each branch of the conductor will carry a portion of the charge if sufficiently powerful, and the interruption in one branch of the conductor be only small.

Your view of my "lateral dischage" *at a distance* of 50 *feet* from the *direct discharge*, seems to have led you into some considerable error concerning *lateral* discharges in the body of a ship from direct discharges through my system of conductors in the rigging. I think that I have stated pretty clearly that this 50 feet, was 50 feet of metallic wire, (see page 175, vol. iv. of these Annals); and I never yet understood that there was a direct metallic communication between the outside of a ship and her powder magazine!!! or that the one was very near to the other: and I think you will admit that the distance of your conductors from the magazine is very trifling indeed, when compared with the distance between the latter and the outside of the vessel.

Moreover, the distribution of my conductors in the rigging is such that every flash of lightning which struck them, would be equally distributed amongst them before it arrived at the body of the ship: so that a small fractional part only, would be carried by any one of the lower branches: and as each branch conductor would carry an equal share, the forces on the two sides of the ship would be so completely balanced as to neutralize each others action on bodies placed directly between them, not only as regards *lateral discharges*, but also as regards the magnetic action of heavy flashes of lightning: for although an electric discharge traversing a single conductor, will magnetize a ferruginous body, a needle, for instance, placed within the sphere of its action, yet no discharge of electricity which passed equally on *both sides* of the needle would magnetize it: because one part of the electro-magnetic action would counteract the other part of it, and they would mutually neutralize each others effects.

In regard to "Dr. Wollaston's judgement" on matters of philosophy, I shall always have a great veneration, and whatever degree of approbation he may have happened to confer upon your conductors, I should have found very little difficulty in making that philospher sensible of the dangerous effects of their electro-magnetic powers when traversed by heavy flashes of lightning. This is such a simple and common "horn-book" affair, yet such an important consideration in the disposition of marine lightning conductors, that its omission in the report of the committee is an *event* in British science, which leaves you and the *scientific councillors,* in no very enviable position. And although it may not have occurred to you before, that the situation of your conductors would give them a most dangerous influence on the chronometors and compasses of the ship, yet now that I have clearly pointed out the fact, it behoves you, at this critical period, to make known to the Admiralty that such is the case, in order that some means may be adopted to prevent those serious consequences which your system of conductors can hardly fail to produce.

---

* Mr. Harris's letter, page 210 of this Number.

I do not find that any other part of your letter requires my notice, and as I have met every other effort which you have made in favour of your conductors, in my former letters, without experiencing the slightest reason for altering my first statements, made in my fourth memoir, I necessarily conclude the discussion, under the same impressions as those with which I began. The errors into which you have accasionally fallen in those papers which you have published since the appearance of my fourth memoir, have certainly tended to rectify my former views of your mode of philosophical reasoning, which, I believe, is the only remuneration I need expect; unless, indeed, my exposure of the dangerous tendency of your lightning conductors may induce those in authority to pause, and re-investigate the whole subject, before any decisive steps may be taken for fitting out the british fleet with any lightning conductors whatever. And as I have some reason for supposing that such will be the case, I am still in hopes of experiencing the great satisfaction of having been instrumental in averting those personal and national calamities, which, in every probability would occur from the effects of lightning, were our fleet to be furnished with conductors such as you have proposed, And should I even be disappointed in that particular, it will always be a gratifying reflection that I have pointed out the means whereby those dangers might be averted, at an expense of little more than the first cost of the material; and without detaining any ship in harbour, or causing any other obstruction in the performance of any part of her duty, whatever may be the nature of her service, and on whatever station she may happen to be placed.

I have the honor to be,
Sir,
Your obedient Servant,
W. S. Harris, Esq. W. STURGEON.

XXIX.—*On the cause of the change in colour which takes place in certain substances under the influence of Heat.*—By C. S. SCHOENBEIN.

It has not fallen within the power of man, up to the present time, to determine the relation which exists between the chemical nature of a body and its colour; it is probable that the determination of this difficulty presents one of the most difficult problems that philosophers and chemists will have to resolve. We know not why copper is red, gold, yellow; the cyanite of iron blue, and, in particular, we are complely uncertain whether the cause of the colour of a substance ought to be sought for in the nature of its mobecules, or in the particular mode of their aggregation. But whatever be the obscurity which reigns in this point of view, and however great our ignorance on the true cause of the colourization of bodies, we know, notwithstanding, that the fact which determines the chemical nature of any substance is that which decides, before any thing, its relations with the light; and, in fact, there are a hundred cases in which we may conclude with certainty, that a chemical modification has taken place in a body after a modification of colour has been observed. But it is not only these luminous phenomena, with relation to colour, which are frequently modified by the effect of the chemical changes of the substance; those of this species of phenomena which may be referred to refraction, to reflection, to inflection, and to polarization, are under the same influence: and, in short, we may safely affirm, that in order to arrive at the establishment of the identity or the chemical difference of substances there exists no re-agent more sensitive than light.

Up to the present time, with our chemical means, we have only been able to determine amongst bodies those differences the most gross, and easily to be perceived; and, without doubt, also, for this same reason, we have admitted as identical with each other a great number of substances, which, upon examination, by the aid of re-agents the most delicate, will eventually demonstrate to us that there is no identity between those bodies. It is then very desirable that opticians should come in to the aid of their chemical brethren, and by furnishing them with such instruments as are necessary, enable them to determine, in a manner at once easy and certain, the slightest qualifying modification which takes place in any which may be subjected to their inquiry. When once the research into the chemical nature of substances, by optical means, shall come into general use, I am persuaded that our knowledge in this respect will be rapidly extended, and we shall acquire more correct ideas than those we possess at present on the intimate nature of substances. The researches of Newton on the power of bodies to refract light, and the researches, still more recent, of the celebrated Biot, have already placed in a striking point of view the great importance it may be to the chemist to have a knowledge of the optical character of bodies.

I have no other end in view, in publishing this work, than that of calling the attention of philosophers and chemists on the importance of the momentary and sudden changes of colour which divers substances undergo under the influence of heat.

In comparing the chemical nature of substances which present this phenomenon in these indicated circumstances, it ought to excite our surprise, above all, that it is only observable in compound bodies. Sulphur, phosphorous, and perhaps, also, selenium, which are considered as simple substances, form exceptions; but the property which these bodies possess of taking various colours in different circumstances, ought, perhaps, to be sufficient for us to presume that they are compound, more especially if we consider that sulphur, and in all likelihood also, the two other substances are.

The number of compound substances which are possessed of the properties of which we are now speaking is very considerable, and it would be too tedious to name them. Amongst those which are solid, I content myself with naming the red oxide of mercury, which, upon being heated, takes a brownish black colour. The yellow basique nitrate of mercury, which in the same circumstances assumes a red colour; the red iodure of mercury, which becomes yellow at an elevated temperature; the citron yellow coloured chromate of potash, which takes an orange colour on being affected by strong heat. The liquids, with some exception, change colour in general when they are heated. A solution of muriate of cobalt, for example, which when cold is of a yellow brown colour, becomes blue on being heated; an acid solution of nitrate of iron, which at the ordinary temperature is completely colourless, becomes a reddish yellow when heated. Nitrous acid, (colourless at 20°) becomes yellow, and even a red brown on being exposed to heat: the colourless combinations of this acid with nitric acid, sulphuric acid, phosphoric acid, &c., becomes equally yellow in the same circumstances. Among the compound gases I know of none of which the colour is sensibly changed by the effect of heat except it is the nitrous acid gas, whose colour is, as is well known, of a deeper hue at a high temperature. But it is very likely that a more profound examination will shew that other aëriform bodies also change colour with their temperature.

The question in point now is to resolve, to what cause the phenomena in question may be attributed; whether it is to mechanical circumstances, or to chemical changes. Up to the present time we have always sought to explain it, by vaguely admitting that heat produces a certain modification in the arrangement of the intimate molecules of a body from whence proceeds a change of colour. This hypothesis may possibly be exact, generally speaking, but it is so vague and indeterminate that it leaves, in a complete uncertainty the question of ascertaining whether heat only changes, the relative position of the compound molecules, or whether the simple heterogeneous atoms combine among themselves under the influence of an elevated temperature, in other affinities than when the body is not heated. Some recent researches of Mitscherlich, Rose, and other chemists, have demonstrated that certain salts undergo an essential modification, one might almost say chemical, though this was not a decomposition in the ordinary sense of the word. Thus the arroganite, heated to a full red heat, is transformed into calcareous spath; the red pyrimidal iodure of mercury into the prismatic yellow iodure of mercury, without the observer being able to perceive any modification in the composition of these two bodies. Other examples of the same nature might easily be cited.

An important circumstance, to which I shall endeavour at the present time to draw your attention, is this, that the red iodure of mercury, on being transformed into yellow iodure by the action of heat, persists some time still, it is true, in its new estate after being cooled, but does not fail, nevertheless, to take its primitive state without the sensible intervention of any exterior action, though mechanical causes, such as a sudden shock, singularly hasten its return to the normal state. The arroganite once transformed into calcareous spath, undergoes no further change. There is not the least doubt that in the case whence arises the question, that heat only produces in the chemical nature of bodies that modification which chemists call *isomeric*. They form new substances, which are distinguished in particular from those from which they proceed by a peculiar form, by their specific gravity, by their hardness, and by their action on light, and, in all likelihood, by other physical properties.

What is it now which takes place in those substances which thus change colour with the temperature? Does this change indicate different chemical combinations among the constituent elements, and ought it to be regarded as a proof that the mass resting identical, the same element can form a series of isomeric combinations, of which each in particular is produced by a determined temperature? The change which takes place in the red iodure of mercury appears to me to be of particular importance in the answer to these questions; on one hand, because the phenomena which this body presents approach in appearance those of bodies which the act of cooling causes to return to their former state, (the elements of this iodure not persisting, in fact, in their new combination); on the other hand, because the body approaches also to carbonate of chalk, the iodure not taking immediately its primitive state when the cause which modifies it has ceased to act. Under the relation of the variability of its molecular composition, this iodure places itself then between the calcareous carbonate and the combination in which the change of temperature, and the modification in the chemical constitution, only remains as much in one as the other.

Let us now seek a solution of the proposed question, at first in that which concerns the oxide of mercury, which presents a change of colour so remarkable. The mode of combination of oxygen with mercury, under the relation of intimacy, ought not to be the same at elevated degrees of temperature as at inferior degrees: it is already acknowledged, that at a certain temperature these two substances separate one from the other, and it will be easily admitted, that as the oxygen holds much more feebly to the mercury, that the oxide is most heated. Now, a difference in the intimacy with which the same elements are combined constitutes already, according to my ideas, a difference either qualifying or chemical. After that, the oxide of mercury in a heated state, is chemically different from the cold oxide, and there exists between the two an isomeric relation. To speak more properly, all the chemical combinations which are of different temperatures, are, it is true, in the same case, but particularly those which heat alone decomposes. It seems possible to me, however, that many compound bodies undergo, in their intimate nature, by the effect of heat, modifications which may have, it is true, a distant reason in a change of relations of affinity, but which are owing, above all, to a momentary derangement of the constituent elements, out of their normal position taken at the ordinary temperature. It is, in effect, a singular fact, that many compound bodies take, when they are heated, a colour which characterises another degree of combination of the same elements. The following examples will serve to make the case in question better understood.

At an elevated temperature, the oxide of mercury takes the colour of the *oxidule* of the same metal; the deuto-sulphuret of mercury, the colour of proto-sulphuret; the proto-chromate of potash that of double salt; the colourless solution of nitrate acid of iron that of the solution of a nitrate basique of that metal; the solution yellow and neuter of muriate of cobalt, that of the acid solution of the same metal, &c. Now though this shadowing change may not be observed in each of the substances which change colour with the temperature, the cases where it presents itself are, however so very numerous, that we are not at liberty to consider this phenomenon as simply the effect of chance, nor to suppose that the changes in colour of these substances are owing to the formation of a new combination: as for example when the deutoxide of mercury is heated it is transformed into protoxide, the neutral salt of chrome into acid salt, the neuter solution of muriate of cobalt into an acid solution, &c. But as, in the examples here cited, the heat does not separate oxygen, nor potash, &c. it is necessary to admit that these substances are found in an intimately mixed state in the bodies which we have submitted to the action of heat, or else, that the new combination possesses still so great an adhesive force for the substance which has been insulated, that it cannot be separated from it. It is possible, also, for example, that a molecule of mercury may be found, during the process of heating, nearer to one of the particles of oxygen enclosed within an atom of oxide than another, and that this second particle remains attached to the oxidule by the effect of a species of affinity, and is thus prevented from disengaging itself under the form of gas. We may suppose again, that there is established, sometimes between the two elements of a combination exposed to the action of the heat such a relation that they are, it is true, completely separated one from the other under the chemical relation, but that they are still retained together by the effect of an attraction, similar to that which Faraday believes to be exercised by platinum on oxygen.

It is the skilful Kielmeyer, if I mistake not, who has advanced, now a long time since, the idea that each particular temperature has its own chemical power. Without wishing to take this assertion for granted, I believe, however, that it is true in general, and that a proof is furnished of its correctness in the change of colour produced by heat in compound bodies. As I have already made the remark, that chemists have not, during a long time, taken account only of the most glaring differences of bodies, and have regarded as identical those substances which have given as the result of analysis the same elements in the same proportions. The discovery of isomery, and of *dimorphie*, with which it is intimately connected, has elicited the fact, that the equality of elements, and the proportions in which they are combined, is not a certain criterion of the identity of chemical substances, and that, in this case, it is quite possible that there may exist great differences in the physical and chemical properties of bodies. However, let this point be once established, and that in particular it has been demonstrated, that by the aid of heat we can accomplish not only the decomposition of substances, but, likewise, isomeric transformations, we may hope that chemists will direct their attention to the qualifying modifications less apparent, and particularly on the transient modifications which these bodies undergo under the influence of heat. Researches of this description would not fail to extend the actual limits of chemistry, and give us more correct ideas on the different modes of combination of elementary substances, as also, to spread the light of day on the relation which exists between the molecular constitution of a body and its physical and chemical properties.

In order to find some experimental proofs in favour of the opinion which I have advanced in relation to the cause of the change of colour on many substances, I have had recourse to the galvanometer. It is a fact, recognised by most philosophers, that every chemical modification, formation, or decomposition, of a compound body, has the effect of destroying the electrical equilibrium of substances which act upon each other. Upon this principle, if the change of colour, now in question, is to be attributed to any chemical modification whatever in those substances in which it is observed, we ought also to see established a voltaic current, and to be able to demonstrate the existence thereof, in favourable circumstances, by means of the multiplicatier. Now, to speak of solid substances whose colour changes with the temperature, they are, unfortunately, such bad conductors of the voltaic current that they do not leave the least possibility of availing ourselves of the use of the galvanometer. It is not the same, however, in liquid substances; but I have made use of them to make a series of experiments, of the results of which I have hereafter spoken.

We know that an acid solution of chlorure of cobalt, a little concentrated, is blue, but that it becomes yellow by the addition of a small quantity of water. If this yellow liquid be heated, it retakes its blue colour, and this colour becomes deeper as the colour of the liquid is more elevated. The chemists explain this passage from blue to yellow by supposing the water to change to acid salt a part of its acid, and that thus the yellow solution encloses another combination than that which is found in the blue solution. As the yellow liquid again becomes blue by a new addition of hydro-chloric acid, and as heat alone produces also this change of colour, we can easily suppose that at a more elevated temperature the yellow neutral solution of cobalt is transformed into the acid blue combination; or, what is precisely the same thing, that the acid carried off by the water to acid salt separates itself a second time from the water by the effect of heat, and forms anew with the neuter combination of acid chlorure. But if chemical modifications of this species do really take place, the electrical equilibrium in the liquid in question ought also, in consequence of what I have already advanced, to be destroyed in these circumstances. If now we put the liquid into a tube in the form of U, and a platinum wire in each branch of the tube, and now heat the column of liquid in one of the branches, until it becomes blue, and then bring the free extremities, of the platinum wires into communication with a delicate galvanometer, there is a current established which travels from the cold column of liquid to the heated one, and we find the force of this current to be greatest, when the difference of temperature between the two branches is most considerable. In my experiments the deviation was about 70° when the liquid was near the point of boiling, that is to say, when the colour was the deepest. I need scarcely add that the needle returned to zero, as soon as the two divisions of the liquid had arrived at the same degree of temperature; that is to say, as soon as the blue colour of the one had again taken the place of the yellow

Such was the exactness also of the manner in which the galvanometer carried itself in the solution of nitrate acid of iron, which is colourless at the ordinary temperature, and which takes a yellow colour on being heated. In the same circumstances as those already given, I have obtained a current which travelled equally from the cold portion of the liquid to the heated one, and which made the needle deviate about 40°. The results have been similar to these too, when, in the place of the liquids of which I have spoken, I have made use of a solution of acid sulphate of iron, or of liquid combinations of nitrous acid with other acids, such as sulphuric acid, phosphoric acid, nitric acid, &c.

In truth, it seems at first that these observed currents are of a thermo-electric nature; that is to say, that they are the effect of the difference of temperature of the two liquids, or of the two platinum wires. M. Becquerel says, in his "Traité de l' Electricité," that when the two extremities of the platinum wire of the galvanometer are plunged into nitric acid, and that in these circumstances, the electrical equilibrium is maintained, this equilibrium will be destroyed, if we come to draw one of the extremities out of the liquid, heat it, and plunge it in anew, and that then a current will be developed which travels from the cold extremity to the hot. The French philosopher considers this current as being of a thermo-electric nature. But if this opinion have any foundation, we should be able to obtain similar currents with all liquids which are good conductors. Now my researches on this point have convinced me to the contrary. Sulphuric acid, perfectly pure, alone, or with water in different proportions, pure hydrochloric acid, the dissolutions of potash, sulphate of potash, carbonate and phosphate of alkali, sulphate of zinc, corrosive sublimate, and many other salts, have been put successively into bent tubes; the part of the liquid enclosed in one of the branches only has been heated, and when I have established the communication with the galvanometer, by means of the platinum wires, I have not obtained even the most feeble current. The absence of the current in this last experiment, seems to bring clearly in view the inaccuracy of the explanation given by M. Becquerel, and at the same time the great likelihood, or else the certainty, that the destruction of the electrical equilibrium, when it coincides with the change of colour in a liquid, is not immediately owing either to the difference of temperature of the two wires, or to that of the two portions of liquid contained in the branches of the tube, and communicating with each other, but to transient chemical modifications, which have been produced by heat in one of these portions.

It is scarcely necessary to remark expressly, that it is possible also to have liquids whose colour does not change, in whatever way they are submitted to the action of heat, and in which notwithstanding, transient chemical changes, take place, for the qualitative changes of a body are not always necessarily accompanied by a change of colour. Those liquids which are found in this position, should also, by consequence, be in a state to produce a current when they are unequally heated. Now the result of my experiment proves that the dissolutions of many nitrates of mercury possess in a very high degree the property in question, when they are submitted to an unequal heat, and it is known that the solutions of this species are colourless at very different temperatures.

Let us now suppose that the preceding remarks are perfectly exact; the result would be that a galvanometer would offer to the observer an instrument, which would put him in a position where he could demonstrate the existence of chemical actions, in which no reaction is announced, and where up to the present time no modification was believed to be operating in the chemical constitution of the substance under observation. I have already named, besides the galvanometer, the chemical microscope, and I believe that the facts which I have just described are sufficient to justify this denomination. It will be by consequence very desirable, that those chemists who are devoted to scientific research, will make use of this precious instrument more frequently than they have hitherto done, and that they will make above all an attentive examination of all the important chemical combinations, which serve as conductors of the current, in order to knew the influence which they exercise on the galvanometer when they are unequally heated.

Permit me, in terminating this work, to express some ideas destined to make comprehensible the importance which isometry will probably sooner or later exert, for that part of geology which is united to chemistry. In considering under a chemical point of view the parts which constitute the crust of the earth, we ought to be surprised to see certain elements predominate over others in the rocks of certain geological formations. I do not wish to recall here the enormous masses of calcareous carbonate, which are encountered in those which are called sedimentary earths. On the other side it is not rare to find in the same formation chemical productions extremely different, placed one by the side of the other, and, what is very remarkable, in such a manner sometimes that the one passes the other by degrees almost insensible, as is seen for example in the calcareous carbonate and the dolomite. These transitions take place sometimes under circumstances which cause us to think of the transformation of one of these substances in the other. And, in effect this idea has been suggested at an anterior epoch, but has ordinarily been repulsed as a product of the imagination of the alchemist, and has been declared completely inadmissible.

Under a chemical point of view, we ought, it is true, to suppose that since our earth exists, the same elements of which we have knowledge at the present day have always existed, and that all the diverse geological formations, inasmuch as they relate to chemical actions, have been owing to the affinity of one of these elements for the other. As to the transformation of one substance into another, it is a fact we are not able to admit; and then say, how, under the relation of quantity, the elements are united in such a manner as to be able to form precisely compound combinations after the chemical proportions, and how these substances, which have power to combine among themselves, have so happily met: the chemists' do not believe themselves bound to give in this point of view any plausible explanation, and they put this fact to the number of those on which actual science does not possess sufficient elements.

Further, the singular fact, that certain substances, accompany each other, or always avoid each other, and that these substances, are then found as often to be of bodies which offer a passable resemblance in their chemical character, such as, amongst which we find together, the chlorine, brome, and iodine; sulphur and selenium; platinum, iredium, palladium, baryte, and strontian; potash and alkali; this fact ought to be considered by the chemist of our days as a pure hazard, two elements being always separated in his eyes by an abyss quite impossible to overcome.

In the opinion of many philosophers, there may have been a time in which all the constituent elements of our planet may have existed in a state of insulation. But this hypothesis implies here, that the compound bodies with which we meet at the present day, have been formed one day by means of synthesis. We can, according to my ideas, make good several arguments which are not favourable to this point of view, and which permit the supposition that many chemico-geological products have been found by another way than that of composition, by means of elements which we can at the present day separate. If the substances which we regard as elementary, were once found in a state of complete insulation, and they may have been at the same time submitted, as at our day, to the law of gravity, they would have been obliged, it seems, to arrange themselves the one on the other, according to their specific gravity. But as it is easily comprehended, would alone have sufficed to hinder the combinations of several of those elements which are now found united. In truth, when the chemist pretends that in the primitive times, these elements, disposed one on the other, in beds or layers nearly concentric, well mixed together by the effect of an unknown cause, and which will come suddenly into activity (an hypothesis which is accorded to it, as we accord to astronomy that of an impulsion, when there is need to explain the curvilinear movement of the planets), this hypothesis will make comprehensible the existence of many geological products; but at all times, a great number of geologico-chemic facts will remain enigmatical to us, and even inexplicable.

But if many substances which we consider as compound, have not been formed by the way of ordinary synthesis, it is still a question, at least to admit, for the sake of convenience, that they have been created such as they are at the present day, or that they have always existed in the same state, it is yet a question, what origin have they had. As for myself, in acknowledging all that we cannot yet answer to this question, no more than others, concerning the origin of mineralogical products, I think that isomery will aid us as we advance to resolve a great number of chemico-geological problems. As this new branch of chemistry becomes developed sufficiently, for the acknowledgment as isomeries, those substances which we have been forced to regard up to the present day, as elements, and we shall the light of day spread itself on many subjects which are yet covered in a profound obscurity.

It is a true principle, and let it for that be often repeated, that nature attains by the most simple means, the greatest and the most varied ends. How complicated and how grand are the effects produced by gravity, the action of which at the same time obeys a law so simple! If then we suppose that the numerous different substances which constitute our earth, are the product of a small number of elementary substances, united amongst themselves in a manner the most varied in their proportions, and in the mode of their combination, we have there an hypothesis which we are authorized to make by analogies, and which will scarcely leave us accusable of wandering in reveries or metaphysics. Let us represent the small number of substances which we admit hypothetically as elements, submitted to the influence of very different temperatures, of voltaic currents of different energies of various forces of pressure, &c., we may conceive how in these circumstances so different, the bodies the most different have power to be formed by means of a small number of elements, these bodies in particular, which we use for decomposition, relatively feeble, do not permit us to resolve into their elements. We know already some facts, which give us authority to presume, that some substances which are now regarded in chemistry as simple bodies and which ought as such, to be invariable in their essential properties, would evince very considerable modification when they are exposed to certain influences and in particular to those of electrical currents and to heat. It has long been known that sulphur can assume very different forms, and that, suddenly cooled after having been heated it passes to a state of coherence essentially different from that which it previously possessed. Phosphorous and selenium present similar phenomena. In my electro-chemical researches, I have myself recently obtained results, which demonstrate that iron, which is regarded as elementary, can, under the relation chemical and under the relation physical, take such modifications, that, in its new estate one ought to regard it in some manner as quite another metal. From the state of a body very oxidable, it is transformed into a substance neuter with regard to oxygen; a metal eminently electro-positive, it becomes a metal electro-negative. Similar modifications have already been observed in some other very oxidable metals. Though these modifications are it is true but transient, and that up to the present time no means have been found to render them permanent, it does not yet follow that, by, example, this result will not absolutely be obtained for iron. It results from this, that the above modifications, are a proof that many bodies which are called elementary, do not bear the character of absolute invariability, in that which concerns the properties which are ordinarily regarded as essential.

In like manner, as the chemist ought to furnish to the geologian his aid in extending that science, so ought the geologian, in his turn, to lend his aid to the chemist. Like light, are not the geological explorations spread on the history of organized beings, and what discoveries may we not hope to make in the field of zoology, by the researches even which are made in our days, on the nature of animals which peopled the primitive world?

We may be well permitted to suppose that the formation of the inorganic bodies of our earth has taken place after determined laws, as much even as that of organised beings which have perished; that in other periods of the history of our planet, there have been epochs of chemical formations, as there have been periods of organic creation: and is it not impossible that both the one and the other have been in a certain mutual dependance, and that one of these classes of phenomena has had in the other the cause of its existence? Now, if, in the actual moment, geologians were to make a strong effort to direct all their attention on the organic remains of primitive times, and, by a self-compulsory effort, construct, with the monuments of the first ages, a basis for the history of our globe; if, further, we should acknowledge that in the course of the last twenty years the zeal and the perpicuity of geologians devoted to this part of the zoology and botany which belongs to their science, have obtained the most extraordinary results in this domain, and have arrived at the solution of problems the most difficult, it ought not to be a case for doubt, that the chemical side of geology has not at least attracted that regard which it merits. But as it is certain that essential modifications, which our globe has suffered in anterior epochs, have been produced by chemical forces, it will result therefrom, that the geologian must necessarily study the matter of the globe under a point of view purely chemical.

In order to arrive in the field of research at results which have some value in science, we ought to take the same ways by which the geognostic zoologists have acquired the knowledge which they possess on the organic life of the primitive world. We ought to study with the greatest degree of care each particular geognostic product; we ought to determine also, as exactly as it is possible, the mutual relations of these products in their nature, both chemical and physical, and in their chronological succession; and, at the same time, to make the most scrupulous comparison between the product which we obtain by the aid of chemical forces yet active at the present day, and the inorganic bodies of the primitive world. In a word, we ought to commence by creating a geochemic comparison before we can possibly have a true geology, before the mystery of the creation of our world can be discovered, and the masses which compose it. But in order to arrive at this elevated and truly gigantic end, which is proposed to science, it is necessary not only to take advantage of all the men who possess not only all the knowledge which philosophy and chemistry is able to furnish, but also who are endowed with that rare faculty of grouping and collecting, under different, general points of view, those masses of particular facts, and to discover a relation and connexion between phenomena altogether strangers in appearance. It is necessary that there arise a man who will be to geological chemistry that which Cuvier has been to the anatomy of the animal fossil kingdom, and what Newton has been to astronomers.

XXX.—*On Electro-type from Engraved Copperplates.* By SAMUEL CARTWRIGHT, Esq.

Having been favoured by Samuel Cartwright, Esq., of Preston, with an excellent specimen of printing on address cards, from an electro-type plate which that gentleman had made, I immediately wrote to him requesting that he would favor me with the plate in order that I might be enabled to place specimens of printing, from it, before the readers of these Annals. Mr. Cartwright finding that he could not comply with this request, in consequence of the plate being presented to the Lancaster scientific exhibition, immediately proceeded to prepare another, which, as soon as ready, was very kindly presented to me by that gentleman, who waited on me at this Institution for that purpose and, in the most liberal manner, communicated to me all the particulars of the process, which is the following.

To the back side of the engraved plate intended to be copied, is soldered one end of a copper wire; and to the other end of that wire is soldered the zinc plate of the operating voltaic battery, which consists of a single pair of metals placed in a porcelain jar, having the copper side in a solution of sulphate of copper, and the zinc side in water, or in salt and water. To the farther end of the conducting wire belonging to the copperside of the battery is soldered another copper plate, about the size of the engraved one. When the battery is prepared, the two copper plates at the farther extremities of its wires, are to be placed in a strong solution of sulphate of copper, the engraved one with its face upwards, and the other directly over it, but not to be in contact with each other. With this arrangement, the cuperous solution becomes decomposed, and the liberated copper is precipitated on the face of the engraved plate: whilst the copper plate of copper in that vessel, suffers dissolution, and feeds the solution with fresh portions of copper.

At the end of about three days the newly formed plate will be thick enough to remove from the engraved one, and the impression will be a very faithful copy of the original one. having the letters and other characters in *relief*, similar to those on a card printed from the engraving.

Since Mr. Cartwright's visit to this institution, Dr. Goodwin, of this town, has taken a very good electro-type plate from the engraved copper-plate from which his address cards are printed; and several other gentlemen are now preparing electro-type plates by similar means.

From my own experience, I find that the dissolution of the copper-plate, which is connected with the copper side of the battery, is very rapid, and when thin, if care be not taken, small fragments from it will fall down on the lower plate, which, if not removed, might injure the process. To prevent any accident of this kind, I put the upper plate in a muslin bag, which prevents the fall of any fragment. If, however, the plate be sufficiently thick to outlast the process, there will be no need of this precaution. The face of the electro-type plate is as smooth as the original, and every scratch, however minute, will be faithfully transferred.

WILLIAM STURGEON.

Royal Victoria Gallery, for the
encouragement of Practical
Science, Manchester.

P. S.—On my requesting to print from Mr. Cartwright's electro-type plate, I received the following letter.

Dear Sir,—I have no objection to your publishing, as you desire, the Electro-type I made for you; but must request that your readers may, at the same time, be informed that I consider it very inferior to what a more practised person would produce.—It is only the second I have made to print from, and before attempting this process I never constructed or used a galvanic Battery.—My reason for sending it to you was, as I have before stated, to shew that the process may be performed by any person who will take the trouble of reading the accounts of it published in you Annals of Electricity, and other periodicals.

The plate is just as it came from the Battery, no graver, burnisher, or even charcoal has touched it, except in drawing the line under my name, to distinguish its impressions from those of the original plate.

Yours truly,

SAMUEL CARTWRIGHT.

Preston, 21st Aug. 1840.

The next business is to proceed with the new electro-type plate, in the same manner as had previously been done with the engraved one; and a *fac-simile* of the latter will be obtained. The process should be continued four or five days to give sufficient thickness to the plate to be printed from.

In order that the electro-type may leave the original plate without injury, Mr. Cartwright covers the face of the latter with bees' wax, and whilst warm wipes of the greater part, leaving only a thin film. The back part of the original plate and its conducting wire are covered with sealing-wax varnish, to prevent unnecessary deposition of copper on those parts.

In the electro-type plate will be seen two specimens, one from the electro-type, the other from the original plate.

W. S.

---

* When electro-type copies of medallions are to be formed in casts of plaster or other porous materials, Mr. Spencer gives the following ingenious mode of giving those matrices the necessary surfaces.

The porous matrix is to be dipped into a weak solution of nitrate of silver, a portion of which becomes absorbed. It is next exposed to the fumes of a warm alcoholic solution of phosphorous, and the absorbed nitrate of silver becomes decomposed.

Mr. Parry, an ingenious gentleman of this town, has lately obtained good electro-type impressions from the green leaves of trees, by properly placing those leaves so as to receive the deposited copper by voltaic copper.—W. S.

XXXI.—MISCELLANEOUS ARTICLE.

On the Application of Electro-Magnetism as a Motive Power; in a Letter from Prof. P. FORBES, of Aberdeen, to MICHAEL FARADAY, D. C. L., &c. &c.*

King's College, Aberdeen, Oct. 7, 1839.

My dear Sir,—Having seen a notice from Mr. Jacobi sent by you to the London and Edinburgh Philsophical Magazine,† regarding the success of his experiments on the production of a moving power by electro-magnetism, I am sure it will give you pleasure to know that a countryman of our own, Mr. Robert Davidson, of this place, has been eminently successful in his labors in the same field of discovery. For in the first place, he has an arrangement by which with only two electro-magnets and less than one square foot of zinc surface (the negative metal being copper) a lathe is driven with such velocity as to be capable of turning small articles. Secondly, he has another arrangement, by which, with the same small extent of galvanic power, a small carriage is driven on which two persons were carried along a very course wooden floor of a room. And he has a third arrangement, not yet completed, by which, from the imperfect experiments he has made he expects to gain very considerably more force from the same extent of galvanic power than from either of the other two.

The first of these two arrangements were seen in operation by Dr. Fleming, Professor of Natural Philosophy in this University, and myself, some days ago; and there remains no doubt on our minds that Mr. Davidson's arrangements will, when finished, be found available as a highly useful, efficient, and exceedingly simple moving power. He has been busily employed for the last two years in his attempts to perfect his machines, during all which time I have been acquainted with his progress, and can bear testimony to the great ingenuity he has shewn in overcoming the numberless difficulties he has had to encounter. So far as I know he was the first who employed the electro-magnetic power in prodncing motion by simply suspending the magnetism without a change of the poles. This he accomplished about two years ago. About the same time he also constructed galvanic batteries on Professor Daniell's plan, by substituting a particular sort of

---

[*] Communicated by Dr. Faraday.

[†] See L. & E. Philos. Mag. for. Sept., p. 164.

canvas instead of gut, which substitution answers perfectly, is very durable, and can be made of any form or size. And lastly, he has ascertained the kind of iron, and the mode of working it into the best state for producing the strongest magnets with certainty.

The first two machines, seen in operation by Dr. Flemming and myself, are exceedingly simple, without indeed the least complexity, and therefore easily manageable, and not liable to derangement. They also take up very little room. As yet the extent of power of which they are capable has not been at all ascertained, as the size of battery employed is so trifling and the magnets so few: but from what can be jugded by what is already done, it seems to be probable that a very great power, in no degree inferior to that of steam, but much more manageable, much less expensive, and occupying greatly less space, if the coals be taken into account, may be obtained.

In short, the inventions of Mr. Davidson seem to be so interesting to rail-road proprietors in particular, that it would be much for their interest to take up the the subject, and be at the expense of making the experiments necessary to bring this power into operation on the great scale, which indeed would be very trifling to a company, while it is very serious for an individual by no means rich, and who has already expended so much of his time and money for the mere desire of perfecting machines which he expected would be so beneficial to his country and to mankind. For it deserves to be mentioned that he has made no secret of his operations, but has shewn and explained all that he has done to every one who wished it. His motives have been quite disinterested, and I shall deem it a reproach to our country and countrymen if he be allowed to languish in obscurity, and not have an opportunity afforded him of perfecting his inventions and bringing them into operation, when the promise to be productive of such incalculable advantages.

L. & E. Philos. Mag.

are at present agitated. He concludes this, his first report, by recommending a series of enquiries, ten in number, which will supply the desiderata immediately required by the engineer and by the chemist.

We have next to notice a report by Professor Powell, "On the present state of our knowledge of Refractive Indices for the standard rays of the solar spectrum in different media." The difficulty which the fact of the dispersion of light has offered to the universal application of the undulatory theory, has been in a great measure removed by the analysis of Cauchy and others, who have considered the distances of the undulatory particles as quantities comparable to the length of a wave. Velocities of propagation of the different rays of the spectrum, are made to depend upon the length of wave which constitutes a ray of a given colour, and upon certain constants proper to the medium. These constants being obtained from observations on refractive indices for certain definite rays (or dark lines) of the spectrum, the refrangibility of any other definite ray (whose wave-length has been ascertained by examining an interference spectrum), becomes known, and may be compared with observation as a test of theory; such experiments have been made by Frauenhofer, Rudberg, and Professor Powell, who has given a tabular view of the various results, without, however, instituting the comparsion between theory and observation, which it would be desirable to extend further than has yet been done. It would be important also to elucidate the disturbing effect of temperature, which prevents even existing observations from being rigorously comparable.

The calculations respecting the tides, which have been prosecuted by the aid of the Association ever since its institution, have been continued this year by Mr. Bunt, under the direction of Mr. Whewell. These calculations have now reached such a point, that the mathematician, instead of being, as at the beginning of this period, content with the first rude approximations, is now struggling to obtain the last degree of accuracy.

The country in which we are now assembled has always been conspicuous for attention to meteorology, a branch of physical science in which the British Association, with its power of combining the efforts of many observers in distant quarters of the globe, may hope to be especially useful.

In Scotland, Leslie opened a new train of inquiry, by examining the earth's temperature at different depths; and his successor in the University of Edinburgh is now directing, at the request of the Association, a large and complete course of experiments on that interesting subject. Framed in conformity with the plans adopted for similar objects by Arago and Quetelet, these researches of Professor Forbes contain also the means of determining the power of conducting heat, which different sorts of rock possess; and may thus throw light on some of those peculiarities in the distribution of temperature at greater depths below the surface, which have become known by experience, but are not explained by theory.

In Scotland, Sir David Brewster was the first to obtain an hourly meteorological journal for a series of years, and to draw from that fertile source new and important deductions, which have had a powerful influence on the progress of scientific meteorology. How gratifying to receive, through the same hands, after a lapse of nearly fifteen years, an additional contribution of the same kind, and from the same country; but embracing new conditions, on a new line of operations, in order to obtain new results. By the observations now in progress at Inverness, and at Kingussie, the influence of elevation in modifying the laws which have been found to govern the hourly distribution of heat near the level of the sea, may be discovered, and thus a great addition be made to the experimental results, for which science has long been grateful to the distinguished philosopher we have named, and which have been described as "of the highest value to meteorology, and as the only channel through which any specific practical information can be obtained in this most interesting department of physics."

This is no ordinary praise. It is the just tribute of one who is worthy to offer it; one, who at the call of the British Association, has conducted at Plymouth a still more extensive series of similar observations, and has added to them hourly comparisons of the temperature and moisture of the air, and an hourly record of barometric oscillations. Mr. Snow Harris has presented in a few pages of our last report the precious results of 70,000 observations, and thus rendered them immediately available in the foundations of accurate meteorology. The documents thus patiently collected are, however, not yet exhausted in value; they may be again and again called into the court of science, and made to yield testimony to other, and as yet, unsuspected truths. They must not be lost. Shall we lay them by in manuscript among other unconsulted records of the past labours of men, or by undertaking their publication, do justice to our workmen, and establish a new claim on the imitation of the present, and the gratitude of future days? This question is of serious import. Already, stimulated by success in thermometric registration, we have set to work on a more perplexing problem; we have resolved to bind even the wandering winds in the magic of numbers. While we speak, the beautiful engines of our Whewells and Oslers are tracing at every instant of time the displacements of the atmosphere at Cambridge, at Plymouth, at Birmingham, in Edinburgh, in Canada, in St. Helena, and at the Cape of Good Hope; and, ere long, we may hope to view, associated in one diagram, the simultaneous movements of the air over Europe, America, Africa, India, and Australia, recorded with instruments which *we* have chosen, by men whom *we* have set to work.

Amongst the causes which tend to retard the progress of science, few, perhaps, operate more widely than the impediment to a free and rapid communication of thought and of experiments, occasioned by difference of language. It appeared to the British Association that this impediment might, in some degree, be removed, as far as regards our own country, by procuring, and causing to be published, translations of foreign scientific memoirs judiciously selected. Accordingly, at each of the meetings at Newcastle and Birmingham, a grant of 100*l.* was placed at the disposal of a committee appointed to carry this purpose into effect. Aided by the contributions of several translations which have been gratuitously presented to them, the committee have been enabled, in the two last years, to publish fourteen memoirs on subjects of prominent interest and importance in the mathematical and physical sciences, bearing the names of some of the most eminent of the continental philosophers.

Such, gentlemen, is an imperfect review of our recent proceedings. In two essential respects the British Association differs from all the annual scientific meetings of the Continent, no one of which has printed Transactions or employed money in aiding special researches. We also differ from them in the communications which, in the name of the representatives of science, assembled from all parts of the United Kingdom, we feel ourselves authorized to make from time to time to the Government on subjects connected with the scientific character of the nation. On our first visit to Scotland, for example, we felt it to be an opprobrium that this enlightened kingdom should, in one essential feature of civilization, be still behind many of the continental states, and we prepared an address to his late Majesty's Government, urging strongly the necessity of the construction, without delay, of a map of Scotland, founded on the trigonometrical survey. Representations to the same effect have since been made by the Royal Society of Edinburgh, and by the Highland Society, and the subject has now engaged that attention which will, we trust, soon procure for this country the first sheets of a large and complete map.

If, then, it be asked why are the men of highest station happy to associate and mingle with us in official duties? Why have the heads of the noble houses of Fitzwilliam, Lansdowne,* Northampton, Burlington, Northumberland, and Breadalbane, alternated in presiding over us, with our Bucklands, our Sedgwicks, our Brisbanes, our Lloyds, and our Harcourts? Why, indeed, on this very occasion has Argyll himself, overlooking the claims due to his high position, and his ancient lineage, come forward to act with us, and even to serve in a subordinate office? May we not reply, that it is, we believe, a consequence of the just appreciation on the part of these patriotic and enlightened noblemen, of the beneficial influences which this Association exercises in so many ways on the sources of the nation's power and honour.

If we have hitherto dwelt almost exclusively on the value of our transactions, researches, recommendations, and the good application of our finances, let it not, however, be supposed, that we are not also fully alive to the advantages which flow from the social intercourse of these meetings, by bringing together, into friendly communion, from distant parts, those who are struggling on (often remote and unassisted) in advancing experimental science.' If, indeed, this principle of union (which we are proud to have borrowed from our German brethren,) has been hitherto found to work so well amongst our own countrymen, we cannot but doubly recognize its value when we see assembled so many distinguished persons from foreign countries. In the presence of these eminent men, we forbear to allude to individual distinctions, conscious that any brief attempt of our own would fall far short of a true estimate of merits, the high order of which is indeed known to every cultivator of science in Britain. Well, however, may we rejoice in having drawn such spirits to our Isle; valuable, we trust, will be the comparisons we shall be enabled to make between the steps which the different sciences are making in their countries and in our own.

---

* The Marquis of Lansdowne, who had accepted the office, was prevented from attending by deep domestic affliction, and the Marquis of Northampton cheerfully supplied his place.

That advantages, indeed, of no mean order arise from such social intercourse, is a feeling now so prevalent, that foreign national associations for the promotion of natural knowledge, have rapidly increased. Germany, France, and Italy have their annual assemblies, and our allies of the Northern States hold their sittings beyond the Baltic. In all this there is doubtless much good, but an occasional more extensive intercourse of a similar nature, to be repeated at certain intervals, is greatly to be desired.

It has therefore appeared to us (and we say it after consultation with many of our continental friends, who equally feel the disadvantage), that the formation of a general congress of science might be promoted at this meeting, which, not interfering with any assemblies yet fixed upon, or even contemplated, may be so arranged as to permit the attendance of the officers and active members of each national scientific institution.

If the British Association should take the first step in proposing a measure of this kind, and should solicit the illustrious Humboldt to act as President, we are sure that scientific men of all nations would gladly unite in offering this homage to a man whose life and fortune have been spent in their cause, whose voice has been so instrumental in awakening Europe to the inquiry into the laws of terrestrial magnetism, and whose ardent search after nature's truths has triumphed over the Andes and the Altai.

If such be your suggestion, then will a fresh laurel be added to the wreath of this city. She who, through the power bequeathed to her by her illustrious offspring, conveys with rapid transit her inventions and her produce to the remotest lands, well can she estimate the value of an union of men whose labours can but tend to cement the bonds of general peace. In such a body the British representatives would, we trust, form no unconspicuous band; and with minds strengthened by the infusion of fresh knowledge, they would, on re-assembling for our own national ends, the better sustain the permanent and successful career of the British Association.

Mr. Taylor, the Treasurer, then read the Report of the Receipts and Expenditure for the past year.

Mr. Phillips announced the order of proceedings; and added, that a steamer had been placed at the disposal of the Association, which would convey the members to Arran at six o'clock on Saturday morning; and that the railway proprietors had offered to convey members to and from Ardrossan.

SATURDAY.

"On a new method of Photogenic Drawing," by Dr. Schäfhaeutl.

After some observations on the comparatively low value of all drawings taken by means of the camera-obscura, in an artistical point of view, and on the principal points on which Mr. Talbot's and M. Daguerre's methods of fixing the drawings of the camera-obscura were founded, the author proceeded to describe his peculiar methods of producing photogenic drawings in Mr. Talbot's, that is, in a negative way; then, secondly, he described two new methods of obtaining photographs in a *positive* way. His first method tended to obtain a paper of very great sensibility by a comparatively short process. He recommended Penny's improved patent metallic paper, and spreading a concentrated solution of the nitrate of silver (140gr. to 2½ drachms of fused nitrate to 6 fluid drachms of distilled water), by merely drawing the paper over the surface of the solution contained in a large dish. In order to convert this nitrate of silver into a chloride, the author exposed it to the vapours of boiling muriatic acid. A coating of chloride of silver, shining with a peculiar silky lustre, was by this method generated on the surface of the paper, without penetrating into its mass; and in order to give to this coating of chloride the highest degree of sensibility, it was dried, and then drawn over the surface of the solution of the nitrate of silver again. After having been dried, the paper was ready for use; and no repetition of this treatment was able to improve its sensitiveness. The author's process for fixing definitively the drawing was as follows:—He steeped the drawing from five to ten minutes in alcohol, and, after removing all superfluous moisture by means of blotting-paper, and drying it slightly before the fire, the paper thus prepared is finally drawn through diluted muriatic acid, mixed with a few drops of an acid nitrate of mercury. The addition of the nitrate of mercury requires great caution, and its proper action must be tried first on paper slips, upon which have been produced different tints and shadows by exposure to light; because, if added in too great a quantity, the lightest shades disappear entirely. The paper, after having been drawn through the above-mentioned solution, is washed well in water, and then dried in a degree approaching to about 158° Fahr., or, in fact, till the white places of the paper assume a very slight tinge of yellow. The appearance of this tint indicates that the drawing is fixed permanently. The author's way for reversing the drawing is, in the principal points, the same as that suggested by Mr. Fox Talbot. In order to obtain a photogenic drawing in a direct and positive way, the author uses his above-mentioned paper, allows it to darken in a bright sunlight, and macerates it for at least half an hour in a liquid, which is prepared by mixing *one part* of the already described acid solution of nitrate of mercury with from nine to ten parts of alcohol. A bright lemon-yellow precipitate, of basic hyponitrate of the protoxide of quicksilver falls, and the clear liquid is preserved for use. The macerated paper is removed from the alcoholic solution, and quickly drawn over the surface of diluted hydrochloric acid (1 part strong acid to 7 or 10 of water), then quickly washed in water, and slightly and carefully dried in a heat not exceeding 212° of Fahr. The paper is in this state ready for being bleached by the rays of the sun; and in order to fix the obtained drawing, nothing more is required than to steep the paper a few minutes in alcohol, which dissolves the free bichloride of mercury. The maceration must not be continued too long, as in that case the paper begins to darken again. The author's second method of producing positive photogenic drawings was by using metallic plates, and covering them with a layer of hydruret of carbon, prepared by dissolving pitch in alcohol, and collecting the residuum on a filter. This, when well washed, is spread as equally as possible over a heated even metallic plate of copper. The plate is then carbonized in a closed box of cast iron, and, after cooling, passed betwixt two polished steel rollers, resembling a common copper-plate printing-press. The plate, after this process, is dipped into the above-mentioned solution of the nitrate of silver, and instantly exposed to the action of the camera. The silver is, by the action of the rays of the sun, reduced into a perfect metallic state, and the lights expressed by the different density of the milk-white deadened silver, the shadows by the black carbonized plate. In a few seconds, the picture is finished; and the plate is so sensitive, that the reduction of the silver begins even by the light of a candle. For fixing the image, nothing else is required, except dipping the plate in alcohol mixed with a small quantity of the hyposulphite of soda, or of pure ammonia.

Professor Graham then gave an Abstract of Professor Liebig's New Chemical Views relative to Agriculture and Physiology, as contained in his Report on the applications of Organic Chemistry in Agriculture and Physiology.

The primary source, it is observed, whence man and animals derive the means of their growth and support, is the vegetable kingdom. Plants, on the other hand, find new nutritive material *only in inorganic substances*. It is obvious, that the last proposition, if true, will afford a firm basis on which to build the superstructure of the chemical physiology of plants. A different opinion has hitherto prevailed. The fertility of every soil has been generally supposed by physiologists to depend on the presence in it of a peculiar substance, to which they have given the name of *humus*. This substance, believed to be the principal nutriment of plants, and to be extracted by them from the soil in which they grow, is itself the product of the decay of other plants. The obvious difference in the growth of plants, according to the known abundance or scarcity of *humus*, was considered an incontestible proof of the correctness of this opinion. Yet Liebig adduces the most conclusive proofs that *humus*, in the form in which it exists in the soil, does not yield the smallest nourishment to plants. 1st. The humus or humic acid of chemists, (obtained by means of precipitating an alkaline decoction of mould or peat by means of acids,) although soluble, when newly precipitated, is known to become completely insoluble when dried in the air, or when exposed in the moist state to the freezing temperature. This is also demonstrated by treating a portion of good mould with cold water. The fluid remains colourless, and is found to have dissolved less than 100,000th part of its weight of organic matters, and to contain merely the salts which are present in rain water. Decayed wood also yields only slight traces of soluble materials. It has, indeed, been admitted by physiologists, that humic acid, in its unaltered condition, cannot serve for the nourishment of plants; and hence they have assumed that the lime of the different alkalies found in the ashes of vegetables render soluble the humic acid, and fit it for the process of assimilation. But even supposing the humic acid to be absorbed by plants, in the form of that salt, which contains the largest proportion of humic acid, namely, the humate of lime, Liebig shows, from the known quantity of the alkaline bases contained in the ashes of plants, in relation to the carbon they contain, that not so much as 1-30th of the carbon of fir wood, nor so much as 1-20th of the carbon of wheat straw, could be derived from humus in this way. 2nd. Humate of lime requires 2,500 parts of water for solution. Now, supposing all the rain water which falls upon a field to become saturated with humate of lime, and to be absorbed by the plants growing upon it, then the quantity of humate of lime, which the plants thus nourished could obtain, might be calculated. But it proves to be quite insufficient to account for the carbon contained in the corn or in beet-root grown upon the land. 3rd. A certain quantity of carbon is taken every year from a forest or meadow, in the form of wood or hay, and, in spite of this, the quantity of carbon in the soil augments—it becomes richer in humus.

The carbon of plants must therefore be derived from other sources; and as the soil does not yield it, it can only be extracted from the atmosphere. Physiologists, in attempting to explain the origin of carbon in plants, overlook the circumstance that the question is intimately connected with that of the origin of humus. It is universally admitted that humus arises from the decay of plants. No primitive humus, therefore, can have existed, for plants must have preceded the humus. That plants derive the *carbon* exclusively from the decomposition of carbonic acid, chiefly and often entirely supplied by the atmosphere, is the conclusion to which Liebig arrives. They restore oxygen at the same time to the atmosphere, agreeably to the observation of Priestley, De Saussure, and others. The decomposition of carbonic acid, it is true, is arrested by the absence of light, and then plants appear to produce and evolve carbonic acid. But then, namely, at night, according to Liebig, a true chemical process commences, in consequence of the action of the oxygen in the air upon the organic substances composing the leaves, blossoms, and fruit. This process is not at all connected with the life of the vegetable, because it goes on in a dead plant exactly as in a living one. The formation of acids is effected during the night by a true process of oxidation; the volatile oils also change into resins by the absorption of oxygen. The carbonic acid, which has been absorbed by the leaves and by the roots, together with water, ceases to be decomposed on the departure of day-light: it is dissolved in the juices, which pervade all parts of the plant, and escapes through the leaves by evaporation. Plants which live in a soil containing humus, exhale much more carbonic acid during the night than those which grow in dry situations—the decomposition of the humus in the soil affording additional carbonic acid to the roots of the former. The opinion is not new that the carbonic acid of the air serves for the nutriment of plants, and that the carbon is assimilated by them, having been advocated by the ablest natural philosophers, but has not been properly appreciated by naturalists—partly, Liebig believes, from their imperfect acquaintance with chemistry, and partly from certain objectionable experiments which were instituted by them in order to decide the point. That the developement of the plants growing from seeds sown in pure Carrara marble and in sulphur did not advance far, although sprinkled with carbonic acid water, is not to be wondered at, seeing that many conditions are necessary for the life of plants; those of each genus requiring special conditions, and should but one of these be wanting, although all the rest be supplied, the plants will not be brought to maturity. The sources of the nitrogen and earthy bodies, which all plants contain, were withheld in these experiments. The mere observation of a wood or a meadow, Liebig considers infinitely better adapted to decide the question, than all the trivial experiments under a glass globe. Having shown that the carbon of plants is derived from the atmosphere, Liebig next inquires what power is really exerted on vegetation by the humus of the soil.

Woody fibre, in a state of decay, is the substance called *humus.* This body possesses the property to convert oxygen into carbonic acid. A substance then remains, *mould,* which is the product of the complete decay of woody fibre. It constitutes the principal part of all the strata of brown coal and peat. *Humus is a continued source of carbonic acid,* which it emits very slowly. Such is the chief function which Liebig ascribes to it in vegetation. There is no reason to believe that humus, if absorbed by plants, would not be assimilated, more than sugar, starch, and gum, which humus considerably resembles, and which, when absorbed by the roots of plants, are not assimilated, but again discharged by the roots, or excreted by the leaves. Cultivation is useful, as tilling and loosen-ing the soil allows access of air to the humus, and thus gives rise to the formation of carbonic acid. When a plant is quite matured, and when the leaves, the organs by which it obtains food from the atmosphere, are formed, the carbonic acid of the soil is no further required.

The Assimilation of Hydrogen.—The solid part of plants (woody fibre) contains carbon and the constituents of water (C+H,O), or the elements of carbonic acid, together with a certain quantity of hydrogen. The wood may be formed from a combination of the carbon of the carbonic acid with the elements of water, under the influence of solar light, the oxygen of the carbonic acid being at the same time evolved. Or,—and this view Liebig thinks more probable,—plants, under the same circumstances, may decompose water, the hydrogen of which is assimilated along with carbonic acid. The oxygen disengaged from plants will therefore come from water. But the volume of this gas set free would be the same, whether derived from the decomposition of carbonic acid or of water. A part, or the whole of the oxygen besides contained in the carbonic acid, must also be set free, in the formation of such a substance as an essential oil, which contains only a small portion of oxygen, or no oxygen, as a constituent.

On the Origin and Assimilation of Nitrogen.—Prof. Liebig established the fact that the third of the organic elements is uniformly derived by plants from *ammonia*. Like water, that body admits of numerous transformations in contact with other bodies. He has demonstrated the existence of ammonia in the atmosphere, by original experiments, having obtained it in a minute but sensible quantity from rain water collected at a distance from all habitations. The diffusion of this substance in the mineral kingdom is also evinced by the existence of calcareous nitre soils and rocks, there being good reason to consider nitric acid as a product of the transformation of the former. A salt of ammonia also sublimes with the boracic acid, condensed in the hot boracic lagoons of Tuscany. Ammonia is also observable in the state of a salt in the juices of plants. The juices of the maple-tree and of beet-root are found, in the process of preparing sugar from them, to contain ammonia in considerable quantities. Putrified urine contains nitrogen in the forms of carbonate, phosphate, and lactate of ammonia, and in no other form. It is employed in Flanders as a manure with the best results. Animal manure, Liebig believes to act only by the formation of ammonia. The latter substance must also form the red and blue colouring matter of flowers. The evident influence of *gypsum* upon the growth of grasses, the striking fertility and luxuriance of a meadow upon which it is strewed, depends only upon its fixing in the soil the ammonia of the atmosphere, which would otherwise be volatilized with the water which evaporates. The ammonia, which is in the state of carbonate, is then decomposed, as in the manufacture of sal ammoniac, and the sulphate of ammonia produced. The advantage of manuring fields with *burned clay* and the fertility of ferruginous soils, which have been considered as facts so incomprehensible, are explained in an equally simple manner. The true cause is this:—The oxides of iron and alumina are distinguished from all other metallic oxides by their power of forming solid compounds with ammonia. The ammonia is separated from them by every shower of rain, and conveyed in solution to the soil. *Powdered charcoal* surpasses all other substances in the power to absorb ammonia and other gases, and has been observed to promote vegetation in an extraordinary degree. Decaying wood possesses the same property. Humus, therefore, is not only a slow and constant source of carbonic acid, but is also a means by which the necessary nitrogen is conveyed to plants. Nitrogen, Liebig observes, is found in lichens, which grow on basaltic rocks. Our fields produce more of it than we have given them as manure, and it exists in all kinds of soils and minerals which were never in contact with organic substances. The nitrogen in these cases could only have been attracted from the atmosphere. Carbonic acid, water, and ammonia, contain the elements necessary for the support of animals and vegetables.' The same substances (he adds) are the ultimate products of the chemical processes of decay and putrefaction. All the innumerable products of vitality resume, after death, the original form from which they sprung. And thus death—the complete dissolution of an existing generation—becomes the source of life for a new one.

But another class of substances is also necessary for the life of vegetables.

The Inorganic Constitution of Plants.—These substances are found in the ashes left after the incineration of plants, although in a changed condition. Many of these inorganic constituents vary according to the soil in which the plants grow, but a certain number of them are indispensable to their developement. Phosphate of magnesia in combination with ammonia is an invariable constituent of the seeds of all kinds of grasses. Plants also contain various organic acids, all of which are in combination with bases, such as potash, soda, lime, or magnesia. Of the different alkaline bases found in plants, Liebig finds reason to conclude, that any one may be substituted for another, the action of all being the same. But the number of equivalents of these various bases remains the same. The analysis of Berthier and Saussure show that the nature of a soil exercises a decided influence on the quantity of different metallic oxides contained in the plants which grow upon it: that magnesia, for example, was contained in the ashes of a pine-tree, grown at Mont Breven, while it was absent from the ashes of a tree of the same species from Mont La Salle, and that even the proportion of lime and potash was very different. But although the composition of the ashes of these pine-trees was so very different, they contained an equal number of equivalents of metallic oxides; or, what is the same thing, the quantity of oxygen contained in all the bases was in both cases the same—being expressed by the number 9.01 in one case, and by 8 95 in another, a coincidence which had escaped the notice of the analyst himself. It is certain that particular acids enter into different vegetables, and are necessary to their life; some alkaline base is also indispensible, in order to enter into combination with the acids, which are always found in the state of salts. The perfect developement of a plant is therefore dependent on the presence of alkalies or alkaline earths, and its growth is arrested when these substances are totally wanting, and impeded when they are only deficient. Hence it is that of two kinds of tree, the wood of which contains unequal quantities of alkaline bases, one may grow luxuriantly in several soils, upon which the other can scarcely vegetate. Thus 10,000 parts of oak-wood yield 250 parts of ashes, and the same quantity of fir-wood only 83 parts. Hence, firs and pines find a sufficient quantity of alkalies in granitic and barren sandy soils, in which oaks will not grow. Liebig supplies various additional illustrations of the influence of the alkaline metallic oxides on vegetation, amply sufficient to place beyond controversy these conclusions, so important to agriculture and to the cultivation of forests. One of these Professor Graham quoted: a harvest of grain is obtained every thirty or forty years from the soil of the Lüneburg heath, by strewing it with the ashes of the heath plants which grow on it. These plants, during the long period mentioned, collect the potash and soda from the decomposing minerals of the soil, which are conveyed to them by rain water; and it is by means of these alkalies that oats, barley, and rye, to which they are indispensable, are enabled to grow on this sandy heath. The supposition of alkalies, metallic oxides, or inorganic matter in general being produced by plants, is entirely refuted by such well authenticated facts. It is thought very remarkable, that those plants of the grass tribe, the seeds of which furnish food for man, follow him like the domestic animals. But none of our corn plants can bear perfect seeds, that is, seeds yielding flour, without a large supply of phosphate of magnesia and ammonia, substances which they require for their maturity. Hence these plants grow only in a soil where these three constituents are found combined, and no soil is richer in them than those where men and animals dwell together. Professor Liebig then applies these great fundamental principles, in this report, to the *art of culture*, under the following heads: use of humus—nutrition and growth of plants—necessity of azotized substances—influence of the food on the produce—composition of soils—the fertility of soils—fallow. Then, under the head of interchange (rotation) of crops and manure, he discusses the varieties and applications of particular manures, composition of animal manures, the essential elements of manure, bone manure, manure supplies nitrogen, mode of applying urine, value of human excrements. In the second part of his report Professor Liebig discusses the chemical processes of fermentation, decay, and putrefaction, under the heads of chemical transformations—eremacausis or decay—vinous fermentation—wine and beer—decay of woody fibre—on the mouldering of bodies—and on poisons, contagious matter, and miasms. The novel theoretical views with which this department of the work abounds are remarkable, equally with those of the preceding part, for their profundity and for their valuable applications. The subjects discussed, however, are numerous, and of such a nature that great injustice would necessarily be done to them in a short and hasty abstract.

Dr. Gregory stated, that having studied Professor Liebig's work, it appeared to him in the highest degree important, as being the first attempt to apply the newly-created science of Organic Chemistry to Agriculture; that, in his opinion, from this day might be dated a new era in that art, from the principles established, with such profound sagacity, by Professor Liebig. He was also of opinion, that the British Association had just reason to be proud of such a work, as originating in their recommendation.

MISCELLANEOUS ARTICLES.

On Beet Sugar; by J. C. Booth.

There are few subjects which have created more sensation in the greater part of Europe, and the United States, simultaneously, than the manufacture of sugar from beet root. That it should have induced many individuals in this country to experiment, with a view to its manufacture, the characteristic enterprise and ingenuity of our people might guarantee, but may we not assign as the chief reason of their failure, or only partial success, the fact, that too many of us still boast of our practical knowledge, with a sidelong sneer at the assistance of science. It is rather more surprising, to observe the intense and all pervading interest manifested on various parts of the continent of Europe, especially in Germany, on the sugar-beet and its important product, as it clearly shows that this learned people have received an impulse with the rest of the world, relative to more modern manufactures, or rather that the zeal with which scientific men have devoted themselves to the advancement of the arts, is now developing its effects on the mass of the community. The frequent questions asked relative to the making of beet-sugar, may be better answered by a concise description of the superior method of extracting sugar from the dried beet, the main part of the account, being taken from Dingler's Polytechnic Journal, for 1838, Bd. LXIX. The drawings in all their details will be omitted, and merely the general features of the process described.

1. *Cleansing.*—They must be washed, to free them from the soil which adheres to them, and this may be executed in a simple tub, or on a larger scale, a vat, into which water flows. A convenient arrangement for this purpose, might be a net-work cylinder, slightly declining from a horizontal position, revolving under water, or through which water should abundantly flow. The beets coming out from the depressed end of the cylinder, will be perfectly clean.

2. *Cutting.*—"They are next cut by a machine into long strips, exhibiting a square by a cross section, i. e. into long parallelopipeds, which is accomplished by a series of small knives attached to a sheet of iron, parallel to, and at short distances from each other, which first make incisions as deep as the required thickness of the pieces, and are followed by a long knife, behind and at right angles to the smaller ones,

by which the strips are separated." The knife with the smaller ones, cut by a vertical motion, but there might be a greater economy of time, by bringing a series of these cutters on a wheel, and attaching the smaller knifes to the large one, suffering them to project a little below it, so that their incisions may be immediately followed by the edge of the long knife.

3. *Drying.*—" Various and simple arrangements have been devised, for drying the pieces thus cut, in all of which the principle consists in exposing them in thin layers to a current of air, heated to a temperature of between 100°. and 145°. Fah.; for if below 100°. they are apt to ferment, and if above 145°. they are liable to decomposition. For this purpose they are placed on wire nets, in the form of drawers, to the depth of one or two inches, the drawers sliding in one over another, at the distance of three inches, to allow a free circulation of air. The drying chamber, or house, is heated, either by a hot air furnace, or by steam tubes. A better arrangement, however, and one requiring but little hand-labour, is a series of endless wire-nets, one over the other, and each passing around a roller at each end. The pieces are carried from the cutting machine, on an endless cloth, up the highest of the nets, on which they fall, and are carried to the farthest end, by its constant motion, where they fall on the next lower endless net, which at this end projects beyond the uppermost, moving in the opposite direction on to the farther end of the second, which does not reach as far as the third, they are received on the latter, and again transported to its farther end, and thus, by moving alternately in opposite directions, on the adjoining nets, they reach the lowest, from which they are thrown off in a dried, or sufficiently dried, state for use. These parallel nets are all in a chamber, heated by steam tubes from below; lower openings in the apartment admitting cold and dry air, the upper ones above the nets, permitting the egress of the hot air, surcharged with vapour. After drying they are ground to powder."

4. *Sugar Extraction.*—" The saccharine matter may be extracted by pure water, but it is found to be more advantageous to add acid or lime to it. The former is preferable, and sulphuric acid the most convenient. To nine parts of water add two-thirds or three-fourths of a pr. ct. of sulphuric acid, (according to the amount of sugar in the beets,) and stir in four, or even more, parts of the powdered beet. The stirring should be continued until the acidulated water is absorbed, when the mass is subjected to pressure in bags; the remaining mass is again treated with the same quantity of equally acid water, and pressed, but the liquid thus obtained, is used for the next fresh quantity of powder. The moistening and pressing are continued until all the sugar is extracted.

The portion first pressed out, is treated with a quantity of slaked lime, a little more than is sufficient to neutralize the acid, and the precipitation of sulphate of lime takes place fully at the temperature of 165°. to 190°. Far. The clear liquid is drawn off and crystalyzed by the ordinary sugar-refining process."

5. *Theory.*—Beside sugar, there are many other vegetable principles contained in the beet, of which gluten and albumen are the most injurious and difficult of management, but by drying they are rendered insoluble, and cease to be troublesome. It is also in consequence of the same operation, that less animal charcoal is required for purifying the sirop, than where the beets were not dried. Sulphuric acid renders more insoluble the gummy matter, and probably decomposes a combination of a portion of the sugar, but as there are other acids also present, a little more lime is added to neutralize them, than is sufficient for saturating the sulphuric acid. The beet may contain from six to twelve pr. ct. of sugar, but much of it is lost in the process of manufacture. It is similar to that obtained from the sugar-cane, and is hence called cane-sugar, to distinguish it from other varieties, as raisin or starch-sugar, sugar of milk, &c.

On Raisin Sugar; by J. C. Booth.

When raisins have been exposed to the air for a length of time, small crystaline grains will be found upon and within them, which have a sweet taste, and are a species of sugar. The same kind may be made by the action of diastase, or sulphuric acid, on starch, and indeed starch-sugar, or rather starch-sirop is much used in parts of France and Germany. The process of manufacture is as follows:—

1. *Conversion of the Starch.*—One thousand parts of water are brought to the boiling point, in an open vessel of copper or lead, and fifteen parts of sulphuric acid added, previously diluted with thirty parts of water. When the fluids are well mingled, a cover is put on the vessel with a small opening in the centre through which the starch is introduced. Four hundred and fifty to five hundred parts of dry starch, (or as much wet as contains that quantity,) are put into the opening in the cover of the vessel, in very small portions at a time, so that the fluid may continue boiling, and not become thick. A few minutes after the last portion is added, the fire is extinguished, and chalk is thrown in to neutralize the acid. The clear liquor is drawn off, when the sulphate of lime has deposited and filtered through ordinary sugar filters. It is then evaporated to one half its volume, twenty-five parts of animal charcoal stirred in with a little blood, boiled and filtered through Taylor's filtering apparatus. This is starch-sirop, from which sugar may be obtained, by evaporating to 40—45°. Baumè and cooling. It forms a white, coarsely granular mass, from which the molasses may be separated in the ordinary manner. One hundred parts of dry starch, givè one hundred and fifty parts of syrup, or about one hundred of dry sugar.

2. *The Theory of the Process.*—The conversion of starch into sugar, by this process, is one of the most singular operations of chemistry, and has given rise to a new doctrine in the science.

We perceive that by the operation on a large scale, they obtain an amont of sugar equal to that of the starch, but De Saussure obtained in a careful experiment, from one hundred of starch, one hundred and eleven of dry sugar. The sulphuric acid is unchanged, for there is the same amount remaining after the operation that was originally introduced; nothing is absorbed from the air, nor is there any evolution of gas, for the operation may be conducted equally well in closed, or in open vessels. The starch alone has changed, and this change is effected by its taking up a certain quantity of water, or rather the elements of water, hydrogen and oxygen. According to Saussure, one atom of starch takes up about two atoms of water. It appears then that the presence of sulphuric acid is sufficient to produce such an alteration among the elements of starch, that a new and different product results. For this reason and from many analagous facts, the French chemists give to this singular method of decomposition the name of *presence;* Berzelius calls it *catalysis,* which signifies a decomposition by the interchange of the elements of a body among each other. The catalyctic influence of sulphuric acid then, is to convert starch into sugar, where water is present. All other acids will produce the same result, and the same kind of sugar may be obtained in a similar manner from other organic substances, such as linen, cotton, wood, &c. But the change is not immediate, for it is observed to convert the starch first into gum, and the gum into sugar. It is, however, not the mineral acids alone, that produce this effect, for an organic substance has been discovered in malt, which possesses the same power in a higher degree. This is diastase, which converts starch into gum (or dextrine) at a temperature of 150°. to 160°. while the mineral acids require 185°. to 205°. One part of diastase will change two thousand parts of starch into dextrine, and at least one thousand parts into sugar, Through the presence of diastase, therefore, or more properly by its catalytic influence, starch of wheat, potato, &c., is first changed into dextrine, and then into sugar; a highly interesting fact, as giving us a clearer view of the formation of gum and sugar in plants, and of the processes for manufacturing alchoholic liquids, which require the presence or formation of sugar, prior to their vinous fermentation.

In concluding the above articles on the manufacture of two varieties of sugar, the following table of the amount of sugar consumed in Europe, in 1836, may not be uninteresting. It is extracted from Dingler's Polyt. Jour, lxvii. p. 319.

Kingdoms of Europe.	No. of millions of Inhabitants.	No. of millions of Sugar lb.	lbs. for one individual.
England	16¼	321½	20
Ireland	8	32	4
France	33	178½	5⅓
Prussia	14	56	5
Bavaria	4	10	2½
Switzerland	2	12	6
Belgium	4	60	15
Holland	2½	35	14
Denmark	2	10	5
Sweden and Norway	4	12	3
Spain	14	87	6¼
Portugal	3½	16½	5
Smaller German states	8	40	5
Italy	18	36	2
Austria in the commercial union	19	40	2
Austria without commercial union	15	25	1⅔
Russia	40	40	1
	207¼	1011½	

Journal of the Franklin Institute.

Copal Varnish.

The following method of preparing a copal-varnish, is not novel, but its simplicity and the superior quality of the product, may render it acceptable to many of the readers of the journal.

Enclose coarsely-powdered copal in a linen rag, and hang it in the neck of a flask, or bottle, to such a depth that it cannot touch the spirits of wine, which is in the bottom of the vessel. Tie a piece of bladder over the mouth of the flask, and make a few perforations with a pin, for the escape of a little alcoholic vapour. If the vessel be placed in a warm situation, thick and viscid drops of the copal, combined with alcohol, will slowly fall into the liquid below, and gradually dissolve, until the whole of the copal is extracted. When dissolved, the clear liquor may be decanted from a very small quantity of sediment, and it will prove a more transparent and beautiful varnish than can be procured by any other method. The same process is applicable to other difficultly soluble resins, and will be found useful where rapidity is not required.

Ibid.

Soda Manufacture in Hungary.

Native carbonate of soda is found in greatest abundance in Little Cumania, particularly near Shegedin; it likewise occurs in many other places, in greater or smaller quantity. It effloresces out of the moist earth, forming a white crust, and in the spring of the year, before sunrise, appears like an extensive covering of snow. With greater care than they now employ, the workmen might readily gather it sufficiently pure for ordinary technical purposes by raking. The whole of the surface is gathered, and sold to the soda manufacturers, who distinguish its quality and richness, by the taste. It is leached in square vats, until the remainder ceases to have a saline taste. The fluid is dark brown, and beside carbonate of soda, contains much sulphate and muriate of soda, humic acid, and other mechanical impurities. It is boiled down in a large sheet-iron pan, to a siropy consistence, transferred to an adjoining pan, and evaporated to dryness under constant stirring. The mass is of a dirty yellow, or brown, with white and black spots. It is gradually heated in a calcining furnace with the access of air, until vapors cease passing off, then fused at a higher temperature, and taken out, when partially cooled. A large portion is employed in the country itself, in the manufacture of soap, the remainder sold as raw calcined soda, as there is no manufacture for crystalizing it. If the demand for it were increased, the production of this salt might be increased to three or four times the present amount, as the country contains numerous soda lakes. Beside Trieste, from which some of the productions of Hungary find their way to the American market, there is a port on the Adriatic, belonging exclusively to that kingdom, whence we might obtain at lower rates, the products of one of the most fertile countries of Europe.

Ibid.

On Galls in the Manufacture of Black Ink.

Blue Aleppo galls are employed in great quantity, in the manufacture of black ink, in consequence of the large amount of tannin they contain, nearly all of which, by a judicious management, is converted into gallic acid. Being greatly superior to oak bark in their content of tannin, they might be substituted for it in the process of tanning leather, were not their high price a serious impediment. They are excrescences on the leaf-stem of the *quercus infectoria*, growing in the Levant, and are produced by the incision of the female gall-wasp. There is, however, another kind of galls, the acorn of the *quercus cerris*, which receive a malformation from the incision of an insect, and produces a substance not unlike the Aleppo galls, but much more irregular, and with bold projecting points. They are found abundantly in Hungary, and the southern provinces of Austria, where they are employed indyeing and tanning, particularly in the latter art. They are known under the name of Knoppern in Germany, and Galles a l'epine in France, and in the former country, are considered but little inferior to good Aleppo galls. A manufactory has been established at Vienna, for obtaining a solid extract from them, which has been successfully employed in dyeing dark colours, and in tanning. Either the knoppern, or their extract, might be obtained at Trieste, and might prove a useful substitute for ordinary galls, whether in dyeing, or in the manufacture of ink.

Ibid.

Assay of Gold.

In the last number of the Journal, a new method of assaying gold, proposed by Lewis Thompson, Esq., is extracted from the London and Edinburgh Philosophical Magazine. It consists in adding to the gold assay-piece, an excess of silver, and then fusing the mass down with the chlorides of silver and of sodium, to remove the base metals. The silver is afterwards separated by nitric acid. "By this plan," says the author, "the tedious process of cupellation is avoided."

It may not be unimportant to some of the readers of the Journal, to be informed, that Mr. Thompson's plan differs from the usual one by cupellation, only in two particulars, in both of which the old process has manifestly the advantage. In this process, lead alone is used to remove the base metals, instead of the two chlorides, and it is simpler, perfectly effectual, and not subject to decrepitation. The second point of difference is that a cupel, composed of bone ashes is used instead of a crucible; and this cupel possesses the invaluable property of absorbing the oxides of lead, and of the baser metals, and leaving a clean button, composed only of gold and silver. In the new process this advantage is not presented, and there will be grains to be separated from the crucible, as after the operation of fluxing; thus adding not only to the labour of the process, but to the uncertainty of the result. We are therefore, led to the conclusion, that the process proposed by Mr. Thompson, is more complicated, more inaccurate, and even more "tedious," than that now in universal use.

Postscript.

Sometime after the above article was communicated, an opportunity was taken of making trial of Mr. Thompson's method of assay, and the results render it proper to modify, in some degree, the above remarks. The gold, the fine silver, and the chloride of silver, were melted together in a small crucible, and the button of gold and silver formed was found to be much more perfect and better insulated than had been expected. Five assays were made, and the results, by the old and new process, expressed in thousandths, were as follows:

No 1,	by cupellation,	968	by Thompson's process	968.5
2,	"	890	"	889
3,	"	936.5	"	936.7
4,	"	900	"	900.2
5,	"	460	"	460 5

This comparison of the two methods is certainly very satisfactory, the greatest difference being but one thousandth.

In employing Mr. Thompson's process, an evil was observed which had not been anticipated. It is, that a sensible portion of the chloride of silver is volatilized during the fusion, and consequently lost. To show this, the following statements of the first and last assays—being of the finest and basest specimens, are presented. The weight 1000 is equal to between 7 and 8 grains.

No. 1. Gold, with silver	1000: no copper
Fine silver	2000
Chloride of silver	2700=2037 fine silver
	5037
Button of gold and silver after melting,	3088
Loss of silver by the process,	=1949
No. 5. Gold with silver	494+copper 506
Fine silver	1400
Chloride of silver	3000=2260 fine silver
	4154
Button after melting,	2580
Loss of silver,	1754

On the whole, though the new process is certainly not so good as that by the cupel, and is not likely ever to replace it where numerous assays are to be made, as at a mint, yet it is certainly better than was supposed when the above remarks were made, and it has the advantage, which is valuable under many circumstances, of not requiring a muffle furnace, or a cupel of bone ashes.

Ibid.

A Description of a New Form of Magneto-Electric Machine, and an Account of a Carbon Battery of considerable energy; by OLIVER W. GIBBS, member of the Junior Class of Columbia College, N. Y.

It is well known, that if a soft iron bar be wound with insulated wire and caused suddenly to approach and recede from the poles of a magnet, temporary magnetism will be induced in the bar, and an electric current in the wire surrounding it. This fact led to the construction of the magneto-electric machine, the principle of which consists in alternately inducing and destroying magnetism in a bar similarly wound with large wire for sparks and deflagrations, and with small for shocks and chemical decompositions About eight months since it occurred to me that a more simple machine than those commonly used (and which all I believe resemble that of Saxton) might be constructed My plan was, to take a bar of soft iron of say an inch in diameter by ten inches long, and to slide upon the middle a disk of brass of two inches radius. This would divide the bar into two parts, upon one of which is to be wound three or four hundred feet of copper bell wire well insulated, and upon the other and separated from the first by the brass disk, about four times that length of fine wire, say No. 25. If now one extremity of the coarse wire be attached to one pole of the battery, and the communication between the other extremity and the other battery pole be alternately made and interrupted by means of a rasp or toothed wheel; magnetism will be induced and destroyed in the iron bar and consequently an electric current will circulate through the fine wire. The use of the brass disk is to prevent by means of a closed circuit, any immediate induction in the fine from the coarse wire, which would inevitably take place were none interposed, and which would convert the instrument from a magneto-electric to an electro-magnetic machine.

Since the above was devised, an obvious improvement has suggested itself. This is founded upon the fact that magnetism is strongest at the extremities of bodies; and consists simply in dividing the bar into three equal spaces by means of two disks of brass similar in size to the one already described. The central division is then to be wound with the coarse and the two outer or polar divisions with the fine wire, connecting the two outer helices in such a manner that they may form one long wire. The battery current is then to be passed through the coarse wire, and the connection made and interrupted as before by a rasp or other interrupting apparatus. As thus constructed, the instrument would produce effects similar to the common magneto-electric machine when used for shocks or decomposition. If it be desired to produce sparks and deflagrations, it would only be necessary to slide off the coils of fine wire from the poles, and to substitute in their stead others made of coarse wire of shorter length and then transmit and interrupt the current through the central coil as before. We should then have within a much smaller compass, an instrument capable of producing all the effects of the common machine of Mr. Saxton, and by combining a number of such bars we might form in a comparatively small compass a magneto-electric battery of great energy. Some of Dr. Page's beautiful interrupting apparatus might doubtless be used successfully with this instrument. As I have no opportunity to construct the instrument myself, I would suggest the trial, especially of the latter form of apparatus, to any who may be interested in the subject. Should it succeed, its advantage would be its superior cheapness and power, (?) and the little space it would occupy.

About the same time that the above instrument was devised, in looking over the list of substances which are capable of forming a galvanic circle together, I was struck with the much higher electro-negativeness of charcoal than of copper in relation to zinc; there being but six substances between zinc and copper, while there are eleven between zinc and carbon, which, moreover, stands even higher than gold, and next below platina. Besides this, its excellent conducting power seemed particulary to qualify it to act as an electrometer. Accordingly, I was led to consider that it might form an excellent battery with zinc or its amalgam, and mentioned the opinion to Professor Renwick. I was however prevented from experimentally demonstrating its powers, until in the month of March I perceived in one of the foreign journals a short account of a carbon battery which had been successfully tried in

England. I immediately constructed a small battery, consisting of only six pairs of zinc and bituminous coal, and arranged as a *couronne des tasses.* The zinc plates were an inch square, consequently there were only six inches of acting zinc surface; the exciting liquid was diluted sulphuric acid. With this battery pure water was easily and rapidly decomposed, though from not having platina electrodes, and from the want of a voltameter, the gas collected was not measured. This experiment was witnessed by Mr. Schaeffer, assistant Professor of Chemistry in the College. To those who possess batteries of considerable power, I would suggest the employment of some form of carbon for electrodes in the place of platina. I hope soon to be able to present a series of experiments on the relative advantages of copper and carbon, especially in the case of the constant battery.

New York, May 9, 1840.

Electricity in Machinery; by AZARIAH SMITH, jun.

Messrs. Editors,—Having frequently heard persons employed in my father's manufactory at Manlius, N. Y., speak of the development of electricity by particular parts of the machinery, I was led by an article in the American Journal for (July ?) 1839, to the examination of the phenomena which furnished me with the following facts; which you will please to publish if they add anything to the light already existing upon this subject.

Upon approaching the machinery referred to, which was connected with the spinning apparatus, and near the centre of the manufactory, I observed fibres of cotton of all lengths up to six inches, extending out in different directions from one end of the spinning frames, and waving as if about to leave their resting place for a band two and a half inches broad, which moved the machinery and connected it with a drum seven feet above; the latter being moved by another drum fifteen feet distant, with which it was connected by a horizontal strap, seven inches in breadth. The two drums were of equal diameter, two feet and eight inches, but the wheel by which the spinning machinery was moved and a free pully by its side were only eight inches; and consequently made two hundred and eighty-eight revolutions in a minute, while the former made seventy two.

Beneath the horizontal strap, and four feet distant from it, the hair of the persons spinning was observed to be affected in a similar manner with the cotton, all the finer and more flexible fibres standing directly upright. Upon placing small fibres of cotton from one to two feet distant from this strap, they would ascend to it, and, adhering to its surface advance with it until within a short distance of the drum around which it passed, when they would fall off and descend to the floor. Occasionally fibres would pass to and fro between the band and the hand placed near it, and once or twice this latter phenomenon took place through a space of two or three feet.

Upon slipping the narrow band from the wheel moving the machinery to the free pulley by its side, the electrical attraction of both the bands was observed to disappear, and this notwithsanding their motions were the same as before—in a moment, however, it was again manifested upon the spinning machine being set in motion by slipping the back upon the motor wheel. This latter phenomena led to an inquiry into the different circumstances of the band in the two cases, when the idea was suggested that the wheel and the free pulley might be made of materials possessing different conducting power, but this a machinist of the manufactory informed me was not the case, both being made of iron and covered with leather. The friction of the spinning machinery, and of the motor wheel upon its axis, which were present in one, but absent in the other case, was the next difference suggested to account for the change, but as the axes of all parts of the machinery were made of iron and connected with iron frame-work, it was concluded that friction here would have no tendency to accumulate electricity. Upon watching the broad horizontal band at the moment the narrow one was slipped from the motor wheel upon the free pulley, the part of it connecting the upper part of the drums was observed to relax, while that connecting their lower surfaces, from being curved downwards by its weights became proportionally tense. In the first case, the upper part of the band was made tense by the great amount of friction in the machinery which it had to overcome, and of course, the friction of the band upon the drums was increased in the same ratio. But when the free pully only was turned, the friction to be overcome, and consequently that of the bands, was much diminished; and this increased amount of friction of the bands upon the drums in the first case, is to be referred to as the exciting cause of the electricity.

From this statement you will observe that there was no friction of the bands upon each other as is mentioned in the article referred to above, since the horizontal bands were parallel, and the vertical ones eight inches apart at their nearest approximation. In another part of the manufactory, however, two portions of a band were observed which were crossing and rubbing upon each other, but their friction was attended with no observable electrical effects. At this time however the band was passing around a free pulley; I was therefore led to inquire as to its electrical state during the motion of its machinery, and ascertained that its attractive power for cotton, &c. at such times was as great as in that of the bands already spoken of.

Although these facts do not authorize us to dispute those in Mr. —— article, yet they naturally suggest the question whether the electricity in that case was not excited by the friction of the band upon the wheels rather than upon each other, and if so, whether the apparent difference between the bands below their junction and above was not in reality caused by the application of the jar, in the one case to a tense, and in the other to a relaxed portion of the band.

Not being intimately acquainted with the action of electrical apparatus in different circumstances, I am unable to say whether increased pressure of the whole flap of the common machine upon the cylinder would materially increase the amount of electricity developed, but from the above facts, as well as the nature of the case, I should suppose it would, and if so, the circumstance properly attended to in the construction of electrical machines, would render them, *cæteris paribus*, much more powerful.

Human Fossil, alleged to be Antediluvian.

A discovery of an interesting nature, which, it is said, has recently been made in Belgium, at this moment invites the inspection of the scientific and the curious at a house in Leicester-square. It has been laid down by Cuvier, and received as an axiom in geology, that the bones of the inferior animals alone were to be found in a fossil state, and that those of man were invariably wanting; a theory whose tendency militated against the Mosaic account of the creation. In the science of geology there is consequently no problem whose solution offers greater interest than that which depends on the existence or absence of the human antediluvian fossil. This question has now, to all appearance, been set at rest by the discovery lately made (?) of the fossil remains of a child, which were found embedded in silex, in a chalk quarry at Diehgen, near Brussels. We understand the proprietor of the fossil has requested the attendance of the Marquis of Northampton, and several members of the geological society, to inspect and test it with the most minute scrutiny. The result of this inspection must be decisive of its claims to antediluvian origin. The appearance which it presents is that of the head and trunk of an infant, completely formed, but apparently much compressed. The head is perfect—the nape of the neck, the articulations of the vertebræ, the bones of the throat, the chest, shoulders, and parts of the arms equally so, and the ribs are distinctly visible. The right arm is broken short off by the shoulder; the left, which is unmutilated, adheres to the side, and is sunk into it. The lower extremities are indistinct, being thrown up into a circular mass below the abdomen. From a section of the lower part, which was accidentally made in its discovery, the formation of flint, in which it was preserved, is at once apparent, and on its surface portions of the bones are clearly to be traced.

An Aurora Borealis of considerable magnitude and brilliancy, but attended with no peculiarity, was seen here from seven till eleven o'clock on Monday evening the 19th instant. It consisted principally of a strong, steady light in the northern heavens, with the usual black, foggy nucleus below; and of many fine streamers which were displayed at different times during its appearance. The colour of both streamers and steady light was of a misty white.

W. STURGEON.

Manchester, October.

ANSWERS TO CORRESPONDENTS.

1st. The electro-magnetic engine with the rotating disc can never be an effective one; and we would advise our correspondent not to loose any time in making engines on that principle.

2nd. Magnetic-electrical machines, having soft iron magnets instead of permanent ones, have long been before the public. Our correspondent may see several of them at Watkins and Hill's Establishment, 5, Charing Cross.

3rd. We do not see that Mr. Uriah Clarke has omitted any part of the description of his electro-magnetic carriage, excepting, perhaps, some wheel, or wheel and pinion inside. He has given the *kind* and *extent* of his batteries; and has also stated that "the carriage is propelled by an arrangement of machinery on the reciprocating principle; and this reciprocating principle was previously described in the Annals for July last. (See vol. 5, p. 33. fig. 3, plate 1.) If there be any wheel and pinion in the arrangement, the pinion will be on the axle of the fly wheel, and the wheel in which it works on one of the axles of the travelling wheels, Hence the fly wheel will make more revolutions than the travelling wheel.

The Daguerreotype plates may be viewed to advantage by means of either a convex lens, or a spherical concave mirror. If the plate be held before the mirror, and the eye a little above the plate, the effect is very beautiful.

Wishing at all times to comply with the solicitations of our subscribers, we have now undertaken to write a series of familiar, and we hope, instructive lectures on the various branches of Electricity and Magnetism, including Mechanical-Electricity, Galvanic-Electricity, Voltaic Electricity, Thermo-Electricity, Magnetic-Electricity, Magnetism, Electro-Magnetism, and Electro-Chemistry.

We are aware, that this undertaking is an arduous task, but being anxious to give every assistance in our power to amateur experimenters, and to render the "Annals" still more generally useful than heretofore, we are in hopes that, by introducing this novel feature into the work, much may be accomplished by assisting those of our readers who, in consequence of the defective condition of our old standard works, and the imperfections and palpable absurdities which later writers have introduced to their productions on these subjects, may be without any other guide in conducting their experimental inquiries.

We are well aware of "the lamentable defect in this kind of knowledge which has recently been elicited in a quarter where one would least have expected it," as observed by a correspondent. And, "probably a few *close* discussions might be the means of developing the talents of the *elite* of British electricians too, although silence may possibly be a wisdom in some of them who have had fair opportunities of exercising their puissance in support of their pretended discoveries, their declining to enter the lists is no favourable interpretation of their claims to public credit."

LECTURE I.

In introducing these lectures to the readers of the "Annals of Electricity, Magnetism, and Chemistry, &c.," as they are intended principally for the instruction of amateur experimenters, it will be necessary to avoid, as much as possible, all those phrases and technicalities which not only puzzle, but absolutely mislead, even those who have, in their own estimation, much higher pretensions to a knowledge of such fashionable appendages to scientific literature, than the persons for whose instruction these lectures are intended; and to whom, therefore, I shall address myself with freedom, and in the plainest language that the present state of these subjects appear to me to be capable of admitting. I do not, however, wish to enter into any engagement that would limit my labours to the humble task of a mere detail of facts, without linking them together in some theoretical system or systems of physical laws; because one of my objects is to trace to the same operations of nature, those facts, and those only, which are easily, and not otherwise, explained, by that code of laws which governs the display of one peculiar class of phenomena. And not to encumber any theoretical system with those phenomena to which they do not appear to belong: but to explain each class of phenomena by its own peculiar code of laws; or if you please, by its own peculiar theory. Hence it is that I shall be expected to be explicit on every point on which I touch, both experimental and theoretical, and either undertake to explain all those experimental facts which I may consider necessary to bring forward, or candidly acknowledge that they are inexplicable upon the theoretical principles which I advance.

It will here be *necessary to enter into certain conditions with my readers, respecting some of those theoretical points,* which to many philosophers, even of the present day, appears to be somewhat doubtful: though I believe the opinions of many others are favourable to those theoretical views by which I propose to be guided.

I wish to be understood then, before I proceed any farther, that, besides those recognised portions of matter which appear to be the principal part of the materials which constitute the earth and its atmosphere, such as the various kinds of solids and fluids which usually receive these general appellations, there are, at least, *three* others, whose reciprocal actions on each other, and whose peculiar operations on the former classes of bodies, are productive of the most surprising, and, in our present state of physical knowledge, the most interesting phenomena that nature has revealed to man. These are the *electric matter*;—the *magnetic matter*;—and the *calorific matter*; each of which I shall consider as a distinct element, possessing peculiarities of force and modes of action, and exhibiting phenomena which no other kind of matter has the power of displaying. They, however, operate on one another in a very remarkable manner, by their peculiar reciprocal excitations, and are thus productive of phenomena which have led some philosophers to the belief of their complete identity.

The fineness and subtilty of the *electric*, the *magnetic*, and the *calorific* particles, lead us to infer that they insinuate themselves into the pores of all other kinds of terrestrial matter; and their inactivity, when unmolested in these their natural habitations, is obviously a consequence of the equilibriums of their respective forces, when in an undisturbed state. So long, therefore, as these natural equilibriums remain unmolested, all of these material agents are perfectly inactive and exhibit no phenomenon whatever. Hence it is, that some exciting process becomes absolutely necessary before any of their respective phenomena can be produced. The processes of excitation which may be employed for bringing these agencies into a state of activity are exceedingly various, as I shall have occasion to show in many parts of these lectures; but for the present, it will be sufficient that I describe one simple mode only, of exciting each individual agent, by means of which, certain phenomena of each class may very easily be brought to pass.

If you take a stick of sealing wax, and, without any preparation, present it to any very light article, such as small feathers, bits of thin paper, &c. placed either on a table, book, or a dish, &c., you will not perceive any action whatever exercised by the wax on these light bodies. In this case you may easily imagine, that there is a complete electrical equilibrium in the body and on the surface of the sealing wax; and also in the light articles to which it was presented: and that it is in consequence of this equilibrium that the electric matter is perfectly inert, and will not act upon the light bodies which you had prepared. I wish it to be understood, however, that, although the electric forces of the wax had not a sufficient degree of intensity to cause a disturbance in the light bodies, it is still possible, that there might not be an absolute uniformity in the distribution of the electric matter, either on the wax or on the other bodies.

Now warm the stick of sealing-wax, taking care not to heat it too much; and then rub it on the sleeve of your coat.

By this simple process you have disturbed the previous electric equilibrium of the sealing-wax, and caused the electric forces to become sufficiently active to produce motion in the light bodies to which you now may present the stick. They will rise up and cling to the wax, often changing their positions on its surface, and sometimes they will be suddenly thrown off again to a neighbouring body, to which they will attach themselves for a short time, and again jump back again to the sealing-wax; again leave it, and again return; and so on for several times before the action ceases. These motions of the light bodies are electric phenomena; and may be repeated many times by renewing the activity of the electric forces, by again rubbing the dry and warm sealing-wax on the sleeve of your coat.

If you prefer a piece of dry woollen cloth to the sleeve of your coat, you may rub the sealing-wax with it with the same effect. Or you may use a piece of dry and warm flannel to rub your wax against; or the fur side of a hare-skin, or a rabbit-skin, which is, perhaps, better than any of the previously named substances. But whatever you may choose to rub the sealing-wax with, let me advise you to have it *warm* and *dry*, because much of your success in the experiment will depend on those conditions of both the *rubbing substance* and the sealing-wax.

It will now be proper to inform you that the motions which the light bodies make *towards* the sealing-wax are considered to be the effect of an electrical *attraction*, exerted between them and the wax; and their motions *from* the wax are considered to be due to an electrical attraction exerted between them and the body to which they fly, and for a while attach themselves. Besides the force of electrical *attraction*, there is also a force of electrical *repulsion*, to which I shall solicit your attention more particularly in due coarse as we proceed.

There are many other bodies which exhibit this class of electrical phenomena, by treating them in the manner I have described for sealing-wax. Such is the case with amber, sulphur, &c. If you use a glass tube for the exhibition of these electrical phenomena, it will also require to be warm and dry, not only on the outer surface, but on the inner surface also; and the rubbing substance ought to be soft silk. A piece of old black silk answers as well as any thing. The rubbing process, in all these cases, whatever may be the nature of the articles employed, is called *excitation*

When your sealing wax, or glass tube is well excited, and held at a short distance above the light bodies, the latter may be made to produce rapid motions to and fro, and dance on the table as if animated, by the active electric forces to which they are exposed. If you place your light bodies on a pewter or a silver plate, or on any metallic flat surface, their dancing motions will be more lively than when placed on any other kind of material: and if you touch the metallic plate with one of your fingers, the activity of the motions will be considerably improved.

I will now solicit your attention to a simple mode of producing *magnetic* phenomena, which I consider to emanate from the energies of an agent perfectly distinct from the electric. You must allow me to suppose that you are already acquainted with an instrument called the magnetic needle; it is sometimes called the compass needle; and when supported on a finely pointed pivot, so as to rest on a horizontal plane, one of its ends, in these latitudes, points towards the north, inclining a little towards the west of that point; and its other extremity, consequently, points a little to the east of the true south. In many other parts on the earth's surface, the direction in which the magnetic needle places itself when at rest, relatively to the geographical meridian, is very different to that in which it reposes in this country. But in every part of the world it is subject to certain influences which are capable of communicating to it peculiar motions, and placing it stedfastly in other positions than those which it assumes when no such local influences are present.

If, after the magnetic needle has come to rest, you were to turn it on its pivot with your finger, so as to point to some other quarter of the world, and then take your finger away from it, the needle would commence a series of movements which would terminate by its settling again in its former position; showing that, by the operation of some hidden force or agency, the needle had a greater tendency to repose in one direction than in another; which, in England, and in many other countries, is more near to the meridian than to a line placed east and west; or to a circle of latitude at that place. With respect to the cause of this peculiar tendency of the needle to place itself in a north and south direction, I can only say, in this place, that it is so completely under the control of the magnetic forces of the earth, that they alone are supposed to constrain it to assume that particular direction; but why the earth is magnetic, and why its magnetic forces should be so situated as to operate on the needle in that peculiar manner, are matters which philosophers have not yet determined. There are, however certain laws of magnetic action, which are well known, and which I will explain in a future lecture, my object at present, being that of showing the simplest, and most easily produced specimens of the three grand classes of phenomena which are so eminently conspicuous in nature, and so easily distinguished from each other.

Perhaps the simplest process for bringing the calorific matter from a state of inactive repose to a state of such activity as to produce ignition and fire, would be that of striking flint against hardened steel, and thus igniting detached particles of the metal, which in their turn, would ignite gunpowder, tinder, &c. In this case the calorific matter, which, previous to the collision of the flint and steel, was perfectly inactive, has, by the operation, become suddenly compressed into a smaller compass than that which it previously occupied, and becomes active fire in the condensed

Balance Sheet; or the present state of the Controversy.

MR. STURGEON,	MR. HARRIS.
Has shown that the experiments made before the Navy Board, at Plymouth, were *inconclusive*; and that the results were not due to any superiority of Mr. Harris's lightning conductors.—Page 163.	Has *not denied* the inconclusiveness of his experiments made before the Navy Board, at Plymouth, nor attempted to shew any other experiments that are more favourable to his system of conducters than to other systems.
Has shewn that there are three distinct kinds of *lateral discharge*, and has described the *first kind* and its mechanical effects. —P. 174.	Has *indirectly* acknowledged the *first* kind of *lateral discharge*, and also its mechanical action.—P. 317.
Has described the *second kind* of *lateral discharge* at page 174. He now refers the reader to the Athenæum for Sept. 30th, 1837. Page 71[illegible], Mr. Addams stated that "he had once seen upon the discharge of a large electrical battery, a wire splendidly illuminated by the lateral discharge, and exhibiting the coruscations spoken of by Professor Henry."	Denies this kind of lateral discharge in the present discussion, page 318 of this vol. but, told the British Association, at Liverpool, that "he had produced beautiful illuminating effects by discharging electricity along a wire enclosed in an exhausted glass receiver."—Athenæum for Sept. 30th, 1837.
Has described the *third* kind of *lateral* discharge, and shewn the cause of its production.—*P.* 174.	Acknowledges this kind of lateral discharge; but gives a different explanation of the cause.
Has contemplated the electrical condition of a lighting-rod, during *the time of its carrying a discharge*; and the effects consequent upon that electric condition.—P. 174, and 418, 419, of this vol.	Has contemplated the electrical condition of a lightning rod, *prior* to the discharge taking place, and consequently when no lightning was present, nor any conductor necessary.—P. 316, 317, 419, of this vol.
Has described, and shewn the effects of, *electric waves*.—P. 174, 180, 181, 182, of this vol.	Has not taken into consideration *electrical waves*.
Has described the electro-magnetic phenomena consequent upon a flash of lightning traversing the main mast conductor of H. M. S. Beagle; and has shewn that, as no magnetic effects were produced, the probability is, that the primitive flash of lightning *did not* strike the vessel; the observed effects being due to an electric wave.—P.179.	Insists on the Beagle being struck; but for want of due attention to the influence of electric waves; and the electro-magnetic influence of a primitive discharge of lightning, he has not taken their effects into consideration.
Has taken into *consideration* the *probable* effects of *oblique* flashes of lightning on the rigging of ships.	Has not noticed the probable effects of oblique flashes *of lightning on the rigging.*
Has disposed his system of conductors so as to prevent, as far as possible, the effects of oblique flashes, in the rigging.	Has disposed his system of conductors so as to afford *no protection* whatever to the *exterior* rigging, against oblique flashes.
Has disposed his system of conductors so as to distribute lightning into several branch conductors, and scatter its effects into comparatively harmless streams, almost the moment it arrives at the rigging.	Has disposed his system of conductors so as to give *no assistance* to each other above deck.
Has disposed his conductors so as to *prevent* the lightning entering the ship, by carrying it overboard on both sides.	Has disposed his system of conductors so as to lead the lightning into the body of the vessel.
Has disposed his system of branch conductors, so as to counteract each other's magnetic effects on steering compasses, chronometers, &c. placed near to the axis of the vessel; whether those instruments be placed above or below deck.	Has disposed his system of conductors so as *to give great facility to magnetic action* on compasses, chronometers, &c. placed either on deck or below.

The difference of the expense, time requisite for the equipment of a 50 gun frigate with the two systems of conductors, may be seen at page 190 of this volume.

for a long series of years, distinguished hyhimself in the fields of science. The phenomena of heat, at all times, display a fine field for the contemplation of the philosopher; and when arranged in that judicious manner, treated in that easy and lucid style, and discussed with that freedom and candour in which they appear in this work, they become familiar and interesting in every department of experimental science, and easily applicable to many of the common affairs of domestic life.

The work is printed on good paper, and with a bold clear type; and contains many illustrative wood engravings and a copper plate. It is got up in a neat style, and ought to find a place in every scientific library, both public and private: and we strongly recommend it to the perusal of families who wish to obtain information in these subjects on which it treats.

LXXV.—MISCELLANEOUS.—*Description of an Electrical Machine, made by Messrs. Watkins and Hill, 5, Charing Cross, London, expressly for the Royal Victoria Gallery for the Encouragement of Practical Science, Manchester.*

Figure 5, plate XI, is a perspective view of this splendid instrument, *f*, *f*, *f*, *f*, is a stout rectangular mahogany frame, supported by four pillars at its angles, as seen in the figure. From each of the two long parallel sides of this frame, rise two mahogany pillars, supporting a cross piece, which form a *vertical* rectangular frame on each side of the horizontal one. The front vertical frame is represented at *e*, *e*, *e*, *e*; and the rear one, which is principally hid from view, shows one of its pillars at *e*, on the right hand side of the picture; its other pillar and cross piece are represented by dotted lines. From each end of the horizontal frame *f*, *f*, *f*, *f*, rises a stout glass pillar g' g' and g' g': surmounted with a brass ball *b* and *b*. These glass pillars, and another shorter one at P, support the curved brass conductor *o*, *o*, *o*: and also four rubbers, two on each pillar, with their silken flaps *s s*, and *s*, *s*. The glass plate, represented by the large oval, is four feet in diameter: and revolves remarkably true, on a stout brass axle, supported in the middle of the cross pieces of the vertical frames *e*, *e*, *e*, *e*, and *e*, &c. The vertical brass rod *r*, screws into the ball *b*, and is intended to keep the upper silken flap from being displaced, by connecting them with silken cord in the manner shewn by the zig-zag lines. The lower silken flap is kept in its place by attaching it, in a similar manner, to its vicinal glass pillar *g g*. The prime conductor *c*, *c*, *c*, *c*, is a splendid appendage to the machine. It is of brass, beautifully polished and lacquered, and furnished with a stout solid glass stem *g*, *g*, which, by means of a brass dovetail is attached to the cross piece of the front vertical rectagular frame *e*, *e*, *e*, *e*. The collecting points proceed from two straight brass tubes terminated with ebony balls as seen in the figure. The massiveness of the frame work, the unusual size and elegance of the prime conductor, and the excellency of workmanship displayed in the whole, give to this machine a degree of magnificence, perhaps never before equalled in any piece of electrical apparatus. Its power corresponds with its magnitude and appearance, and, if possible, surpasses my anticipations at the time I gave directions for its structure. I have already passed through a series of twelve Lectures on electricity since the arrival of the machine at this Institution, and its performance has given both our Directors and myself the greatest satisfaction.

WILLIAM STURGEON.

Royal Victoria Gallery, for the Encouragement of Practical Science, Manchester.

METEOR SEEN AT SANDWICH, KENT.

Letter to the Editor on a Singular Meteor seen at Sandwich, Kent.

SIR,

About midnight on the 6th inst. a most extraordinary Meteor was seen by many persons in these parts, but I had not the good fortune to be one of the observers. Persons with whom I have conversed say, it was as large as the moon at full, the light intensely brilliant, even more so than the brightest day light, and that our gas lamps for a few seconds were reduced to insignificant specks of light. It passed to the N. W. I hope you will get some better account of this phenomenon.

W. H. WEEKES.

Sandwich, Feb. 18*th*, 1840

Note on Voltaic Batteries.

In my "*Experimental Researches in Galvanism, &c.*" published in 1830, I have stated that, if any method could be devised for keeping the transfer of the mercury to the copper, amalgamized zinc might be very advantageously employed in the structure of voltaic batteries. During the course of experiments which I was then pursuing, I discovered that, although the electrical powers of copper, and some other metals, became deteriorated by a partial coating of mercury, iron, on the contrary, had its electrical powers improved by such a coating of mercury, and formed with amalgamated zinc, a more powerful battery than copper with zinc.

About a year ago, I formed a battery of twelve iron gas tubes, each twelve inches high; and strips of amalgamated zinc, which performed remarkably well. I am now making a very extensive battery of similar materials for this Institution, which I shall describe in the next number of the Annals.

About a month ago, Mr. J. P. Joule purchased one of Grove's batteries of Watkins and Hill, and has been led to try sheet iron instead of the platinum. That gentleman informs me that the iron performs very well.

WILLIAM STURGEON.

Royal Victoria Gallery of Practical Science,
Manchester, March 10th, 1840.

END OF VOL. IV.

WHITE AND CARTER, PRINTERS, 20, ST. ANN'S-SQUARE, MANCHESTER.

state it is made to assume by the blow that is given to it by the flint and the steel. The blacksmith makes a nail red-hot; by giving a few smart blows with a hammer; and the Indian obtains fire by rubbing two blocks of wood against each other. These, and many other mechanical processes, are productive of fire, by calling into action the calorific matter which, previously, was so perfectly inert, as to be incapable of igniting the most inflammable matter. In some chemical compounds this latent calorific matter is so susceptible of activity by mechanical operations, that it requires extreme caution to prevent their ignition, even during the necessary processes of preparing them, and transferring them from one vessel to another.

In the course of these lectures I shall have occasion to show that an *active* portion of the electric matter, has the power of disturbing an inactive portion, and thus causing it to become active also. Active portions of the electric matter will also disturb other active portions of it, and become productive of very interesting phenomena. Active magnetic matter is also productive of its own class of phenomena, by the operation of its peculiar forces on other portions of matter of its own kind: such, also, is the case with the calorific matter; for one portion will disturb another portion, and thus become the exciting cause for the display of other calorific phenomena. Moreover, these distinct kinds of matter have the power of reciprocally operating on one another, in such a manner as to become the existing agents for the display of each others phenomena. Hence it is that we employ the terms *electro-magnetism*,—*magnetic-electricity*, —*thermo-eletcricity*, &c., the adjective in each expression implying the exciting agent, and the noun the character of the phenomena produced. I shall also have to employ the terms *galvanic-electricity* and *voltaic-electricity*, all of which terms I shall endeavour to explain in their proper places as I proceed.

ANNALS OF ELECTRICITY &c. Vol V Pl. IV.

Fig. 2

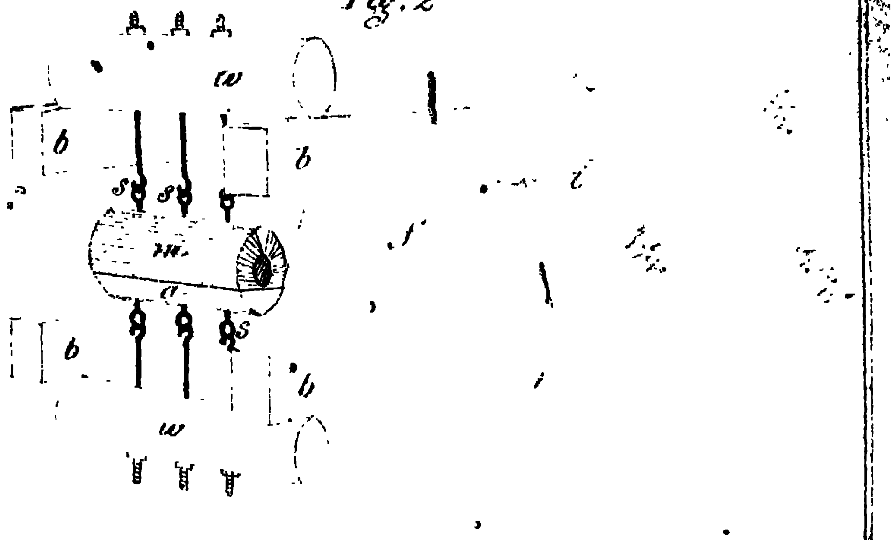
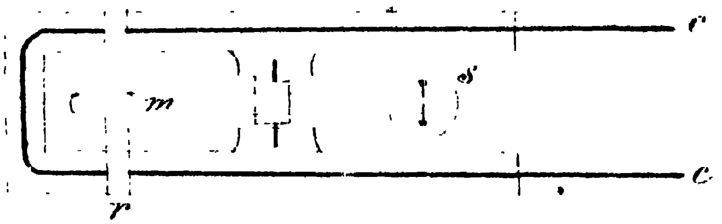


Fig 1.



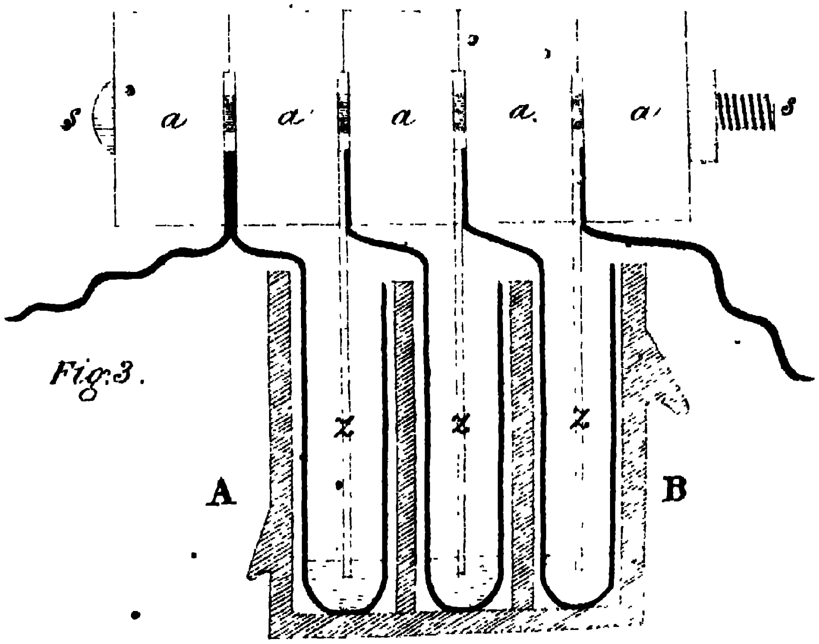


Fig. 3.

Printed from

AN ELECTROTYPE,

Prepared for & Presented to

Wm STURGEON ESQre

by Sam Cartwright

Preston Augt 1840

Printed from

AN ELECTROTYPE,

Prepared for & Presented to

Wm STURGEON ESQre

by Sam Cartwright

THE ANNALS
OF
ELECTRICITY, MAGNETISM,
AND CHEMISTRY;
AND
Guardian of Experimental Science.

MARCH, 1840.

Considerations on Chemical Forces; By M. GAY LUSSAC.— FIRST MEMOIR *on Cohesion.**

I purpose presenting, successively, in several memoirs, some reflections on affinities; this subject appears to me one of great interest, but it is very difficult, and in entering upon it I should wish to reckon upon the indulgence and favorable concurrence of chemists.

In the year 1718, a time when chemistry was yet obscure, Geoffroy, the elder, endeavoured to classify bodies according to the chemical relationship observed between them. He established the proposition, that, *whenever two substances which have a disposition to join together, are found in connexion, if a third which has a greater inclination for one of them, approach, it will unite to that, and cause it to abandon the other.*

For the support of this proposition, Geoffroy made a very simple table of the relationship between the different substances, then known. It is printed in the *Memoirs de l'Academi royale des Sciences,* for the year 1788, page 202; but I thought it would be interesting to reproduce it here, as a historical monument, such as Geoffroy has given, replacing the chemical symbol of each substance by its proper name.

---

* From the Compte Rendus; translated by J. H. Lang, Esq.

Ardent Spirits.	Acid of Marine Salt.	Nitrous Acid.	Vitriolic Acid.	Absorbing Earths.	Salt fixed Alkali.	Salt Volatile Alkali.	Metalic Substances	Mineral Sulphur.	Mercury	Lead.	Copper.	Silver.	Iron.	Antim.	Water.
Salt fixed Alkali.	Tin.	Iron.	Oily principle or primitive Sulphur.	Vitriolic Acid.	Vitriolic Acid.	Vitriolic Acid.	Acid of Marine Salt.	Salt fixed Alkali.	Gold.	Silver.	Mercury	Lead.	Antim.	Iron.	Spirits of Wine and Ardent Spirits.
Salt volatile Alkali.	Antim.	Copper,	Salt fixed Alkali.	Nitrous Acid.	Nitrous Acid.	Nitrous Acid.	Vitriolic Acid.	Iron.	Silver.	Copper.	Calamin Stone.	Copper.	Silver, Copper, Lead.	Silver, Copper, Lead.	Salts.
Absorbing Earths.	Copper.	Lead.	Salt volatile Alkali.	Acid of Marine Salt.	Acid of Marine Salt.	Acid of Marine Salt.	Nitrous Acid.	Copper.	Lead.						
Metallic Substances.	Silver.	Mercury	Absorbing Earths.		Spirit of Vinegar.		Spirit of Vinegar.	Lead.	Copper.						
	Mercury	Silver.	Iron.		Mineral Sulphur.			Silver.	Zinc.						
			Copper.					Antim.	Antim.						
			Silver.					Mercury							
	Gold.							Gold.							

The substance, at the head of each column, is compared with those beneath in a decreasing order of affinity. Thus, in the first column, ardent spirits (or acids) have a greater affinity for fixed than for volatile alkali salt, absorbing earths, and metallic substances. In the fourth column, it is the oily principle, or primitive which has the greatest affinity for the sulphuric acid: afterwards come the fixed, and volatile alkali salts, alkali salt, absorbing earths, iron, copper, and silver.

Examining the different relations expressed in each column of the table, we perceive that Geoffroy, has confounded the effects of affinity, which ought to have been separated from one another, and has compared some things which are not comparable. Thus the decomposition of sulphuric acid by the pretended primitive sulphur, iron, copper, and silver, cannot be assimilated to the affinity of this acid for their bases. But this is not surprising, as in the time of Bergman, half a century later, the same confusion still existed. Geoffroy, had not accompanied his table with any explanation, he was limited in making the application of it to the preparation of corrosive sublimate by several processes, and he has done it in a tolerably successful manner. Geoffroy's table, notwithstanding its imperfections, is a fine conception; it is also the first progress made in philosophical chemistry.

It appears that for some time but little importance was attached to Geoffroy's table of relations. Subjected to several disturbing causes which often made them vary, they were considered as vague, indeterminate, and entirely dependent upon circumstances.

But Bergman, thinking that all the operations of chemistry, synthesis or analysis, were founded upon attractions which were not understood, because they were subjected to certain conditions, which incited, stopped, or disturbed them, at last drew the attention and interest of chemists to the causes of chemical phenomena, and his dissertation, *De affinitatibus electivis*, published in 1775, also fixed a remarkable epoch in the history of the sciences.

Bergman distinguished in a body the attraction of similar molecules, which he designated by the name of *attraction of aggregation;* and the attraction of heterogeneous molecules, which he calls *attraction of composition;* when this acts so that one substance displaces another in a compound, it then takes the name of *simple elective attraction;* and if it act between two compounds, whose elements may be reciprocally changed, it takes that of *double elective attraction.*

Notwithstanding the opinion which some chemists had of the inconstancy of affinities, Bergman appeared to consider them as absolute, determined forces, but whose effects could be modified by certain causes, the influence of which he often appreciated in an ingenious, though sometimes very incomplete manner.

The first of these causes he found in the difference of volatility of substances presented in the same sphere of action. Bergman conceived that the difference in the affinity of two substances for a third, at a given temperature, might be more than compensated at a higher temperature, by a difference of volatility in favour of the substance which had less affinity than the other, but more fixity.

Before Bergman, the results of the affinity between three substances were confounded with those in which there were four, that is, the products of the simple elective affinities with those of the double affinities; and as they are really very different, an objection was raised, from this circumstance being badly comprehended, to the theory of affinities. Thus, from the table of Geoffroy, fixed alkalies have a greater affinity than lime for acids, since in fact they separate it from the gypsum. However, it is said, that if we dissolve chalk in aquafortis, and add a solution of vitriolated tartar, the gypsum regenerates immediately; a proof that the calcarious matter here manifests a greater power. Bergman remarks with propriety, that the two circumstances are very different, since in one there are only three, while in the other, there are four substances present. He explains the reproduction of the gypsum in the mixture of nitrate of chalk with sulphate of potassa, from the double elective affinities, conceiving the sum of the two active affinities to be greater than that of the quiescent. The explanation is certainly ingenious, but at present it is not enough.

The effects of affinity may, according to Bergman, be further disguised, and the theory blamed, by unexpected alterations in the substances present:—for example, nitric acid separates marine acid from its alkaline base, a fact which has been known for some time; but Mayraf has discovered that marine acid can, in its turn, displace nitric acid in saltpetre. While ignorant of the true nature of marine acid, says Bergman, this reciprocal displacement of one acid by another has escaped all explanation; but now we know that marine acid contains some phlogistic, all difficulty vanishes. Nitric acid displaces muriatic by simple affinity; the latter yields its phlogistic to the nitric acid, whether it be free or combined with a base, and hence their reciprocal displacement becomes the consequence of this alteration. It is also thus that white arsenic (arsenious acid) decomposed by distillation salts formed by nitric, but not those formed by marine acid, because they also contained a certain quantity of phlogistic.

Bergman equally well explains the anomalies of decomposition due to solubility. It happens, says this illustrious chemist, that at first no trace of decomposition appears, though it has really taken place; thus vegetable, displaces mineral alkali from its combination with acids, although we perceive no conglomeration, no precipitate; this circumstance has caused some celebrated chemists to conclude that vegetable, has no power over mineral, alkali. But let us suppose that a little of this latter has been eliminated, is it separated?—certainly not; it remains in solution: for if we evaporate it we shall obtain *crystallized* mineral alkali, with which we could produce Glauber's salts or quadrangular nitre.

I shall here conclude these quotations. They are sufficient to shew that Bergman has deeply studied the theory of affinities, and enriched it with numerous and useful observations. What he says of simple elective affinities is in general exact. The imperfections that we remark in it, extend even to the state of the science, still uncertain and often obscure in its march; and perhaps the *Chemical Statics* has made us too quickly forget the real services Bergman has rendered to philosophical chemistry.

In what concerns double elective affinities, with the equilibrium of active and quiescent forces, Bergman has certainly shewn great clearness: his explanations are seducing; but he has not understood the correct explanation of the precipitates, obtained by the concourse of double affinities.

Bergman, in imitation of Geoffroy, has explained nothing concerning the measure of affinities, and he was right. This question is even now a delicate one, and not at all easy of access. He has confined himself to arranging the bodies by their greater or less affinity.

Such were the ideas of Bergman on affinities, which were prevalent 'till Berthollet published his researches on affinity, and his *Chemical Statics*, but, they were then eclipsed by the great eclat thrown on these two productions.

Berthollet, in the study of affinities, was engaged with two principal ideas: the influence of the force of cohesion in chemical phenomena, and the proportion of affinities, which he thought to find in the mass of bodies that enter into combination.

According to this illustrious chemist, cohesion or reciprocal attraction of similar molecules is a powerful force which can balance the affinity of heterogeneous molecules, and determine combinations and decompositions.

It does not exist at the time it shows its effects only, but even a long time before it becomes effective. He shews from this analogy, that as soon as a liquid becomes gaseous, and a gas liquid, the dilation of the former, already influenced by the gaseous state it is about to take, and the contraction of the second, influenced by the liquid or solid state it is taking, follow a progression more rapid than the greatest distance of this term. But this argument of Bergman for the establishment of the influence of cohesion, some time before its effects are manifested, remains without foundation, when we consider that there is no unique, constant point for the change of a liquid into an elastic fluid, and reciprocally; but on the contrary, this change is incessant at all temperatures and under all pressures.

Whatever opinion others may form from Berthollet's demonstration, it is sufficient for me to state that he adopts the pre-existing influence of cohesion, and that he has made it enter into all the precipitations and chemical solutions. The affinity,' says he, which may produce the solid state, ought to be considered as a force which not only acts when the solidity appears, but even before that time; so that every time he produces a solid substance, whether by separation or combination, we must look in the reciprocal action of the parts which acquire solidity for the cause itself which produced it, although it may not be manifested before.

The theory of decompositions has received from Berthollet unexpected improvements. We are indebted to him for the principle that the change of acids and bases between two salts takes place, every time the salts proceeding from the change or only one of them, have less solubility than the given salts. This principle is of a fortunate fertility, and, we may say, constitutes one of the finest acquisitions of chemistry. But Berthollet taking cohesion for the first cause of double decomposition, does not appear to me to have given the true demonstration of it. He supposes it is the cohesion of the salts not yet existing, which nevertheless determines their formation, and this supposition is inadmissible. For if we can agree with him that the cohesion begins to act in the solution of a salt before the time of crystallization, there is no more of it even when the salts do not exist any longer, as in the case of the mixture of two saline solutions.

Bergman supposed that affinity was an absolute force, admitting no division in its effects; and had only established among bodies a relative affinity. Berthollet, on the contrary believed that affinity was not used in an absolute manner, without division; that thus a base in the presence of two acids did not exclusively combine with the most powerful, as Bergmen thought; but that it divided itself between the two in proportion to their affinity and quantity. Hence the principle of Berthollet, *that the affinity of different acids for the same alkaline base, is in the inverse proportion to the ponderable quantity of each of them, which is necessary for the neutralisation of an equal quantity of the same alkaline base.* At present, and, I may say, for some time past, this measure of affinity has been abandoned; At the time Berthollet wrote his *Statique chimique*, the atomistic theory was but little understood, and some years later, Berthollet would certainly not have proposed a method of measuring affinity that gives nothing else but the atomic weights or equivalents, which we know to be independent of chemical attractions, or at least to have but very distant connexions with them. I hope hereafter to be able to return to this subject, as well as to the division of one substance between two antagonists. For the present I shall confine my observations to the force of cohesion, since it is made to take so large a share in most chemical phenomena, and as it is of the utmost importance to the better appreciating its real influence.

The attraction of hetreogeneous molecules has been rightly distinguished after Bergman, from that of homologous or similar molecules, which has also been designated by the name of *aggregation*, and since Berthollet, by cohesion. These two forces have, without doubt, the same origin, but not appearing to have any common tie in different bodies, their effects could not be confounded.

Cohesion itself takes different names from the lights in which we consider it. It is called *tenacity*, when weight or force is opposed to it for determining the rupture of a body. It is called *hardness*, when taken for the resistance one body offers to another with which we wish to cut it. *Tenacity, and hardness* are evidently *cohesion* itself; or at least they both essentially depend upon it. Bodies which have the most tenacity are also generally those which have the greatest hardness or, according to our notions, the most cohesion. Nevertheless this ought only to be understood of uncrystallized bodies, because, for crystallized bodies, there exists an easy cleavage, and we can very well conceive that there may exist notable differences between *hardness and tenacity*, according to the direction of the rupture and separation of the particles.

Comparing among themselves the three states which the same body can take, we have been led to make each of these states depend on the relation of the peculiar cohesion of the molecules of this body to their repulsion. It is very certain that in solids the cohesion is the greatest; in liquids it is much less, but it is always something, since there is no liquid which does not take the globular form, and that a drop suspended from a solid, may be divided into two parts, of which the lower adheres to the higher, notwithstanding the weight which inclines it to fall.

The word *cohesion*, in a chemical point of view, is taken under another acceptation. Here the action is complex; the body to be dissolved and the solvent are both present, and each acts on the other. The resistance the one offers to the other is called *insolubility*, which must never be taken but in a relative sense. This resistance, let us now say, insolubility, depends essentially, according to the established belief, both on the cohesion or reciprocal attraction of the similar molecules of the body to be dissolved, and its affinity for the solvent we present to it. So that it is supposed, if the body, instead of being solid, were liquid, the solvent would take a much more considerable quantity of it.

This, if I mistake not, is the opinion commonly formed of chemical cohesion and solution. Not being able to divide it nicely, and proposing to discuss it, I thought I ought here to introduce these details, which their shortness will without doubt excuse. The progress of science brings daily new modifications into our ideas, and it is very necessary to fix the starting point of a discussion, if we wish it to be clear and useful.

But before treating of cohesion with regard to its influence in chemical phenomena, I shall allow myself to turn my attention to a physical operation which also appears connected with cohesion, and appears to me very proper to throw light on the method of influence of this force, I speak of volatilization.

I suppose a volatile body, able to present itself under the solid and liquid forms within the limit of temperature accessible to observation; water for instance. If the elastic force of its steam be determined, starting from 20 below zero, at which it is solid and possesses a great cohesion, we find that the progression of this elastic force, is no way affected by the passage from the solid to the liquid, or, reciprocally, from that part of the liquid to the solid state, that is to say, that the elastic force of ice at zero is precisely the same as that of water, at the same temperature; a similar observation for every other degree of the thermometer at which we can find water at the same time in the solid and liquid state, the elastic force of steam will remain the same for both; and, however, without exactly deciding the degree of cohesion of ice, in comparison with that of water, we may admit that it is incomparably greater, perhaps more than a thousand times.

This observation which struck me some time ago I have verified on hydrocyanic acid, which we know solidifies at about 15 below zero and still preserves a very great volatility. The progression of the elastic force of its steam, has been no way affected at the time of the change of state; and this result may be considered as general.

Hence there is no relationship between the cohesion or attraction of the molecules of a body and their repulsive force; the one is consequently quite independent of the other and the elastic force of steam is only determind by the number of molecules able to maintain themselves in a gaseous state, in a limited space at a given temperature.

However when we consider that salt water produces a steam whose tension is less than that of pure water, at the same temperatures,* a result which can only be explained by an affinity of the aqueous for the saline molecules, we may ask, in assimilating this affinity to that of water for its own molecules, if the space above a surface of water is really saturated with steam, that is to say, if the equilibrium established, the least cooling of the steam taken from the action of the water, the least reduction of space would not occasion the precipitation of a certain quantity of steam; or whether, for the same space above salt water, the saturation is not complete, so that the steam taken from the action of the liquid might be cooled or reduced in volume within certain limits, without the least precipitation of its molecules. I am disposed to believe that the space above pure water becomes completely saturated with steam, from the consideration that the difference of the attraction of the molecules of ice among themselves, to that of the molecules of water, avails nothing in the elastic force of the steam of each of these bodies, taken at the same temperature. Nevertheless, the experiment does not appear to me less interesting to try, and although very delicate, I propose to prepare for the execution of it.

The observation that the elastic force of a body remains constant at the instant of the change between the liquidity and solidity, doubtless clashes with the received ideas relative to the molecular constitution of each of these states; but it would not oppose them less even if we derived from it the consequent, that molecular attraction is the same for a liquid as for a solid at the instant of the change of state; for this is accompanied with variations as much in the volume of the body as in its quantity of caloric, which appears to announce a great alteration in its molecular constitution. And whether the molecules in taking the solid state, are only caused to approach; whethêr they are placed together otherwise; or finally whether they unite in small geometrical groups which, by their arrangement, would modify the volume of the body; results all of which, depend necessarily on another mode of action in the molecular forces; at least it is certain from our scientific analogies, that they are then in very different conditions from what they were before the change, and it is still very remarkable that their elastic force is indifferent to all these pertubations.

---

* It has been pretended that the stream which comes from an aqueous saline solution, boiling later than water (at 110° for instance) was always at 100°. This is a very great error; steam has always the same temperature as the last liquid bed it traverses; but what has caused the deception is, that steam, as also every other elastic fluid, cools very rapidly until the time of their condensation, when the cooling is more powerfully compensated by the liberation of their latent caloric..

These preliminaries established (and I consider them of great importance from their connexion with the principal question which I have started) I shall turn my attention to the effects of cohesion, and follow them up more particularly in solutions.

We will look for some bodies uniting the double condition of being soluble in a solvent, and of being able to appear solids and liquids within accessible limits of temperature for the determination of their solubility.

Among salts I do not know of any which combine these two conditions.

Among acids, I thought camphoric acid, of which we find a table of solubility in Berzelius, from Brandes, would furnish me with an example of solubility under the desired conditions; and in fact this acid whose fusibility is given at 63°, appeared to show a solubility above and below this point, which was subject to a law of regular continuity. But wishing to repeat these experiments of Brandes with some camphoric acid, such as is obtained from M. Leibey, I perceived that this acid would not fuse even at 300°, and consequently I abandoned it.

Among inflammable bodies, cetine, paraffine, fat solid acids, present no anomaly in their solubility in alcohol while passing from the solid to the liquid state; the progression in proportion as the temperature increases is perfectly continuous and regular. I shall give by and by these different solubilities, regretting, much, that I have not among the salts more conclusive examples.

But the cohesion of these different bodies while they are solid being greater than when they are liquid, and their solubility not being disturbed at the instant of changing from one state to another, neither before nor after it is absolutely necessary that it be independent of the cohesion.

Further, if I take the solubility of an oil in alcohol I find that it acts in general precisely like that of a solid, although liquid, that is to say without great cohesion; the solubility, very feeble at a low temperature goes on increasing progressively with it. Thus a body whether it remains constantly liquid, or whether at first solid it afterward becomes liquid, presents under each of these circumstances the same kind of solubility.

Gaseous substances themselves such as chlorine, do not seem to me to undergo any alteration in the progression of their solubility at the moment of their change of state.

Finally, if the cohesion of a salt had a great influence over its solution, the solvent would never be completely saturated by simple contact with it, and the solution separated from the salt, might be reduced in temperature a certain number of degrees without giving up the salt. But it is not so, setting aside the accidental circumstance of inertia of the molecules, the solution gives up salt immediately it becomes the least cooled.

Hence I am inclined to think cohesion has nothing to do in general with solution. As the elasticity of vapours, so, the solution of a body, varies with the temperature; it is doubtless, also connected with the reciprocal affinity of the solvent and the body dissolved; but the effects of affinity not being variable with the temperature, while those of solution depend essentially upon it, it would be difficult not to admit that in solution as in evaporation, the product is essentially limited to each degree of temperature, by the number of molecules able to exist in a given portion of the solution; they separate themselves for the same reason as the elastic molecules are precipitated by a decrease of temperature; and probably also, like these latter, by the compression and reduction of volume of the solvent.

Thus when the temperature decreases in a solvent saturated with a body, the molecules in excess with regard to the new temperature will be precipitated, not by virtue of the cohesion, which we suppose ought to incline them to separate and aggregate, but because they can no longer be maintained in the solvent as takes place for a vapour saturated space which has just been cooled, hence it would be of but little matter whether the molecules repulsed from the midst of a solvent, once separated, took the solid, or liquid; or even the elastic form.

Whenever solution is essentially connected with vaporisation in this manner, that both are dependent upon the temperature and obedient to its variations. Hence they ought both to afford, if not a complete identity of effects at least a great analogy: their essential difference consists in the gaseous molecules not having need of a solvent to keep them in a given space their repulsive force being sufficient for this purpose. On the contrary in the solution of a solid or liquid body, the molecules could not keep themselves in the space if they were connected by affinity with the molecules of the solvent. This condition fulfilled, the solution following its particular course, yielding to temperature as every vapour has also one particular to itself.

Hence the analogy which solution and vaporisation have holds to their complete submission to temperature; and as the variates it appears to me incontestable, that the elastic force of the vapour of a body is quite independent of the state of this body or of the cohesion of its molecules since it remains constant when the latter varies, I shall still be disposed to admit from these analogies that solution is regularly independent of cohesion.

However, if there exist analogy between vaporisation and solution, we may ask, why while the elastic force of vapours follows a regular ascending law, the solubility of some salts such as sulphate, seleniate of soda, presents all at once a point of repulsion and a decreasing course.

I shall remark first that the difficulty remains the same whether there be an analogy between vaporisation and solution or not, and thus it cannot constitute a serious objection; in the second place the retrograding point in the solution of some bodies, may be easily explained by the consideraton that at this point it is no longer the same body which continues to be dissolved. Thus for chlorine from 0° to about 8° a space of temperature during which it is in a hydrate state, the solubilty is ascending but at this latter point, the hydrate is overcome and immediately, as the solubility follows a decreasing progression as far as 100, at which it is almost nothing. This is very evidently hydrate of chlorine which is dissolved from 0° to 8° above that of chlorine only. Finally for sulphate of soda, the decrease of the solubility in proportion as the temperature increases above 33° may be attributed to a diminution of affinity. I shall return to the solubility of this salt.

As there is some interest to know whether a salt susceptible of forming a hydrate, dissolves in water, hydrated or anhydrated, I shall mention a fact which seems to me necessary to remove the uncertainty: it is that, whenever an anhydrous salt, or any other body not having the property of forming a hydrate, is dissolved in water, there is constantly a production of cold; and that, on the contrary, when the salt can form a hydrate, there is a production of heat. When the hydrate is complete, before the solution in water the case is the same as when the salt cannot be hydrated. We may perceive that it might sometimes happen that the heat produced by the hydration was less than the cold produced by the change of state, but I have not yet perceived any exception. The fact that I have just particularised will also establish a fresh analogy between solution and vaporisation, relatively to the heat rendered latent in the change of state.

In comparing solution with combination, we may assign a remarkable difference between them, viz. that solution varies at every instant with the temperature while combination is not similarly obedient to these variations.

If my observations be correct, they will greatly weaken the influence which Berthollet has attributed to cohesion in all chemical phenomena; but I feel too much the weight of this illustrious authority not to be, in defiance of my own arguments, and not to be staggered in my new convictions. It is with this sincere feeling of doubt, that I shall indicate some applications of the light in which I consider cohesion.

Berthollet has often repeated that when one body precipitates another from it, it is not always an indication of a superiority of affinity; that it is the cohesion which takes the precipitated which determined the decomposition.

From the principles which I have established, cohesion on the contrary, has only a secondary place in the precipitation, as in the solution: the precipitation is a constant proof of a greater affinity; cohesion only shows it by rendering its effects sensible.

With regard to decompositions by double affinity, our explanations are equally divergent. If we submit a solution of sulphate of soda with one of nitrate of lime it makes a precipitate of sulphate of lime, and nitrate of soda remains in solution.

Bergman explains this result by saying that the sum of the *active* affinities which are in motion carries it over that of the *quiescent* affinities.

According to Berthollet there is a double decomposition, because the sulphate of lime is the most coherent of the four salts which may be conceived from the mixture in the solution, previously to all precipitation. Berthollet conceived that although the sulphate of lime does not exist, still the cohesion which it must take determines the formation of it as well as the separation.

This explanation I believe has never appeared satisfactory. As long as the sulphate of lime is expected not to exist in the solution, the cohesion that it should take cannot be cited to explain its formation and precipitation; and for the same reasons we can no more invoke the insolubility: it does not determine the change as a first cause, it only renders it sensible and effective, when it has been used in determining the separation of its products. What then is really the cause which presides over the decompositions by double affinity?

If we turn our attention to the precipitates resulting from the action of the double affinites, we find that those are not the most stable precipitates which contain acids and the most powerful bases. Thus sulphate of potassa although formed of elements endowed with a powerful affinity, is transformed in its mixture with acetate of lime, into sulphate of lime, the base of which has a much less affinity for the sulphuric acid, than the potassa. In the mixture of sulphate of lime with carbonate of ammonia, the lime is precipitated with the carbonic acid in a much less stable combination than it formed at first. It would be easy to give many similar examples.

Hence it will not be correct to say, that after the mixture of two saline solutions, the strongest acid always combines with the strongest base; it would appear on the contrary, that the salts in a state of neutralization, may change acids and bases independently of their reciprocal affinities.

Judging only by the results of experiment, the change is manifested by the precipitation of a new insoluble salt alone, whose formation, according to Berthollet, would even be the cause of the change. But as the reasons he has given for it are not satisfactory, we may ask, if the cohesion of a salt not yet existing, or its insolubility, which does not even carry the idea of cohesion, can exercise their action before the formation of this salt and be the real cause of it.; or rather even, if not being able to determine this formation they only exercise their influence afterwards, causing the separation of one of the new salts produced at the moment of the mixture.

To myself, after the observations I have presented on the slight influence of cohesion in solutions and chemical precipitations, the question does not appear doubtful.

I shall recall to mind first that the solubility of a solid body in a solvent, is no way affected by the difference of molecular attraction between the solid and liquid state, that consequently the change cannot be affected any longer.

But to these considerations we may add others which appear to me of great weight.

The change between the acids and bases of two salts may take place, according to Berthollet, in several ways. Besides the insolubility which most usually determines it, a difference of fusibility, density, and volatility, may also, very well produce it. But in the case, for example, of a difference of volatility, we can no longer invoke the reciprocal affinity of the molecules as for a solid, or even for a liquid, since, on the contrary, the molecules of the salt which is separated are in a state of repulsion, and that we may also demonstrate, as in the case of insolubility, as in that of volatility, it is always the most volatile salt that is formed.

Thus the change taking place, according to the received opinion, under very different circumstances of solubility, density, fusibility, and volatility, one of them cannot be the true cause of the change to the exclusion of the others, and consequently this cause ought to be considered otherwise, independent of these different circumstances.

Since the change is not determined by the reciprocal affinity of the acids and bases, since also it is no longer by the secondary causes we have just enumerated, and as however these latter cause separations, it necessarily follows that the change precedes them, and we can only be satisfied with regard to these different causes of separation, by admitting that at the moment of the mixture, before any separation, there is a complete confusion between the acids and bases, that is to say, the acids combine indifferently with the bases and reciprocally; the order of combination is of little importance, provided the acidity and alkalinity are satisfactory, and they are evidently so, whatever equilibrium may be established between the acids and bases.

This principle of indifference of equilibrium (*equipollence*) being established, the decompositions produced by double affinity are explained with very great simplicity. At the moment of the mixture of two salts, two new ones are formed, bearing some relationship to the two former, and allowing one of these properties, insolubility, density, fusibility, volatility, &c. to be stronger in the new than in the given salts, there will be a disturbance of the equilibrium, and separation of one salt, sometimes even of several.

Still it is essential to consider that, although we admit a confusion at the time of the mixture of two or more saline solutions, it may not always rigorously take place. We know in fact, that the molecules of a compound oppose a sort of inertia to the change, and that time or disturbance is often necessary to cause this change.* Many saline solutions, and particularly that of sulphate of soda, keep themselves sur-saturated at very inferior temperatures to that at which they ought to begin giving up the salt. A solution of sulphate of magnesia mixed with a solution of oxalate of ammonia, when left undisturbed, gives no precipitate of oxalate of magnesia for a long time after the mixture, whereas it is produced in a few seconds by means of a rapid agitation. Besides this circumstance of the inertia of molecules, which is opposed to the change, we may admit, in the case of a complete reciprocal saturation, such a state of indifference, or if we prefer it, instability between the acids and bases, that the slightest circumstance, even a very feeble cohesion, might disturb the equilibrium and determine the change.

Even admitting that the confusion has taken place, we might yet conceive that the separation of the newly formed salts, would not be instantly effected, and for this reason we see that water remains liquid several degrees below zero. Hence it is possible to conceive that the reciprocal action of the molecules which separate themselves from the solvent, determines and accelerates the phenomenon. But this reciprocal action of the molecules to reunite into a liquid or solid mass, I always consider as only occupying a secondary place in chemical phenomena.

It is easy to demonstrate the change between the elements of two salts, although it may not be accompanied by the formation of a precipitate. Let us imagine a solution of sulphate of protoxide of iron, and pass through the mixture a current of sulphurated hydrogen: there will immediately be made a precipitate of sulphuret of iron, which makes us suppose that it was formed previously from the acetate of iron. I know that in the real case, we may object to this change having taken place between the strongest acid, sulphuric, joined to the strongest base which is here the soda; but the objection will not appear founded if we recollect that the reciprocal affinity of the acids and bases appears quite foreign to the formation of precipitates formed by the concourse of double affinities.—Every other base besides soda, the weakest we can choose among those which are not precipitated by the sulphurated hydrogen, would produce a similar effect. Thus acetate of alumine mixed with sulphate of iron, determines its decomposition by sulphurated hydrogen.

The principle of chemical equilibriums (*equipollences*) which has just been admitted with regard to saline substances, appears to me to extend to all analogous compounds, that is to say to all those, in which the sum of the neutralizations, after the mixture will be the same as before, as for example, for water and a chloride.

Here is a very remarkable fact. It would appear that, in the reciprocal combination of two acids with two bases, there is expended a certain quantity of action, whether chemical or electrical, which remains constant in the change.

I had wished to say a few words on solution: but I find myself prevented by the difficulty of the subject, which is much greater than it appeared at first sight. I shall confine myself to remarking, that the word solution is applied under very dissimilar circumstances, which ought, however, to be carefully distinguished. In a solution properly so called, such as a salt in water, there is no decomposition between the solvent and the body dissolved; the effect varies in general with the temperature. On the contrary, in a solution by an acid or alkaline solvent, there is generally decomposition, formation of new products, and the effect no longer varies with the temperature as in the other solution. Hence we must determine in each particular case, whether it is simply solution, whether it is the consequence of the formation of new products, or if in that these two circumstances cannot be joined together. But to arrive at this determination some data, which will form the subject of another memoir, are still wanting.

I terminate this first work without having nearly exhausted the matter it embraces, but as I said at the beginning, the matter is difficult, and I had only promised making a few observations. Perhaps they will have more interest in being strengthened by these I have yet to present. In the mean time I leave that to the criticism of chemists, and shall consider myself happy if, at least as *conjectures*, they attract their attention.

XLV. OPTICS.—*Note on irradiation*; *by* M. J. PLATEAU.*

At the session of the 6th of May last, M. Arago much wished to entertain the Academy with my memoir on irradiation, and presented at the same time some observations on the theoretic part of this work. M. Arago thought that we could not preserve the physiological explanation which I have endeavoured to confirm, and advances a new theory from which the irradiation would be the result of the chromatic aberration of the eye. The considerations mentioned by M. Arago, not having been printed, I have been prevented from becoming perfectly acquainted with them, and am not aware of their tending to refute the arguments I have brought forward in favor of the physiological theory. I shall not here recall these circumstances, but shall content myself with examining the new hypothesis presented by M. Arago.

---

* From the Compte Rendus. Translated by J. H. Lang, Esq.

It is true, the eye is not, at present, recognised as a perfectly achromatic instrument, and it necessarily follows from this non-achromatism, that the images of objects are surrounded, on the retina, by a small band of aberration, which ought to increase a little the apparent dimensions of luminous objects projected on an obscure ground, and diminish those obscure objects which are projected on a luminous ground. But whether this effect can be sensible under ordinary circumstances, and whether the small band of aberration has sufficient breadth for us to distinguish it, and to attribute to it the phenemenon known as irradiation, is the question I hope to solve.

I shall first remark, that by virtue of the same cause which produces it, thes mall band, which the chromatic aberration of the eye draws around images, cannot be exempt from colors. Consequently, if this irradiation, manifested by a white object on a black ground, was due to this cause, it appears that the object would appear colored on the edges.—But among all the observers who have engaged themselves with occular irradiation, not one has made the least mention of colored appearances, and in the numerous experiments that I have made on irradiation under a great number of different circumstances, I have never seen any thing similar.—This absence of visible colors, might, with difficulty, be attributed to the small angular width of the irradiation; the persons among whom the phenomenon has much developement, will be easily convinced, by repeating some of my experiments, or by observing the well known appearance of the current, that the band of irradiation is of a width quite sufficient to allow its color to be seen if it had any.

In the second place, I do not see how it would be possible to explain by the aberration of refrangibility, this singular law to which irradiation is subject, viz.:—that when two objects of equal brilliancy are only separated by a small interval, each of them diminishes the irradiation of the other in the parts in view, and that, in proportion as the two objects approach one another, so that at last when they touch, the irradiation is nothing for each of them at the point of contact; or how to admit an action exercised by a luminous image on the aberration produced about another image.

But we may easily decide by direct experiments, whether occular irradiation is due to chromatic observation or not. It is sufficient, in fact, to try if the irradiation be also produced when the object is bright, by a homogeneous light. If in this case, we no longer perceive the irradiation, we may admit as true, the hypothesis which attributes this phenomenon to the chromatic aberration of the eye, but if, on the contrary, the irradiation still appears, and to the same degree as with a compound light, equal in brilliancy to the homogeneous light employed, it will be impossible to discover the cause of the phenomenon in the aberration with which it acts. The following experiments have been executed for the purpose of deciding this point.

The homogeneous light I have made use of is that which proceeds from the flame of a mixture of alcohol, water, and salt. I have imbibed with this mixture a quantity of cotton wick which I put behind an unpolished glass placed vertically, This mixture lighted in a dark room, gives a voluminous flame, and the unpolished glass obscured on the other side, forms a tolerably bright luminous field. To render the light still more homogeneous, I placed between the flame and the unpolished glass, a glass of a deep yellow color. Every thing being thus prepared, I placed successively before the unpolished glass, the apparatus already described in the 28th section of my memoirs, and that which served for my experiments or measure, after having reduced, in this latter, the vertical edge of the movable plate, by prolonging that of the fixed one. These apparatus thus projected, were placed on a field of very considerable brightness, and a light so nearly approached to homogeneousness, that, in observing them by refraction across a prism placed vertically at five metres distant, their image not only preserved a perfect plainness, but only presented laterally a greenish shadow so faint that it requires great attention to perceive it. I ought also to mention, that in order to give the eyes more sensibility, the experiments have not been made by day in a dark room, but at night.

But, under the circumstances I have just described, and which necessarily exclude the effects which might have depended on the aberration of refrangibility, the above apparatus have shewn me a very distinct irradiation. The same result was discovered by M. M. Burggrarve and Le Francois, two of the persons who assisted me in the experiment or measure mentioned in my memoir, and who are, therefore, accustomed to judge of the phenomena of irradiation. In order afterwards to test the results produced, and those which would arise from a compound light, and one of a similar brilliancy, I placed before the above mentioned unpolished glass, another similar glass, behind which I lighted several wax candles so disposed as to afford an uniform light, and these I moved to and fro' till the brilliancy of this second glass appeared equal to the first. An opaque screen also separated the wax-candles from the alcohol flame, so that each of the glasses received but one of the two lights. I had thus two luminous fields of the same brilliancy, but, of which, one was lighted by a homogeneous yellow light, and the other by a light, which, without being as white as that of the day, was evidently sufficiently compounded for the purpose. I then placed before these two luminous lights, the apparatus of irradiation in themselves identical, and so disposed, that in observing them simultaneously, it was easy to perceive if the irradiations developed by the two lights, differed sensibly from each other. But this comparison, made by the two persons before mentioned, and myself, gave us no appreciable difference; the two apparatus shewed a distinct irradiation, but that which proceeded from this compound light was neither more nor less extensive than that which arose from the homogeneous light.

These facts I think, lead to the following necessary conclusions, that we must admit the existence of the aberration of refrangibility of the eye; which irradiation ought to be attributed to another cause, and the effect of the aberration considered as entirely hidden, under ordinary circumstances, by the band of irradiation.

XLVI. *Brief notice of the Extrication of Barium, Strontium, and Calcium, by exposure of their chlorides to a powerful voltaic circuit, in contact with mercury as a "cathode;" and the distillation of the resulting amalgams by means of vessels of iron.**

Agreeably to the statements made by Sir Humphrey Davy in his Bakerian lecture, that celebrated chemist was not quite successful in isolating either barium or strontium, as he declares that he was not enabled to expel from them completely the mercury, by amalgamation with which they had been reduced to the metallic state from that of oxide. In the most successful experiments made by him for the isolation of calcium, the tube broke, and the mass took fire before the distillation was accomplished.

---

* From Silliman's Journal.

Dr. Hare has recently obtained, by an improved process, all three of the metals above mentioned. In this, saturated solutions of the chlorides are substituted for moistened oxides; the mercury and solutions being both refrigerated by ice-water, or a freezing mixture within receptacles contrived for the purpose. Two deflagrators, each comprising one hundred Cruickshank pairs, severally exposing one hundred inches of zinc surface, were employed alternately. In consequence of this mode of operating, the charge of acid, at first feeble, was gradually strengthened by additions, so as to render the reaction towards the close as forcible as at the commencement. This is highly important, since the difficulty of decomposing the chloride increases with the quantity of calcium combined with the mercury.

The resulting amalgams were severally subjected to distillation by means of a crucible enclosed in an air-tight iron alembic, being protected from the access of air by caoutchouchine naphtha, mercury and desiccated hydrogen. For the complete expulsion of the mercury, a heat above the softening point of glass was necessary.

So great was the avidity for oxygen of the metals thus obtained, that to see their bright, metallic white colour, the eye must follow closely after the movements of a file or burnisher employed to expose a fresh surface. Metallic whiteness is soon succeeded by a straw color, as in the case of steel filed at a high temperature. But the whole mass is soon reduced to a pulverulent oxide. Of this the color is dark, in consequence of a resinous coating resulting from reaction of the metal with the naphtha necessarily employed to prevent the excess of atmospheric oxygen. In consequence of this coating being insoluble in water, but readily soluble in hydric-ether, oxidizement ensues more readily in the last-mentioned liquid than in water.

The metals in question were all brittle, and much harder than potassium or sodium. By the evolution of the mercury, they are left in a form resembling, in some degree, that of metallic arsenic.

Davy informs us that he employed only fifty or sixty grains of mercury. Dr. Hare has employed a half-pound avoirdupoise, which is seventy times as great, and is under the impression, that with sixty grains it would not be possible to isolate a perceptible quantity of calcium. Operating with much larger quantities of amalgam, he has found no residue besides a stain upon the glass of the tube employed to distil off the mercury.

XLVII. *Process for a Fulminating Powder—for the Evolution of Calcium and Galvanic Ignition of Gunpowder; by* DR. HARE.

An equivalent of quick lime, with an equivalent and a half of bycyanide of mercury, is subjected to a red heat in a porcelain crucible enclosed within an air-tight alembic of iron, so as completely to exclude atmospheric air. The resulting residual mass was found, in two experiments, to have the weight which would correspond with an equivalent of calcium, united to an equivalent of cyanogen. From the filtered solution of the compound thus produced, in acetic acid, a precipitate was obtained by the addition of nitrate of the protoxide of mercury. This precipitate when well dried was found to constitute a powder capable of fulminating by percussion.

Isolation of Calcium by the deflagration, in a receiver, of desiccated hydrogen, of the compound formed by igniting in a close vessel, bicyanide of mercury with pure quick lime.

By exposing the compound of cyanogen with calcium, obtained as above mentioned, either in vacuo or in an atmosphere of desiccated hydrogen to a current from two hundred pairs of Cruickshank plates, each comprising one hundred square inches of zinc surface, the calcium appeared to be isolated. Particles displaying metallic characteristics under the burnisher, and which effervesced in water, were observed, while the gas escaping had an odor resembling that of silicuretted hydrogen evolved by silicuret of potassium, under like circumstances.

Deflagration of phosphuret of calcium.—By exposure of the phosphuret of calcium to the current from the deflagrators, as above described, calcium containing a trace of phosphorus appeared to remain. The phosphorus was condensed upon the receiver in sufficient quantity to obscure the glass. The residual mass thrown into water effervesced extricating hydrogen slightly phosphoric in its odor. When compounds of carbon with calcium were similarly exposed, the residue had a metallic appearance, but did not decompose water.

On one occasion, a portion of the charcoal forming the anode was fused into a globule, having the consistency and other characteristics of plumbago. It appeared more compact than the globules obtained by us many years since, of which a portion was forwarded to Dr. Hare at the time.

Of Professor Daniell's adoption of Dr. Hare's method of igniting gunpowder by galvanic ignition.

During the summer of 1831, a method of igniting gunpowder by galvanism was contrived by Dr. Hare, the idea having been suggested by the abortive efforts of an ingenious individual of the name of Shaw, to effect this object by mechanical electricity. Of the apparatus described for the purpose in question by Dr. Hare, engravings and descriptions were published in this Journal in the autumn of 1833. We advert to these facts now in consequence of the recent publication of analagous experiments by Pro. Daniell, King's College, who in this case, as well as in that of his "re-invention" of a hydro-oxygen blow-pipe of Dr. Hare, was no doubt ignorant that he had been anticipated.

In performing his experiments, it would seem that Pro. Daniell used his ingenious apparatus, known as the sustaining battery, which, although peculiarly qualified for the production of a durable current, is, as we think, far less competent than the calorimotor of Dr. Hare, to produce a transient intense ignition, such as would be the most efficacious in igniting gunpowder.

XLVIII. *Method of adjusting the Dipping Needle;* by THOMAS PERRY, Professor of Mathematics, United States Navy.*

TO THE EDITORS.

Gentlemen,—Finding it necessary, some months since, to re-adjust a needle belonging to my instrument for measuring the magnetic dip, I adopted a method which, from its simplicity, I am induced to communicate, in the hope that it may be serviceable to others in similar circumstances.

The instrument being firmly fixed, and accurately levelled, the direction of the magnetic parallel of latitude and meridian, and the true dip, were approximately ascertained by properly reversing its faces, axis, and poles. The plane of its face was then made to coincide with the parallel of magnetic latitude, and the substance of the needle carefully ground away, *from the sides perpendicular* to its plane of motion, until it assumed the same position (the vertical) upon reversing its axis. The plane of the face was then brought into the magnetic meridian, and the needle again ground upon the sides parallel to the plane of motion, so as not to affect the previous adjustment much, until it indicated nearly the true dip. These processes were successively repeated, until the errors, saving such as result from the imperfection of the circles were found, upon making all possible reversions, to be less than the probable errors of observation.

---

* Silliman's Journal.

This method may be advantageously employed in the final adjustment of new needles. I have employed it successfully in one instance. Two small screws, at right angles with each other, might also be added, which would render grinding unnecessary; but their weight would prove some incumbrance, and they would increase the liability of the adjustments to derangement.

The value of the process results from the difficulty of rendering manufactured and tempered steel devoid of magnetism. Its correctness of principle is obvious from the impossibility of correct indications in two different positions of the needle, except when the centre of gravity coinciding with the axis of motion, the influence of this force becomes nothing in all cases.

In making these adjustments, it is better that the magnetism be of feeble intensity, provided that it be sufficient to overcome inertia and friction; as, in this case, the influence of any other force is more obvious. Any two different planes or even the same might be employed by a little modification of the process; but those specified are most eligible, as in them the forces affecting the position of the needle, present the greatest disparity.

U. S. Ship Independence, Jan. 28, 1839.

XLIX. *Formula for discovering the Weight and Volume in a mixture of two Bases, by* Dr. JNO. M. B. HARDEN, Riceboro, Liberty County, Geo.*

TO THE EDITORS.

In the 12th volume of the "Philosophical Magazine" there is a paper by Mr. Golding Bird, upon the subject of "indirect chemical analysis," in which he gives two formulæ, by Puggendorff, for the quantitative estimation of two different bases in mixtures of those bases. These formulæ are sufficiently exact, but probably not as simple or comprehensive as might be desired. He alludes also to one annexed by the French translator to the "Analysis of inorganic bodies," by Berzelius, which I do not find in the English translation of that work. As it may be well to multiply methods for the solution of such problems, I send you the following formula, which, although from the well known principles which it involves, I cannot suppose that it has any claim to novelty, I have never seen proposed for this object. If you should consider it worthy the notice of the analytic chemist, you will please insert it in your highly useful Journal.

---

* Silliman's Journal.

In the mixture of two bases, it is proposed to find the weight and volume, or bulk of each base, by having given the specific gravity of each ingredient, together with the specific gravity of the mixture and its weight. Now, since the specific gravities of each base or ingredient of the mixture are supposed to be known in most, if not all cases, all that is necessary will be to determine by experiments, the specific gravity and weight of the mixture, in order to find the quantities desired. Let A = sp. gr. of one ingredient, B = sp. gr. of the other, and C = sp. gr. of mixture. Let also the weight of the mixture = 1, and x and y = the weights of the bases; then it is evident that

$$\frac{x}{A} + \frac{y}{B} = \frac{x+y}{C} = \frac{1}{C} \text{ and } x + y = 1.$$

These equations reduced, give

$$x = \frac{AC - AB}{AC - BC} \text{ and } y = \frac{AB - BC}{AC - BC}.$$

Multiply these fractions by the number expressing the weight of the mixture, and we have the weight of each base or ingredient; and as the volumes are inversely as the specific gravities, they are found by dividing the weights by the sp. gr of each.

We give as an example, the mixture of oxygen and azote in atmospheric air:

$$x = \frac{1.1111 - 1.1111 \times .9722}{1.1111 - .9722} = \frac{309}{1389} \text{ proportional weight of oxygen.}$$

$$y = \frac{1.1111 \times .9722 - .9722}{1.1111 - .9722} = \frac{1080}{1389} \quad \text{do.} \qquad \text{do. of azote.}$$

Now, since 100 cubic inches of air weigh 30.5 grains, it will be found that the *weight* per cent. of oxygen in atmospheric air is 22.23, and of azote 77.77, divide these by the sp. gr. of each, and it will be found that the *volume* per cent. of azote is 79.8, that of oxygen 20.2 nearly, which corresponds exactly with the result of the most rigid and careful experiments.

I need scarcely remark that this formula applies only in cases where the specific gravities are determined by the same standard of comparison, although in every case they may be reduced to the same by an easy mathematical calculation.

Liberty Co. Geo. Aug. 15th, 1839.

L. *Of the Reaction of Sulphuric Acid with the Essential of Hemlock;* by Mr. CLARK HARE, of Philadelphia.*

If equal parts of sulphuric acid and oil of hemlock be mingled together, refrigeration being employed to prevent too great a rise of temperature, a black acid resinous mass results. By the addition of carbonate of lead and water, the unaltered sulphuric acid, present in great quantity, is converted into an insoluble sulphate, which, mingling with the resin, gives rise to a yellow mass resembling putty in its consistency, while there will be found dissolved in the water two soluble salts of lead.

The presence of a very large quantity of coloring matter, interferes with the examination of these salts. This, however, in a great measure disappears on precipitating the lead by sulphydric acid gas, resaturating the liberated acids by the carbonate, and again throwing down the lead in the state of a sulphide. The partially decolorized acids thus obtained may then be saturated with barytes, and the resulting salts evaporated to dryness, when they assume the appearance of an amorphous mass. By washing with absolute alcohol, one of the salts present in this mass is dissolved. On the solution of the other in water, and subsequent crystallization, it proves to be the acetate of barytes.

The salt dissolved in the alcohol does not appear susceptible of crystallization, probably on account of its extreme solubility. On drying it assumes a gummy appearance, and by still farther desiccation, may be obtained in the state of a dry mass destitute of cohesion, and susceptible of being with facility reduced to the state of a powder.

When exposed to heat in a retort, this salt resists an elevated temperature without alteration, but at length, if heated rapidly, carbonizes, giving off sulphurous acid and a small quantity of essential oil and water. There remain in the retort a spongy carbonaceous substance, and a large quantity of sulphite of barytes. As this result proved the acid united with the barytes to consist of organic matter, combined with sulphuric acid and modifying its properties, in order to ascertain the quantity of the latter present, barytes was precipitated by carbonate of potash, the precipitate weighed and the resulting potash salt evaporated to dryness. It was then intimately mingled with the black oxide of copper and nitrate of potash, nitric acid added, and the whole mass gradually heated to redness. Red fumes are given off during the whole of the process, and while the nitric acid at the beginning of the operation prevents the deoxidation of any portion of the sulphuric acid; at the end, the oxide of copper prevents the explosive reaction which would ensue, were nitric acid and nitrate of potash alone present.

---

* Eldest Son of Dr. Hare.

The result of two experiments made in this manner, the mass after ignition being washed with diluted chlorohydric acid, and the solution precipitated by barytes, was as follows:—Carbonate of barytes 12½ gr's. Sulphate of barytes 16½ gr's. Carbonate of barytes 13¼ gr's. Sulphate of barytes 16½ gr's. The quantity of sulphuric acid as calculated from the quantity of sulphate precipitated, is in each case, 5.59 gr's., while as calculated from the precipitate of carbonate of barytes, on the supposition that one atom of it is present in the barytes salt for each atom of base, it would be 5 gr's. in the first instance, and 5.3 gr's in the second. It will therefore be perceived that in both experiments the quantity of sulphuric acid, as calculated from the results, exceeds the quantity necessary for forming an equivalent with the base present. This must be attributed either to some inaccuracy in performing the analysis, or to the presence of a small quantity of some sulpho-organic acid, containing in its neutral salts, two atoms of sulphuric acid for each atom of base. The former explanation is by far the most likely to be true, and it seems probable that the composition of a neutral salt of this acid may be represented by one atom of sulphuric acid, one atom of organic matter, and one atom of base.

A number of compounds possessing the properties of acids have been discovered, consisting of an acid of sulphur modified by some organic substance. These compounds may be divided into two classes. In one are comprised those acids which are composed of two atoms of sulphuric acid, united to one of organic matter acting as a base, and which consequently, in forming neutral salts, unite with but one additional atom of base. In the neutral salts formed by the other class, two atoms of sulphur are also present for each atom of organic matter and each atom of base, but are combined with oxygen in such proportion as to form hyposulphuric acid, so that the organic matter present cannot be considered as acting the part of a base. Under the first of these heads may be enumerated the sulphovinic, sulphetheric, sulphomethylic, and sulphocetic acids; under the second, the benzosulphuric, sulphonapthalic, and probably the sulphovegetic, and several others. For the acids contained in the first class, custom seems to have assigned as a nomenclature, a name derived from the organic matter entering into their composition, modified so as to terminate in *ic* and having the term sulpho prefixed. For the second, no fixed rule seems to have been laid down. The German chemist who discovered one of the two acids whose composition has been ascertained with sufficient accuracy to enable us with certainty to place them under this head, gave to it the name of benzosulphuric, while the other acid still retains the appellation of sulphonapthalic, which it received when its composition and properties were still supposed to be analagous to those of the sulphovinic and other acids which belong to the first class. The acids described in this article, if the view given of its composition be correct, must be considered as belonging to a division of the second class hitherto unoccupied, unless by the sulphindigotic acid of Berzelius. In the hemlosulphuric, as in the other acids of this class, there is present one atom of an oxacid of sulphur modified by an atom of organic matter which does not, as in the first class, act as a base, or diminish the saturating power of the acid. If, therefore, we should adopt the nomenclature of the German chemists, with the change of sulphuric into hyposulphuric as necessary to designate with precision the acid of sulphur in question, for the acids of the second class, calling them benzohyposulphuric and napthalohyposulphuric; and applying the same idea to the acid described in this article, name it hemlohyposulphuric, the ends to be attained in forming a nomenclature would perhaps be as well answered as is practicable, without departing too widely from established custom.

Hemlosulphuric acid possesses a sour taste and peculiar odor. It does not appear susceptible of crystallization, either when free or as far as I have examined its compounds, when combined with bases. The salts which it forms with potash lime and barytes leave in the mouth a decided and long continued impression of sweetness. Though extremely soluble they are not deliquescent. If the hemlosulphate of barytes be kept for a length of time at a temperature between 500° and 600°, the sulphate of barytes and organic matter of which it is composed separate, the latter in the shape of a resinous powder insoluble in water, though soluble in alcohol and ether. This seems a singular instance of a body very soluble in water, affording by the mere separation of its constituents, two others eminently insoluble in that liquid.

In the resinous yellow mass into which the greater part of the hemlock of oil is converted by the action of the sulphuric acid, there is present a yellow oil which contains sulphuric acid combined with it in a neutralized state. By the action of ether, this oil may be dissolved, and by subsequent evaporation, deposited, but when thus obtained it is contaminated by so much resin that though the presence of sulphuric acid may be ascertained, it is impossible to determine the atomic composition.*

From the reaction of sulphuric acid, with oil of turpentine, nothing more appears to be produced than a reciprocal decomposition; though a different result might have been anticipated from the close analogy which appears to exist between this essential oil and that of hemlock. Caoutchouchine, however, reacts with sulphuric acid in a manner quite analagous to the oil of hemlock, giving rise to a yellow resin and an acid compound of sulphuric acid and organic matter, which forms soluble salts with lead and barytes. An oil, however, separates and floats on top, which appears insusceptible of farther attack from the acid.

LI. *Results of Experiments on the Vibrations of Pendulums, with different suspending springs; being the substance a paper by* W. J. Frodsham, F. R. S., *read before the Royal Society, June* 21, 1838. Forwarded for insertion in this Journal.

The experiments of which I am about to give an account, and from which I propose to draw some practical conclusions, were undertaken with a view to determine whether some particular condition of the suspending spring of the pendulum, with respect either to its length, its strength, or both, might not cause it, with a lighter maintaining power to produce a given arc of vibration, or, with a given maintaining power, to produce a greater arc of vibration than any other; and at the same time to ascertain whether some practical means might not be devised for making unequal arcs of vibration in the ordinary pendulum, correspond to equal intervals of time.

My attention was drawn to the subject many years ago, when having replaced the spring of a turret-clock by a stronger one, I found the arc of vibration materially altered.

---

* It is well known that by the reaction between chouchydric acid and pure oil of turpentine, two species of artificial camphor are generated, one solid, the other liquid. Having obtained both of these compounds a few years since. Dr. Hare subjected the oil of hemlock to chlorohydric acid by the same process, but could not thus obtain any concrete camphor. That which he did obtain was analagous to the liquid artificial camphor above mentioned.

Having often reflected upon the subject, I at length resolved to make some experiments to satisfy my mind respecting it; and I accordingly had made for the purpose a lenticular pendulum bob of about fourteen pounds weight, a cylindrical rod passing through it, with a nut working on a screw at the lower end, and supporting the bob.

The upper end of the rod was slit to receive the spring; and the spring and the rod were attached to each other by a pin passing through a hole in both.

But before fixing the pin, what I call an *isochronal piece* was slid over the top of the rod, and if this part of the apparatus had served only to attach the rod and spring more firmly together, and prevent any wavering motion of the pendulum, it would have rendered an important service. This, however, was but a secondary and incidental effect of its application.

The piece, which I have so named, is a brass tube about five inches long, fitting the pendulum rod very nicely, and slit to form a spring for about an inch at the bottom, so as to slide rather stiffly on the rod. At the upper end of the tube is a *clip*, which is made to embrace the suspending spring firmly by means of two screws; so that after the pendulum has been brought to the proper length by the adjusting nut at the lower end of the rod, the length of the acting part of the suspending spring may be varied at pleasure, without in the least altering the length of the pendulum, by merely sliding the isochronal piece up or down the rod, and tightening the screws of the *clip*.

I also provided five springs of different degrees of strength, and a silken string, by which, in the first experiments, the pendulum was suspended.

The pendulum used was an uncompensated one, but in each experiment it was adjusted to nearly the proper length for mean time.

Commencing with the silken thread, or rather two parallel threads, one behind the other, I suspended the pendulum within the case of a clock, perfectly detached from the works, no maintaining power being applied.

Each degree of the scale on which the arcs of vibration were noted, was nearly ·8 of an inch in length, and a degree was sub-divided into twenty equal parts.

I drew the bob aside 2°, and leaving it to vibrate by its own gravity, I found the arc of vibration was reduced from 2° to 1°, and from 1° to ½°, in the times noted as under.

Arc of vibration	from 2° to 1°	in	20m.	15s.
Do.	do. 1 to ½		23	6

On repeating the experiment, the results were:—

Arc of vibration from	2° to 1° in	21m.	0s.
Do. do.	1 to ½	24	0

Drawing the pendulum aside 1°, I found from five successive trials that the arc of vibration was reduced to half a degree in the times following:—

From 1° to ½° in	21m.	45s.
Do. do.	22	45
Do. do.	22	0
Do. do.	22	30
Do. do.	23	0
Mean, - - -	22	24

The mean of the two preceding corresponding results is 23m. 12s. The difference may be satisfactorily accounted for, by the difficulty of setting off the pendulum at the precise point intended, and of noting the time when the arc is diminished to the proposed quantity.

It is apparent from these experiments, that when a pendulum is freely suspended, and left to vibrate from its own gravity, the arc of vibration is sooner reduced from 2° to 1°, than from 1° to ½°, as might indeed be anticipated from the increased resistance experienced by the bob, while moving through a greater space in the same time.

I attached the pendulum, suspended as before, to a clock, with a maintaining power of 6lb. 8oz., but the clock stopped in 39 minutes; and setting it off again, it stopped in 43 minutes; but on applying a weight of 6lb. 11oz., the clock continued to go; thus showing that a weight of 6lb. 11oz. was sufficient to keep the pendulum in vibration, while one only 3oz. lighter was not.

The arcs of vibration in the preceding experiments being smaller than is desirable in practice, I proceeded to experiment with heavier weights, the pendulum being still suspended by the parallel silk threads, noting in each case the arc of vibration and the *rate* of the clock, viz., its gain or loss in 24 hours.

In the following experiments each succeeding pair is to be considered as giving the results for two consecutive days, though more than one day occasionally elapsed between the times at which the sets were taken.

Weight.			Arc of Vibration.		Rate.	
14	6oz.		2°	3,	— 9s	.0
8	0		1	30	+ 0	.7
14lb.	6		2°	3	— 10	.0
8	0		1	30	0	.0
11	2	1	1	45	— 7	.0
8	0		1	30	+ 1	.0
19	0		2	15	—13	.0

It hence appears, that when a pendulum is suspended by a flexible string, a heavier weight and a consequent greater arc of vibration, causes the clock to lose.

The following are the dimensions of the springs which were experimented with:—

Number-	Breadth	Thickness..
1	·350 inch	·001 inch.
2	·390	·002
3	·395	·003
4	·395	·004
5	·400	·035

The pendulum being suspended by the weakest string, No· 1, the times were noted as before, in which the arcs of vibration were reduced from 2° to 1°, and from 1° to ½°, no maintaining power being applied.

Arc reduced from	2° to 1° in	1h.	58m.
Do.	do.	1	57
Do.	1 to ½	2	8
Do.	do. ½	2	10

With the same spring, and a maintaining power of 4lb. 1oz. and 2lb. 2oz., the following arcs of vibration and rate of the clock resulted from two consecutive days, the effective length of the spring being ·92 inches.

Weight.	Arc.	Rate.
4lb. 1oz.	2° 3′	– 9s. 6
2 2	1 30	– 6 1

The pendulum being suspended with spring No. 2, and clipped at ·92 inch, without maintaining power, the arcs of vibration were reduced as follows:—

From	2°	to	1°	in	2h.	20m.	0s.
Do.	2	—	1	—	2	20	44
Do.	1	—	½	—	2	26	0
Do.	1	—	½	—	2	26	0

Applying 4lb. 1oz. and 2lb. 2oz. in succession, as a maintaining power, I found as under:—

Weight.	Arc of Vibration.	Rate.
4lb. 1oz.	2° 9′	−0s. ·2
2 2	1 36	+2 ·5

With spring No. 3, and effective length ·92 inch, the following results were obtained on two consecutive days:—

Weight.	Arc.	Rate.
4lb. 1oz.	2° 15′	—2s. ·5
2 2	1 39	—2 ·8

Reducing the effective length of the spring to ·8 inch, the following results were obtained on consecutive days:—

Weight.	Arc.	Rate.
4lb. 1oz.	2° 9′	0s. ·0
2 2	1 30	0 0
4 1	2 9	—0 ·5
2 2	1 30	—0 ·2
4 1	2 9	—0 ·2

Hence, with either of these lengths of this spring, the rate does not appear to be perceptibly influenced by the extent of the arcs of vibration. In fact, the vibrations of the pendulum may, for all practical purposes, be considered as isochronous.

The effective length of the spring was then increased to ·92 inch, and the following results were noted, without maintaining power:—

Arc reduced	from 2°	to 1°	in 2h.	26m.	0s.
Do. do.	2	1	2	25	15
Do. do.	1	½	2	37	0
Do. do.	1	0	2	36	40

On three other occasions, with the same spring, and effective length ·92 inch, the following comparative results were obtained:—

Weight.	Arc.	Rate
4lb. 1oz.	2° 15′	—4s. ·0
2 2	1 39	—4 ·2
4 1	2 15	—5 ·0
2 2	1 39	—5 ·2
4 1	2 15	—5 ·0
4 1	2 15	—5 ·0

Shewing that even with different lengths of this spring, the vibration may be considered as isochronous, with considerably different arcs of vibration; and also that with this spring, a greater arc of vibration is produced with the same maintaining power, than with any other spring that has been tried.

Spring No. 4 was next applied without maintaining power.

With it the arc of vibration was from

2° to 1° in	1h.	47m.
do.	1	48
do.	1	50
1° to ½°	1	54
do.	1	55
do.	1	58
do.	2	0

Applying maintaining power of 4lb. 1oz. and 2lb. 2oz. respectively, with ·97 inch effective length the following results were noted :—

Weight.	Arc.	Rate.
4lb. 1oz.	2° 6′	—2s. ·2
2 2	1 30	1 ·2

Even with this comparatively stiff spring, the arc of vibration is greater with a maintaining power of 4lb., 1oz. than it was with 14lb. 6oz., when the pendulum was suspended by two parallel silk threads. But the rate appears to vary more with the arc of vibration, than it did when No. 3 was used.

Reducing the length of this spring to ·66 inch, the following results were obtained :—

Weight.	Arc.	Rate.
4lb. 1oz.	2° 3′	—14s. ·1
2 2	1 27	—11 ·5

Sliding up the isochronal piece still further, till the length of the effective part of the spring was reduced to ·50 inch, the following were the results :—

Weight.	Arc.	Rate.
4lb. 1oz.	2° 3′	—18s. ·0
2 2	1 12	—14 ·5

This further shortening of the spring appears to have had a perceptible effect on the arc of vibration, when the lighter weight was applied.

I lastly attached the strongest spring, No. 5, and with effective length 1·0 inch.

Weight.	Rate.
4lb. 13oz.	—15s. ·5
2 10	—13 ·5

Reducing the length of this spring to ·8, the following results were obtained:

Weight.	Rate.
6lb. 3oz:	—14s. ·6
2 10	—12 ·4

Sliding up the isochronal piece still further, till the length of the effective part of the spring was reduced to ·50 inch the following were the results:

Weight.		Rate.
4lb.	13oz.	— 12s. ·0
2	10	— 8 ·2

The lighter weight, 2lb. 2oz. employed on experimenting with the weaker springs, was found insufficient to keep the pendulum in vibration with No. 5; 2lb. 10oz. was found adequate to the purpose, and it was therefore employed.

In experimenting with this spring, the arcs of vibration were not noted, as I found that both it and No. 4 were too strong for the weight of the bob I was using, and to which the experiments indicate that No. 3 was excellently adapted.

The arc of vibration with the spring, No. 3, (viz. 2° 15′) using a weight of 4lb. 1oz. required 19lb. weight to produce it when the pendulum was suspended by the silken threads.

It appears then, from the preceding experiments on suspending springs differing in length and strength, that there is one which, with a given maintaining power, produces a greater arc of vibration than others, and gives the same arc of vibration with a smaller maintaining power; and, further, that with this same spring the vibrations may, in point of time, be all considered as isochronous, whether the arcs are large or small. And with the aid of the *isochronal* piece, a spring of the proper length and thickness may easily be selected in a very few trials.

It may be noticed too, that unless this pendulum is first *isochronized* by some such method as that which has been pointed out, anomalies may be imputed to *imperfect compensation*, which have their origin in a very different source.

In fine, it may be stated in conclusion, that if the pendulums of astronomical clocks were furnished with what I have called an isochronal piece, any person possessing a few springs of different degrees of strength, may with very little difficulty determine what spring is best adapted to the weight of the pendulum, and also what part of the spring may be most advantageously employed in action; and I shall not think that the attention which I have given to this subject has been misspent, if any thing that I have done may contribute to the advancement of an art to which I have been professionally devoted during the whole of my life.

London, March, 1839.

LII. *Effects of Lightning upon the packet ship New York;* by Mr. Charles Rich, at the request of the Editors.*

Upon my first visit to Liverpool in May, 1827, the vessel in which I arrived was moored in Prince's dock along side the packet ship New York, Capt. Bennett. This ship I repeatedly visited, and indeed was obliged to cross her deck to reach the wharf. Having been informed that she had been injured by lightning during her passage, I examined her several times, and the following are the main facts that I remember.

The ship sailed from New York in April, and on the third day out, being the 19th, while in the Gulf Stream, in lat. 38° 9′ N. and lon. 61° 17′ W., was struck by lightning at about daylight in the morning. The passengers being still in their berths, were roused by a heavy report like that of a cannon close to their ears, and the cabin was filled with a dense smoke smelling like sulphur. It had been broad daylight, but was now almost dark as night. Rain fell in torrents—hail covered the deck; the lightning and thunder were almost simultaneous; the sea ran very high, and the water being at 74° F. and the air at 48°, the copious evaporation produced pillars of condensed vapour reaching to the clouds. The scene was one of terrific sublimity. Some parts of the ship and spars were for a moment on fire, but were quickly extinguished by the rain.

The fluid first struck her main royal mast, burst asunder three stout iron hoops with which it was bound, and shattered the mast head and cap. It passed down the mainmast, one branch entered a store-room and demolished the bulk heads and fittings; thence it went into the cabin, and conducted by a lead pipe passed out through the ship's side between wind and water, starting the ends of three five inch planks. During its progress it burst open the harness casks, shivered to pieces the large looking glass in the ladies' cabin, and being conducted by the quicksilver on the back, it left the frame uninjured; it overturned the piano forte, split into several pieces the dining table, and by its influence so highly magnetized the chronometer as to render it during *that* passage not trust-worthy. Most of the watches which were under the gentlemen's pillows were so highly magnetized as to stop them, and render it necessary to remove all the steel work. The gentlemen themselves were, without exception uninjured, owing doubtless to the non-conducting properties of the beds upon which they were sleeping. At the time the ship was struck, the lightning conductor had not been put up; but it was immediately after the accident raised to the main-royal-mast head.

---

* With additional facts selected by the editors from the full account published in Liverpool, May 12, 1827, and quoted in the New York Spectator, June 20, 1827.—*Silliman's Journal*

The conductor consisted of an iron-chain with links one fourth of an inch thick and two feet long, turned into hooks at each end; at the top it ended in an iron rod half an inch thick and four feet long, having a polished point and rising two feet above the mast head; the chain descended down over the quarter, and being pushed out from the ship's side about ten feet by an oar, descended a few feet below the surface of the water.

Near two o'clock, P. M. it was observed that only four seconds intervened between the lightning and the thunder. At two o'clock there was a simultaneous flash and a shock like that in the morning; passengers in the cabin saw the appearance of a ball of fire darting before them while the glass in the round house came rattling down. To those on deck the ship appeared to be in a blaze, so vivid was the flash which they saw distinctly darting down the conductor and agitating the water. All parts of the ship as before were filled with smoke smelling of sulphur. Although the conductor was of the size which Dr. Franklin thought sufficient to sustain the severest shock of lightning without injury, yet it was literally torn to pieces and scattered to the winds, while it saved the ship. The pointed rod at the top of the conductor being fused, was shortened several inches and covered over with a dark coating; some of the links of the chain had been snapped off and others melted.*

The shock affected the polarity of all the compasses on board, causing them to vary from the true point and to range between each other, but they gradually returned within three points of truth. The chronometer of Captain Bennett, the commander of the ship which did not usually vary more than three seconds in crossing the Atlantic, was now quite out of time; it had gained for a considerable period seven-tenths of a second (in 24 hours,) and being 9m. 42s. *slow* of Greenwich time when the vessel left New York, was found at Liverpool to be 24m. 33s. *fast* of Greenwich, making a difference of 34m. 15s.

Three gold lever watches belonging to gentlemen passengers became so magnetized as to require that the principle part of the steel work should be removed. These parts had become true loadstones acting as magnets. It is in our recollection also that in other accounts published at the time it was stated that the knives and forks and other articles of steel and iron became magnetized. Happily no person was killed, although several were knocked down and more or less injured.

---

* It is said that the same thing once happened in a Dutch church in New York; a chain connected with the clock was melted and probably saved the church.

Remarks.—In consequence of receiving the notice* communicated by Mr. Rich, we have been induced to republish the principal facts in the case of the packet ship New York, although the events happened twelve years ago. The case was so remarkable, that the results ought to be preserved as part of the permanent records of science.

No case could more decisively prove the importance of conductors. Had the ship been furnished with the iron chain and rod at the moment of the first stroke it is almost certain that she would have escaped with little or no injury. Had the topmast which was then shivered (its stout iron bands two or three inches broad and half an inch thick being burst asunder) been protected, there can be no doubt that the lightning would have shot down the conductor, saved the mast, and passed harmlessly into the sea. This was decisively proved in the second case, when the ship was again struck at two o'clock, P. M.

Her iron chain was then up, and the pointed iron rod ascended two feet above the highest topmast. She appears to have been enveloped in a condensed electrical atmosphere; the clouds being so low that the flash and explosion were simultaneous; and had there been no conductor, the second stroke, which appears to have been more powerful than the first, might have proved fatal to many of those on board. The discharge which the conductor received seems to have been more than it was able to convey away; hence some of the people were prostrated although not killed; they were evidently affected mechanically by the explosion, and electrically by the all-pervading electrical atmosphere around, but not being made part of the chain of discharge they escaped with little harm. The conductor was melted at the top and glazed, doubtless with vitrified oxide, and the chain exploded in fragments all about the ship. This proves that the conductor, although it preserved the ship, was not perfect in construction or sufficient in size.

Hooks and chains are objectionable because the continuity of communication is interrupted by the intervening films of air. It were much better to adopt the rope made of twisted copper wire. It might be made of any desired size, and having perfect continuity, there would be no interruption to the passage of the electricity. Being perfectly flexible, it might easily be coiled and stowed away like any of the rigging, and it would adapt itself to any flexion of the spars and masts. It should be terminated above by a solid pointed conductor of copper or iron. Such a protection as this we can hardly doubt would prove sufficient, although in the case of very long ships it might be proper to have more than one conductor. In steam ships there is an additional protection derived from their vast metallic apparatus which by its communication with the water affords the best possible channel of discharge.

---

* Of which a short account was published in this Journal, Vol. xxi, p. 351.

It is true that some years ago an explosion occurred in Charleston harbour, in the boiler of the Savannah steam packet, from her being struck by lightning; caused possibly by the sudden expansion of the steam already generated, or the sudden generation of more steam by the intense heat. In conversation with the late Mr. Samuel Howard in whose charge the boat was at the time, he distinctly attributed the explosion to the lightning.*

In the case of steam ships it may therefore be prudent to pass the conductor directly into the water and not to the boilers or other metallic apparatus; although we should hardly expect any mischief, especially in the Atlantic steamers, whose amount of conducting surface is so prodigious. Every thing however goes to prove that all ships, especially ships for passengers where the risk of life may be great, should be provided with the best metallic conductors.

Another fact which is remarkable in the case of the packet ship New York, is the energetic magnetism that attended the lightning; chronometers, common watches, and compass-needles being all (by the lightning) rendered erratic and dangerous guides, no longer to be relied on. We conceive that good conductors would probably prevent or greatly mitigate even these effects; but as it may not be possible entirely to shun the effects of electricity, and as it is of the utmost importance that the compass-needle should always be correct, we venture to suggest a remedy.

Let every ship be provided with a small calorimotor and the appendages of helix-wires, acids, &c. With this apparatus the needles could be instantly restored or new ones (unmagnetized and carried for the purpose) may be magnetized with certainty and with all requisite energy and dispatch. Practical directions can easily be given if desired.

---

* He was a gentleman of uncommon intelligence and good judgment—SEN. ED.

New Haven, September 9, 1839.—Eds.

LIII. *On a remarkable property of Electrical Tension;* By Christian Doppler, Professor of Mathematics at the Polytechnic Institute of Prague. *From the* Zeitschrift fur Physik, &c., *Vol.* V, *Part* 8, *p.* 342. *Vienna,* 1837.

Marked and decided as has of late been, thanks to the researches of the first philosophers of the day, our progress in all the branches of electrical science, and active as their endeavors have been to add further facts to those we are already in possession of with respect to the reciprocal action of electric currents on each other or upon magnets, or, inversely, the influence of the latter on electrical currents, yet, we are nevertheless, forced to confess, that the insight which we have gained into the essential nature of this mysterious fluid, has by no means kept pace with our progress in other respects.

And however remote our hope may be of seeing this portion of natural science worked out in the satisfactory manner that others have been, yet we cannot but assent to the importance, and indeed the ultimate necessity, of entering upon the enquiry: and consequently, every effort we make and every fact we can adduce, tending, even indirectly, to further the investigation, is worthy of attention.

Bearing this in mind, I do not hesitate to make known the results of an experiment, which, should its truth be borne out by subsequent observers, may possibly lead to inferences of some importance.

Some years ago, on the occasion of my publishing an essay upon the kindred subject of the probable causes of electrical excitation,* I was led to the conclusion, that wherever there is a case of electrical tension there must of necessity occur a change in the shape of the electrified body; and therefore, that on submitting a metal rod to such tension it must necessarily contract. To test the truth of this inference, the following experiments† were instituted alternately. A brass tube of about three feet long, and likewise a solid bar of similar length, but not near so thick as the former, was laid upon insulating supports between the two feelers of a very sensitive arrangement of levers of contact, being however kept out of contact with them, by the insertion of strips of glass of suitable thickness.

---

* See Jahrbücher des k. k. polytech: Institutes zu Wien. vol. 17.

† They were performed at the Polytech: Institute at Vienna, about five years ago, with the aid of the Comparator, an instrument admirably calculated for such delicate measurements.

Now, immediately on receiving even a moderate charge of electricity, the index of the lever of contact began to move perceptibly, and to indicate that a gradual contraction of the bar was taking place, and this motion augmented so rapidly as the tension increased that, in order to enable the eye to follow the range of the index with greater facility, it became necessary to substitute a simple lever of contact, for the compound one which was at first employed. Every time the electric spark was drawn from the bar, or every time that it spontaneously discharged itself, the instantaneous recoil of the index of the lever, indicated the restoration of the original length of the metal; from which, however, there was again a transition to contraction, immediately the state of tension was renewed.

These experiments were repeated several times, and always with the same results, with however this difference, namely, that the contraction when the tube was employed was, probably on account of its greater extent of surface, much more marked than when the bar was used. These results are the more surprising, inasmuch as on account of the gradual increase of temperature, (for in these preliminary experiments, a single pair of galvanic elements was also used,) we should rather have looked for an expansion of the metal.

Now, though at the time of performing these experiments, I had reason to rest satisfied with having completely established what I had in view; yet, I now feel convinced from having subsequently thought the subject over, that the results then obtained, bear out certain inferences not perhaps altogether unimportant respecting the constitution and actual nature of the electric fluid. Nothing but the idea however, that this problematic phenomenon may be looked on by other experimenters as of sufficient importance, to have its existence completely established or disproved, by a repetition of my experiment, could induce me to lay it thus before the public, in a state so imperfect in many respects. And though for the present, that is to say, till it is established as an indisputable fact, I very properly refrain from expressing an opinion on the subject; yet I trust I may be permitted to subjoin a remark or two, and to allude, in passing, to an application of which, this new property of electrical tension is perhaps susceptible.

Simply putting this property of electrical tension beyond a doubt by careful and accurate experiments, would certainly—as far as it went, be a step in science; but the subject would gain additional interest, if in the investigation regard, was at the same time, paid not only to the length, but also to the shape and other qualities of the conductor, semi-conductor, or non-conductor. For in point of fact, it is by no means improbable that a contraction which is considerable enough to be measured and expressed in numbers, will turn out to be proportional to the length of bars of similar form, but that its amount will vary with the different metals employed. And this result may be especially anticipated in the case of such metals as indicate opposite states of electricity, as for instance, copper and zinc. It would, in fine, be well to enquire whether the same identical bar charged to an equal amount of tension, as indicated by the electrometer, first positively and then negatively, would indicate precisely the same amount of contraction.

Now should this power of electrical tension to contract metal rods so considerably (a fact of which, as matters now now stand, I cannot entertain a doubt,) be really borne out by further experiments; the idea of having recourse to it for the construction of an electrometer on a new principle, suggests itself readily enough. Without entering into a discussion as to the best arrangement for such an instrument, I may be permitted to observe that probably any thin strip of metal, one of whose sides is covered with an elastic non-conductor; as for instance, a coat of elastic varnish, would, on being coiled up into a conical spiral, probably answer the purpose very well. One of its ends would have to carry an index, as is the case with a metalic thermometer, or would be made to communicate its motion to a lever.

The amount of contraction thus placed at our disposal and which, all things considered, is by no means inconsiderable, justifies the presumption that such an arrangement would furnish us with a very sensitive electrometer.

It will not perhaps be thought two much if in concluding this short communication, I express the hope that other observers will consider this phenomenon worthy of further notice and examination.

JULIAN GUGGSWORTH.

Wormwood Scrubs, 18th April, 1839.

LIV.—*On the Vindicating Electricity of Compact Solid Insulating Strata.*[*]

The first phenomena that have been observed with regard to the *vindicating* electricity of compact insulating strata, were those, a notice of which was sent by the father Jesuits at Pekin, to the academy of St. Petersburg, in the year 1755, and which may be read in the 7th volume of the new commentaries of this academy. Signor Symmer in his third Memorial, which was read in the Royal Society of London, the 20th of December, 1759, says he charged two thin sheets of glass, joined together by their naked surfaces, and externally coated; when the charge was completed, he took the upper plate, by two of its angles, and when he raised it, he saw that the under plate stuck to it, and remained suspended to it; when he had discharged the plates, the adhesion ceased. He recharged the two plates, then having inverted them when thus united, he made the plate that communicated at first with the chain, communicate now with the ground, and that which communicated with the ground, communicate with the chain; when he found that after the electrization had, in this state of things, been continued a certain time, all adhesion ceased. Using afterwards two plates coated on both their contiguous surfaces, he found that no adhesion took place. Signor Symmer makes use of these two experiments in order to confute the theory advanced by certain philosophers, of two electric fluids, the one *afffluent*, the other *effluent*; he pretends that each of the two distinct united glasses may be considered as the one of the surfaces of a single plate; that one of the glasses is impregnated with an electricity of one kind, and the other glass with an electricity of another kind; he moreover is of opinion that the adhesion of the two naked plates of glass is a demonstrating proof of the existence of two antagonist forces.

Signor Cigna, in the fourth chapter of his dissertation, carried still farther the experiment of the fathers of Pekin, and of Signor Symmer. He relates that two naked glasses, by rubbing the upper surface of them, remained united, both to each other, and to the gilt paper, or the sheet of lead, on which they were placed; that in this state they gave no sign of electricity; that if they were then separated from the paper, or the lead, they manifested on their two external surfaces the same kinds of electricity; that if the paper or lead was again joined to the glasses, the electric signs again ceased; that if the paper, or lead, was kept parted from the glasses by means of a silk ribbon, the paper or lead manifested an electricity contrary to that of the glasses; that if the glasses were likewise kept separated from each other, they also manifest contrary electricities.

---

[*] Beccaria's artificial electricity.

I do not propose to repeat all the numerous experiments which I related in my book intitled, *Observationes atque experimenta quibus electricitas vindex latê constituitor et explicatur.* I am actually employed in promoting my enquiries on this subject, and if I meet with some success, I propose to publish what discoveries I shall be able to make. Mean while I shall only repeat in this place, the experiment which is made with the two plates, A B, *a b*, M N, *m n*, (Pl. IX. fig. 1.) jointly charged, and I shall express the successive effects of the *vindicating* electricity in this experiment with the figure 2.

And first, in order to perceive the unity which really takes place in all the phænomena of the *vindicating* electricity, however contrary to each other some of them may appear, it must be observed, I. That the law of the vindicating electricity of compact insulating strata, for instance, plates of crystal, is the same with the law of the *vindicating* electricity of rare insulating bodies, for instance, silk ribbons.* II. That the whole specific difference between them lies in the former being capable of a charge, which the latter are not. III. Thence it results that the alterations of electricities, which are readily affected with bodies of a rare texture, by disjoining and rejoining them, and not so with compact insulating strata; such alterations are confined to those surfaces of the latter which are kept joined together by the contrary electricities of the other two surfaces, which constantly endeavour to preserve *their contrariety to each other, and their equality with the electricity of the surfaces which are united together.*

For instance, I. Two ribbons contrarily electrified, when they unite together, reciprocally destroy their electricities, and thus remain adherent. After the same manner, if two plates A B *a b*, M N *m n*, are joined by their respective surfaces, *a b*, M N, contrarily electrified (I suppose the surface A B to be positively electrified, and the opposite *a b*, negatively; therefore *M N is positively electrified*, *m n*, negatively) these two contrary electricities will endeavour to destroy each other; the redundant fire in M N will endeavour to diffuse itself into *a b*, and fill up its deficiency; but this reciprocal suppression of electricities cannot be effected otherwise than by a joint annihilation of the excess in A B, and of the deficiency in *m n*; therefore, in consequence of the impenetrablility of the plates, some external communication becomes necessary; and this will no sooner be procured, than the excess of M N will diffuse itself into *a b*, when the electricity of the two surfaces *a b*, M N, will be annihilated after the same manner as the electricities of the two ribbons were before.

---

* Beccaria gives a chapter on *rare* insulating bodies, in the same work.—EDITOR.

Again, the two ribbons, when they are separating, freely recover their electricity, which they had readily lost when they joined; and in the same manner, the two plates M N *m n*, A B *a b*, in the instant they are separating, endeavor to recover on their surfaces *a b*, M N, the electricity they have lost in consequence of their union together, and of the communication of their external surfaces. Yet it is to be observed that the surface M N, in its endeavor to recover its excess, is restrained by the difficulty which the insulated opposed surface *m n*, experiences in dismissing an adequate part of its own fire; and the surface *a b* likewise, in its endeavour to recover its deficiency, is restrained by the difficulty which the opposite surface A B experiences in recovering an adequate excess; whence it happens that the two disjoined plates,—I. Manifest electricities reciprocally contrary; II. Similar electricities take place over the two opposite surfaces of the same plate; III. And this electricity is of the same kind as that recovered by the disjoined surface.

The reason is, that in disjoining the two surfaces, *a b* M N, I. The the surface M N, by endeavouring to recover its former excess, endeavors at the same time to drive away a quantity of natural fire from the opposed surface *m n*. Now, as the latter remains insulated, it cannot transfuse any fire into the ground, neither can it accumulate any within its coating *c d*; it therefore must accumulate it on the open surface of this coating, against the contiguous air: so that there will result an excessive tension in the natural fire of the ambient air, and a redundant atmosphere around *m n*. II. Likewise, in the act of the same separation, the surface *a b*, in endeavouring to resume its former defficiency, draws, according to the Franklinian theory, certain quantity of redundant fire, to the opposite surface A B: now, as this surface remains insulated, it cannot derive this fire from the ground, neither can it draw it from the internal substance of its own coating; it must then draw it from the outer surface of this coating, that is, from the surface of the contiguous air (if before separating the plates, the coatings are taken off, the experiment will equally succeed). Therefore, a particular relaxation will arise in the natural fire of the air around the plate A B; there will result a deficient atmosphere.

This explanation how the atmospheres arise, which take place over the surfaces opposite to those which are disjoining, likewise suffices to explain the singular circumstance of similar electricities arising over opposite surfaces of the same plates. If while the two plates A B *a b*, M N *m n* are separating, two sharp points are kept presented to their external surfaces, the brush appears on the point directed to A B, and the star on the other, which is directed to *m n:* the same force which, when the points are presenting, draws a brush to A B, and drives the fire that forms another brush from *m n*, this same cause I say, when these two surfaces remain insulated, draws to A B the natural fire of the contiguous air, creating a deficient atmosphere over it, and throws excessive fire from *n m*, into the air contiguous to it, raising in it a redundant atmosphere.

That afterwards, over the external surfaces correspondent to *a b*, M N, when they are separating, atmospheres arise that are homologous to the electricity which these surfaces recover, is what appears natural, when we consider, *that the latter surfaces resume, by virtue of their separation, greater electricities than those which can possibly be raised on the opposite surfaces, which are insulated.* This principle being admitted, it follows that if the surface M N, cannot drive from the opposite surface *m n*, a quantity of fire sufficient to produce in it a deficiency equal to the excess recovered by the same M N, it follows, I say, that a portion of this excess must flow outward, against the contiguous air, and there produce a a redundant atmosphere. Likewise, if the surface A B cannot draw to itself a quantity of fire sufficient to produce in it an excess equal to the deficiency recovered by *a b*, it follows that this A B must, from the air contiguous to it, draw a certain quantity of fire, and thus produce a deficient atmosphere over itself. That is to say, the excess redundant in, and flowing of, N M, against the air contiguous to it, *ipso facto* lessens the excess in this M N, and thus brings it to a state of less inequality with respect to the deficiency actuated in *m n*; and the fire which from the contiguous air flows into A B, *ipso facto* lessens the deficiency in it, and thus brings it to a state of less unequality with regard to the excess in *a b*.

These explanations of the *vindicating* electricities of two plates, may be demonstrated by the experiment in which, after jointly charging and discharging them, I continue for an hour and more to obtain sparks by touching them when separated, and again touching them when rejoined; and reciprocally, the above explanations throw a complete light on that same experiment, which I never could repeat without exciting the wonder of those who were unacquainted with electrical operations, and attracting the attention of the Philosophers who came to see my experiments. I join the two plates A B *a b*, M N *m n* together, by their naked surfaces in contact with each other; and then introduce into the coating C D, for instance, the electricity of the chain; the charge completed, I discharge them; this done, I separate them, and touch the coatings; I join them again, and then again touch them; and thus doing, I continue to excite a very long series of sparks: here follows the manner after which I operate.

I begin with exciting sparks from the coating alone of the upper plate; that is to say—I. I continually touch with one of my fingers the under coating *c d*. II. When I separate the plate A B, I take care not to touch its coating C D. III. Having separated this plate, I immediately touch it, and give a spark to it; that is to say, I give to A B an excess adequate to the deficiency contracted by *a b*, at the instant of the separation. IV. I cease touching A B; I rejoin the two plates, and touch again C D, and draw sparks from it; by means of which I draw off the excess I communicated to A B after the last separation, and which it does no longer require, when in a state of conjunction. V. Proceeding thus, with the usual caution, not to touch the coatings in the act of separating, or of rejoining the plates, I continue to give sparks after every separation, and take them back after rejoining the plates.

In general the spark which I draw after rejoining the plates, is more divided than that which I gave after separating them. In very favourable weather, after separating the plates, I often draw two or more successive sparks; but after rejoining them, the fire that leaps from my finger is completely united into one spark, and much more vivid.

In order to understand the reason of this difference, we must consider—I. That the fire which flies from *a b*, in consequence of the deficiency which now takes place in it, goes to M N in order to form the excess which this M N wants; therefore as an excess arises in A B, in consequence of my touching it at times, so a deficiency arises in *m n*, in consequence of its constant communication with my hand. II. When I rejoin the two plates, the excess I have introduced into A B cannot be annihilated but so far as the excess in M N runs to fill the deficiency in *a b*; and the excess in M N does not depart, but when I give fire to *m n*, in order to fill its deficiency. III. In fact, if, while I rejoin the plates, I keep my fingers at a distance from *m n* (or its coating *c d*) then I cannot draw from A B the excess I introduced into it; because as I do not then fill the deficiency in *m n*, the excess cannot be annihilated in M N, nor the deficiency in *a b* supplied. IV. However, when I touch *m n* (or *c d*) while I rejoin the plates, the excess of A B is not for all that thrown out at once, because the surfaces *a b* M N, do not instantaneously touch each other in all their parts; hence a slowness and successiveness take place in all the respective annihilations of the excess in M N, of the deficiency in *a b*, and of the excess in A B. V. But when after separating the plates I present my finger to C D, or (A B) the excess is at once thrown to it from my finger, owing to the violence which the whole A B then wants an excess adequate to the deficiency then completely formed in *a b*.

Conformably to what has been said above, we must take care that every time that the plates are joined, they be pressed together for some few seconds of time, in order that the small charges which have been formed by the separation, may have time both to dissipate entirely, and to arise again with more strength, when the separation will be again effected.

1. If after touching the plates when rejoined, they are again disjoined without drawing a spark, and then rejoined no spark will be thrown from A B, because it has in such case, received no fire. II. If after touching the plates when separated, they are rejoined, then disjoined again, without previously drawing a spark, A B then receives no spark, because it has given none at the time of its last joining with the other plate; *so true it is that insulating bodies contrarily electrified, are disposed, when they join together mutually to annihilate their reciprocal electricities, as well as to recover them again, when they are separated.*

I have hitherto, in the experiment of the two plates, only examined that kind of electricity which is common both to compact insulating bodies, and to those of a rarer texture: I mean that kind of electricity, *by virtue of which they recover, when separated, the electricity which they had lost by their being joined together, and which I call positive vindicating electricity.* Now, I shall in the same experiment, examine that kind of *vindicating* electricity which is proper to compact insulating bodies, *and by virtue of which, when they are separating from one another, they give up the electricity with which they had been impregnated;* this I call *negative vindicating electricity.*

Having therefore jointly charged the two plates A B *a b*, M N *m n*, I begin the operation of successively disjoining and rejoining them: in order to effect this more easily, I clip one of the angles of one of the plates; and then I observe, I. That the plates, when they are disjoining, manifest signs of a *negative vindicating* electricity. II. They afterwards reach to the last limits of this electricity. III. Then successively follow, for a very long space of time, to give signs of a *positive vindicating* electricity. That is to say, I. At first, the surfaces *a b*, M N, when they are separating, lose a part of the electricity with which they are impregnated. II. Then they reach a certain term at which they do not, notwithstanding they are again separated, lose any more of the electricity which remains in them, nor recover any portion of that which they gave up when the *negative* vindicating electricity began to act, or even afterwards when the *positive vindicating* electricity began to take place.

In the meanwhile, the similarity of the atmospheres that take place over the two surfaces of the same plate, both when the *positive vindicating* electricity, and the negative one obtain, though it has been looked upon as fatal to the Franklinian theory, really proceeds from the following principle, which is the foundation of this theory, which is, *that the contrary electricities of plates, which by virtue of the separation of the latter, are become unequal on each opposite surface, severally endeavour to return to a state of equality; that is to say, that electricity on the one of the two surfaces, which the separation, has caused to have grown less, endeavours to lessen the electricity on the other surface; and vice versa, that electricity which, in consequence of the separation, is become superior to its opposite one, tends to increase the latter.*

Therefore, when I at first begin to separate the two plates A B *a b*, M N *m n*, the excess of M N and the deficiency of *a b* endeavour mutually to lessen each other; but the other two surfaces A b, *m n*, being insulated, their respective excess and deficiency are not altered; that is to say, the excessive fire is, as it were, drawn from M N into *a b*; the deficiency in *a b*, thus become less than the excess in A B, and endeavours to lessen it; it therefore drives a portion of this excess in A B, against the air contiguous to it, and thus creates the redundant atmosphere over A B: and reciprocally, the excess in A B being now greater than the deficiency in *a b*, endeavours to increase it; it drives a part of the fire remaining in this *a b*, into the air contiguous to it, and raises over it a redundant atmosphere. Likewise, the excess in M N being become less than the deficiency in *m n*, endeavours to lessen it, it draws fire into *m n* from the air contiguous to it, and thus renders its atmosphere still more deficient; and reciprocally, the deficiency in *m n*, being greater than the excess in M N, endeavours to draw fire into the latter, from the air contiguous to it, and thus raises a deficient atmosphere over it.

On the other hand, when after the rise of the positive *vindicating* electricity, I again separate the plates, both the excess in M N, and the deficiency in *a b*, continue to be reproduced, though the contrary correspondent electricities cannot arise on the surfaces A B, *m n*, which remain insulated: therefore the greater deficiency in *a b*, endeavours to increase the lesser excess in A B, by drawing the natural fire from the contiguous air into it, and thus raises over A B a deficient atmosphere; and reciprocally, the less excess in A B endeavours to lessen the deficiency in *a b*; to that end it draws fire into it from the air contiguous to it, and thus raises over it a deficient atmosphere. Likewise, the greater excess in M N endeavours to increase the deficiency in *m n*, driving its fire from it into the air contiguous to it, whence results a redundant atmosphere over *m n*; and reciprocally, the less deficiency in *m n* endeavours to lessen the excess in M N; to that end it drives a part of the latter's redundant fire into the air contiguous to it, and thus raises a redundant atmosphere over it.

Conformably to these principles. I. When I separate the plates A B *a b*, M N *m n*, for the first time after their being charged, they resist so much the separation, that there is great danger in breaking them. II. From the coating C D a strong spark leaps to the nearest finger of that of my hands which holds the plate A B *a b*, and the edge of its coating C D appears all round sparkling with very vivid brushes: all this demonstrates to me that a diminution of the excess of A B takes place, at the instant when the dificiency of *a b* is forcibly lessened. III. Likewise, in the act of the same separation, a strong spark flies from the finger with which I hold the plate M N *m n*, to it coating *c d*, and its edge appears all round shining with vivid sparks; this manifests to me that a diminution of the deficiency of *m n*, is effected at the same time that the excess of M N is forcibly lessened. IV. Meanwhile, the flashes of light which appear between the surfaces *a b*, M N, while they are separating, are produced by the fire which, by virtue both of the excess in A B which remains superior to the deficiency in *a b*, and of the deficiency in *m n*, which remains superior to the excess in M N, endeavours to leap from the above *a b* into M N. V. In this state of things, the upper plate A B *a b* repels the white ribbon from both its surfaces; over which, as has been explained in the preceding paragraph, similar redundant electricities take place. VI. On the contrary, the under plate, M N *m n*, repels a black ribbon from both its surfaces, by virtue of the deficient atmosphere, which as hath been also explained, takes place over both its surfaces.

The plates being joined again, the intensity of these attractions and repulsions lessen; because the excess of M N, and the deficiency of *a b* are now respectively kept back by the external deficiency of *m n*, and the external redundancy of A B. The adhesion of the plates takes place again, but in a less degree than formerly, proportionably to the diminution which the original charge has suffered from the first separation; and by proceeding to a second separation, the same phenomena continue to take place by virtue of the same causes as formerly, though their intensity is proportionably lessened.

Continuing thus to join and separate the plates, we pretty soon attain a term at which, I. The plates cease to manifest any sensible adhesion. II. In separating them no light appears. III. After the separation, they do not sensibly draw or attract rubbed ribbons. This term is the point of the contrary inflexion, the limit between the negative *vindicating* electricity which takes place at first, and the possitive one which succeeds to it. This term is sooner attained, according as the insulation of the plates is less complete: in this case one plate sometimes reaches to this term a little before the other, which still continues to draw and repel ribbons with a sensible degree of force. Lastly, this term is attained, before the effect of the separations has entirely annihilated the charge introduced at first into the plates. In fact, if they are rejoined immediately after the term is passed, they still give pretty strong shocks.

If, after the term is passed, the plates, are successively joined and separated, but without touching them; they begin, by virtue of these successive separations, to recover their former electricities: that is, the surface *a b* of the plate A B *a b*, begins to recover a part of what deficiency it had at first, and the surface M N, begins to recover also a part of what excess it may have lost. Whence it happens that, after the separation, the deficiency of *a b*, being become greater, endeavours to increase the excess of A B, by drawing into it the natural fire of the air contiguous to it; and reciprocally, the excess of A B, being less than the deficiency *a b* endeavours to lessen it, by drawing into the same *a b*, the natural fire of the air contiguous to it; so that *a b* and A B, then begin to repel the black ribbon. Likewise the excess of M N, being become greater than the deficiency in *m n*, endeavours to increase it, by driving the fire of *m n* into the air contiguous to it; and reciprocally, the deficiency of *m n*, being less than the excess of M N, endeavours to diminish it, by driving the fire of M N into the contiguous air, whence M N and *m n* begin to repel the white ribbon. And thus the *negative vindicating* electricity becomes changed into a *positive vindicating electricity*.

By continuing thus to rejoin and disjoin the plates, those portions of electricity that had been lost are pretty quickly recovered on all sides, by virtue of these successive separations; the adhesion of the plates, and the repulsion of the ribbons also increase in proportion; so that it appears that all these phenomena of the *positive vindicating electricity*, continue till that degree is attained, at which the charges that had been introduced are annihilated.

Beyond this term, if the plates are continued to be rejoined and disjoined, for an whole hour or more, without being touched, they continue to shew some adhesion to each other; they continue when separated, to repel ribbons conformably to the kind of electricity which they have resumed on their internal surfaces, &c.

I have represented in the fig. 2. of the Pl. IX. the series of the above alterations of the vindicating electricity. Now I shall make use of this figure, in order to explain the vindicating electricity of the plate M N *m n*, (Pl. IX. fig. 1.) The same explanation will serve for the electricity of its fellow-plate; only, the ordinates must be taken on the other side of the absciss. Let the two equal right lines O F, *o* F represent the excess introduced into M N by the charge, and the deficiency introduced into *m n*. On the first separation of the plates, M N will, for instance, lose the portion *u* F of its excess: therefore, it will in consequence of this separation appear negatively electrified over both its surfaces; the plates being joined again, it will recover part of its former excess, and will thus be brought to have then the whole of its excess equal to P G. In consequence of a new separation, a portion x G of the same excess will again be lost; and thus it will at last happen, that M N will have that precise degree of excess at which a further separation can no longer lessen it; so that H is the point at which the *vindicating* electricity begins to be altered, that is, from negative becomes positive. At a following separation, by virtue of which the remaining excess is already reduced to the less value R I, the plate, instead of continuing to lose any more of its excess, on the contrary begins to recover the portion of it I *y*. Hence, as the remaining excess from the charge, in M N, is gradually reduced to to the less values K S in K, L A in L, and o in M, the surface M N gradually recovers greater portions of its former excess, K *s*, L *z*, M *&*. From that point afterwards the surface M N, by virtue of other successive separations, will for a very long while continue to recover portions of its former excess, which (the operation being continued without touching the plates) will gradually vanish at every successive conjunction of the same.

And, thus the portions of a curve O Q M, *o q* M, will, with their respective ordinate, express the excesses and deficiencies, both primitive and remaining, of M N and *m n*; the portions of a curve *u* H *&* *v*, V H & V, will, with their ordinates, express as far as H, the negative *vindicating* electricities, and beyond H, the *positive vindicating* electricities, of the surfaces M N, *m n*. The same portions of the curve which serve to express the degrees of positive and negative vindicating electricities that take place at every successive separation of the plates, will also serve to represent the progression of the mutual adhesion of the plates. *u* F, U F will express the greatest degree of the adhesion of the plates, when they still retain their whole charge; which value will gradually lessen conformably to the successive lessening ordinates, *x* G, X G; at the instant when the negative electricity will take place, this value will be o in H, that is, at the point of the contrary inflexion; and thence it will continue quickly increasing, then very slowly decreasing, conformably to the successive ordinates, I *y*, I Y, K *s*, K S, L *z*, L Z, M *&*, M *&*, &c.

With respect to the experiments that are made on the *vindicating* electricity of a single plate A B *a b* (Pl. IX. fig. 3.) by disjoining its coating C D, they differ much in point of intensity and duration, from the experiments that are made with the two plates jointly charged. Of this difference the cause partly at least is manifest: in the separation of the two plates jointly charged, the *vindicating* electricities of the two sursurfaces which are disjoining, co-operate together; and this circumstance must increase the effects, and better preserve the efficient causes; that is, the dispositions introduced by the charge of the plates, by virtue of which they endeavour to dismiss their respective electricities to a certain degree, and beyond this degree, to recover the same.

With regard to the manner after which the same vindicating electricities exert themselves, I observe, I. That positive *vindicating* electricities exert themselves after the same manner, when only one plate is used, and separated from its coating, as when both are used, and successively separated from each other. II. Negative vindicating electricities also exert themselves after the same manner, if the charge introduced into the single plate is very weak, consisting for instance, of only two or three sparks from the first conductor; because the charge which is usually introduced into the joined plates, is likewise small, on account of the thickness of the whole. III. But if the charge introduced in the single plate be much intense, then the phenomena which result from disjoining the coating of it, while the plate retains its whole charge, are proportionably different from the phenomena which result from separating the two plates, when they only possess their *joint* charge.

That is to say, each of the plates that retain their charge, manifests in consequence of a separation, the same electricity on both its surfaces, with that of the surface which is disjoined; but the plate which has been charged alone, and possesses a considerable degree of charge, manifests that kind of electricity on the surface which is disjoined from its coatings, which is proper to that surface; and the contrary kind of electricity on the other surface. Thus, if the single plate A B *a b* be strongly charged, positively in A B, and negatively in *a b*, it will, after the coating C D is taken off, repel a white ribbon from A B, and a black ribbon from *a b*.

The reason of this is, that charges universally endeavour, with a force proportioned to their intensity, to grow gradually less; and this force counteracts the force with which they endeavour to keep their state of mutual equality, the force by which the single charged plate endeavours, when separated from its coating, to *actuate similar atmospheres in the air contiguous to its two surfaces.* When I take off the coating C D from A B *a b*, which I suppose to be strongly charged, I lessen the electricity of A B; therefore, by virtue of the force with which the two contrary electricities constantly endeavour to keep their state of equality, the deficiency in *a b* must lessen, and the excess in A B of course somewhat increase: as the electricity on both surfaces strongly endeavours at the same time to grow less in consequence of its very intensity, the deficiency in *a b* very strongly lessens by the united efficiency of the two above causes, and the excess of A B, even after the separation of its coating, will continue to decrease a little, in consequence of the lessening force, which arises from the intensity of its charge, and surpasses that which tends to an equality; thence, a certain quantity of fire flows from A B into the contiguous air; but *a b* at the same time draws fire from the air contiguous to it with very great force, and after this manner the above effects take place.

I have repeated the above observations from my above mentioned book on the *vindicating electricity*, and added some new ones, in order to throw some more light on the subject; with regard to the nature of the adhesion which accompanies *vindicating* electricities, I shall only subjoin two trials I have made about it. The first is as follows; if two plates, either charged, or lately discharged, and which therefore strongly adhere to each other, are immersed into an extensive subtle flame, or, when taken from this flame, are suspended within a large glass bell, emptied of air they soon part from each other. The other experiment is that of disjoining bodies naturally joined, for instance, strata or sheets of talc, or of *spato:* no electricity at all arises from these bare separations. With respect to the cause of the *vindicating* electricity, and of the adhesion that accompanies it, it certainly would, if discovered, throw a considerable light on the properties of insulating bodies, on the manner of their charges on the nature of electric atmospheres, and consequently on all the most striking phenomena of electricity, such as the *brush* the *star,* and the electrical motions. A consideration this which is very apt both to excite us to investigate such cause, and restrain us from barely *imagining* it.

LV.—*Synoptic View of the precise amount of pure Carbon, yielded by the rigid analysis from the Charcoals of thirty principal known Woods;* by W. F. Weekes, Esq., Surgeon. Lecturer on Philosophical and Operative Chemistry, &c, &c., Sandwich.*

Some twelve years since I was induced from circumstances arising out of engagements in the laboratory, to undertake a somewhat extensive series of experimental researches relative to gaseous, liquid and other products of numerous specimens of ligneous fibre, exotic as well as indigenous; subsequent to which course of enquiry, the *charcoals* of the respective woods were made the subject of extremly cautious analysis. From my minutes of the results then obtained, I select thirty of the principal specimens, and have brought them into a tabular view, under the impression that it is a point of some importance to the chemist and man of general science, as well as to certain manufacturers and others, to possess a source of reference upon which may be placed unqualified reliance, as respects the per centage of *pure carbon,* generally present in the charcoals from various specimens of wood; though I am aware that some few results of this description have already been given to the scientific world, by analytical chemists of no small celebrity. I shall only further observe, that the whole series of charcoals was obtained by close distillation from woods cut down in their full vigour, and afterwards gradually dried by exposure to the atmosphere. The following synopsis is arranged in the order of their purity downwards:—

---

* Communicated by the Author.

CHARCOALS.	Amount of Pure Carbon in 100 grains.
Mulberry	99,50
Chestnut	99,38
Yew	99,05
Birch	99.
Cherry	99.
Box	98,75
Maple	98,75
Sycamore	98,75
Ash	98,75
Cedar	98,75
Lime	98,75
Holly	98,10
Lignumvitæ	98,40
Willow	98,25
Beech	98,13
Pear	98,13
American Oak	97,50
Hawthorn	97,50
Laburnum	97,50
Poplar	97,50
Alder	97,25
Evergreen Oak	96,88
Plum	96,87
Mahogany	96,25
Elm	96,25
Apple	96,25
English Oak	95.
Walnut	93,75
Ebony	92,40
Lancewood	86,25

Hence it will appear that between the two extremes of the table, mulberry and lancewood, independent of variations in the intermediate series, there exists a difference in purity amounting to 13,25 grains per cent.; and it may be further worthy of remark that, notwithstanding the striking want of uniformity in the *external* character of many woods, precisely the same amount of pure carbon appears to be essential to their constitution.

LVI.—*On Tornadoes and Œrsted's Memoirs respecting them.* By Robert Hare, M. D. Professor of Chemistry, in the Pensylvanian University, Philadelphia.

TO THE EDITORS OF THE NATIONAL GAZETTE.

Dear Sirs,—I believe it is generally admitted by electricians that the enormous discharges of the electric fluid, which, during thunder gusts, take place in the form of lightning, are the consequence of the opposite electrical states of an immense stratum of the atmosphere coated by the thunder clouds, and a corresponding portion of the terrestrial surface. In a memoir published in the 5th volume of the American Philosophical Transactions, republished in Silliman's Journal, volume 32, for 1837, I had endeavoured to show that the tornado was the consequence of the same causes producing, in lieu of lightning, an electrical discharge by a vertical blast of air, and the upward motion of electrified bodies. In your Gazette of the 30th ult., you have re-published an article by the celebrated Œrsted in which it is alleged that tornadoes or waterspouts cannot be caused by electricity, because there is no evidence proving that persons exposed have experienced electrical shocks. To me it appears evident that the scientific author confounds the different processes of discharge to which I have alluded, the one occurring in thunder gusts, the other in tornadoes; also that he has forgotten that a shock can be given neither by a blast of electrified air, nor by a continuous electrical current, a transient interruption of the circuit being indispensable to the production of the slightest sensation of that nature. If a person, having a conducting communication between one of his hands and a charged surface of a well insulated battery, hold in the other hand a pointed wire, the battery will be discharged through him and through the wire, producing a blast of electrified air from the point, without his experiencing any shock; neither would a shock be given to any person by exposure to the blast thus produced.

This form of electrical discharge to which I ascribe tornadoes, in which electricity is conveyed from one surface to another by the motion of air or other moveable bodies intervening, is by Faraday designated as "*convection,*" from the Latin "conveho," to carry along with.

In the comparatively minute experiments of electricians, the process of convective discharge, is exemplified not only by the electrified aerial blast, but likewise by the play of pith balls, the dance of puppets, or the vibration of a pendulum, or bell clapper. The passage of sparks is found to arrest or to check such movements, and in like manner the passage of lightning has been observed to mitigate the vertical force of a tornado.

While a meteor of this kind, which passed over Providence last year, was crossing the river, the water, within an area of about three hundred feet in diameter, was found to rise up in a foam, as if boiling. Meanwhile two successive flashes of lightning occurring, the foam was observed to subside after each flash. It is thus proved that a discharge by lightning is inconsistent with the discharge by convection, and that so far as one ensues, the other is impeded.

In an account of a tremendous storm of the kind of which I have been treating, published in Silliman's Journal for July last, it is mentioned, that, at its commencement, it was only a violent thunder gust. This is quite consistent with the experience acquired by means of our miniature experiments, in which a discharge, by sparks, may be succeeded by a discharge by convection, or vice versa, or they may prevail alternately. In one case the electric fluid passes in the gigantic sparks called lightning, in the other it is conveyed by a blast of electrified air. In the former case animals are subjected to deleterious shocks, while in the latter no other injury is sustained than such as results from collision with the air, or other ponderable bodies.

In the case of the tornado, the vertical blast is accelerated by the difference between the pressure of the air at the earth's surface, and at the altitude to which the blast extends. Should this be a mile there would be a difference nearly of one hundred and forty-four pounds per square foot. During the tremendous gale which prevailed at Liverpool last winter, the greatest pressure of the wind was estimated at only thirty pounds per square foot. So far as the ingenious inferences and observations of Mr. Epsy, as to the buoyancy resulting from a transfer of heat from aqueous vapour to air hold good, the vertical force so alleged to arise, will co-operate to aid the influence of electric discharges by convection.

The distinguished author of the memoir alluded to at the outset of this communication, conceives that were electricity the cause of tornadoes, the magnetic needle should be disturbed by them; and without advancing any proof that such disturbance does not take place, founds thus an objection to electrical agency. I conceive that it would be unreasonable to expect a magnetic needle to be affected by an electrified blast of air, if protected from its mechanical force.

It has been shewn, by Faraday, that without peculiar management, tending to prolong the re-action, the most delicately suspended needle cannot be made to diverge in obedience to the most powerful discharges of mechanical electricity. An electrical spark may impart a feeble magnetism, but it is too rapid and transient to effect a needle. Moreover, when a needle is at right angles to an electric current, which would be quite competent to influence it, if parallel to it, there can be no consequent movement, since the current tends to keep it in that relative position. The direction of every electrical discharge, inducing a tornado, must necessarily be nearly at right angles to the needle, since it must be vertical, while the needle is necessarily horizontal, when so supported as to traverse with facility.

I do not perceive any facts or suggestions in the article by Œrsted, which are competent to render the phenomenon of which he treats more intelligible than it was rendered by the accurate survey and examination of the track of the New Brunswick tornado, by Dallas, Bache, and Espy, in connexion with accounts published by other witnesses of that and other similar meteors.

It seems to be admitted, on all sides, that within a certain space there is a rarefaction of air, tending to burst or unroof houses. That the upward blast consequent to this rarefaction, carries up all moveable bodies to a greater or less elevation; that an afflux of air ensues, from all quarters, to supply the vacuity, which the vertical current has a tendency to produce. Trees, within the rarified area, are uprooted, and sometimes carried aloft; but on either side of it, or in front, or in the rear, are prostrated in a direction almost always bearing towards a point, which during some part of the time in which the meteor has endured, has been under the axis of the column which it formed.

It appears to me that all the well authenticated characteristics enumerated by Œrsted, are referable to the view of the case thus presented. This distinguished author assumes that there is a *whirling* motion, although between American observers, this is a debated question It seems in the highest degree probable that gyration does take place occasionally, if not usually, since in the case of liquids rushing into a vacuity, a whirlpool is very apt to ensue. But as slight causes will in such cases either induce or arrest the circular motion, such movements may be contingent. It would however appear probable that when gyration does exist, it may, as the consequent generation of centrifugal force tend to promote or sustain the rarefaction, and thus contribute to augment the force, or prolong the duration of a tornado.

From observations made upon the track of the recent tornado at New Haven, I am led to surmise that there was more than one axis of gyration and vertical force—I conceive that in consequence of the diversities in the nature of the bodies or the soil, there was a more copious emission of electricity from some parts of the rarefied area than others. In two instances waggons with iron wheel tires and axles, were especially the objects of the rage of the elements. Trees equally exposed were unequally affected, some being carried aloft, while others were left standing. The area of a tornado track may be more analogons to a rough surface than a point, and the electricity may, from its well known habitudes, be given off from such bodies as are from their shape or nature most favorable to its evolution.

Since these inferences were made, I have observed in Reid's work upon Storms, that similar impressions were created by facts observed during a hurricane at Mauritius in 1824. It was remarked that narrow, tall, and decayed buildings, ready to turn into ruins escaped, at but little distance from new houses which were overturned or torn into pieces. It was inferred there were local whirlwinds, subjecting some localities to greater violence than others in the vicinity. In the case of other hurricanes similar facts have been noticed.

It may be expedient here to subjoin, that I consider a hurricane as essentially a tornado, in which an electric discharge by "*convection,*" associated with discharges in the form of lightning, takes place from a comparatively much larger surface. In the case of the hurricane, however, the area of the track is so much more extensive, that the height of the vertical column to the diameter of the base being proportionably less, there is necessarily a modification of the phenomena, which prevents the resemblance from being perceived. In the case of the hurricane, the column is too broad to come within the scope of a human eye.

So much has lately been presented to the public, either through the newspapers, journals, or lectures, which I consider demonstrably incorrect that I can hardly, consistently with my love of true science, remain an inactive observer of the consequent perversion of the public mind. Unfortunately it is difficult, if not impossible to discuss such subjects without a resort to language and ideas, which are too technical and abstruse for persons who have not made chemistry and electricity an object of study.—I have however prepared a series of essays, in which the causes of storms are stated, agreeably to my view of this important branch of meteorology.—I am, gentlemen, yours truly,

ROBERT HARE.

LVII.—*An account of a remarkable Tornado which occurred towards the last of June, at Chatenay, near Paris; being translated from the Report of a Parisian savant, Peltier, appointed to ascertain whether insurers were liable for the losses under policies against damage from thunder storms* (See Journal des Debats for the 17th of July.) *Also Remarks and Annotations by* R. HARE, M. D. Professor of Chemistry, in the Pensylvanian University, Philadelphia.

FOR THE NATIONAL GAZETTE.

Messrs. Editors:—You had published a memoir on Tornadoes by a distinguished foreigner, Œrsted. Conceiving the impression conveyed by that article less worthy of consideration than those which had been presented in a memoir which I had previously published, I hope that I shall be considered as having had a sufficient incentive for endeavouring through the same channel to correct the erroneous impressions which that memoir was in my opinion of nature to produce.

In my letter to you of the 26th ult. it was stated that I considered tornadoes as the consequence of an electrical discharge superseding the more ordinary medium of lightning. From an acticle which has since met my attention in the Journal des Debats, published on the 17th July at Paris, it appears that a tremendous tornado accurred about the last of the preceding June in the vicinity of that metropolis. The losers applied for indemnity to certain insurers, who objected to pay on the plea that the policies were against thunder storms not against tornadoes. This led to an application to the celebrated Arago, who referred the case to another savant, Peltier.

From the report of Peltier, of which I subjoin a translation, it will be seen that, excepting his neglect of co-operative influence of the elasticity of the air, he sanctions my opinion that a tornado is the effect of an electrical discharge.*

---

* I had presented copies of the pamphlet containing my memoir to M. Arago and several other members of the institute. In a subsequent conversation he referred to some of the suggestions which it contained. As it conveyed a view of the question decisively favourable to the claimants, it may be inferred that it must have been alluded to by Arago and thus have become the source of Peltier's impressions. It may therefore be anticipated that due acknowledgment will be hereafter made by him when he realises his promise of making a more elaborate report on the tornado of Chatenay. Before entering upon the arguments by which I sustained my hypothesis it was briefly stated in the following words: "*After maturely considering all the facts I am led to suggest that a tornado is the effect of an electrified current of air superseding the more usual means of discharge between the earth and clouds, in those vivid sparks which we call lightning.*"

"Yesterday," says Peltier, "I visited the commune of Chatenay in the canton of Ecouen, department of Seine and Oise, and investigated the disasters experienced in the month of June last, from a tornado which first originated over the valley of Fontenay des Louvres. At present I can give only a summary account of this wonderful phenomenon.

"Early in the morning a thunder cloud arose to the south of Chatenay, and moved at about ten o'clock over the valley between the hills of Chatenay and those of Ecouen. The cloud having extended itself over the valley, appeared stationary and about to pass away to the west. Some thunder was heard but nothing remarkable was noticed, when about mid-day a second thunder storm coming also from the south and moving with rapidity advanced towards the same plain of Chatenay. Having arrived at the extremity of the plain above Fontenay, opposite to the first mentioned thunder cloud, which occupied a higher part of the atmosphere, it stopped at a little distance, leaving spectators for some moments uncertain as to the direction which it would ultimately take. That two thunder clouds should thus keep each other at a distance, led to the impression that being charged with the same electricity, they were rendered reciprocally repellent, and that a conflict would ensue in which the terrestrial surface would play an important part. Up to this time there had been thunder continually rumbling within the second thunder cloud, when suddenly an under portion of this cloud descending and entering into communication with the earth, the thunder ceased. A prodigious attractive power was exerted forthwith, all the dust and other light bodies which covered the surface of the earth mounted towards the apex of the cone formed by the cloud. A rumbling thunder was continually heard. Small clouds wheeled about the inverted cone rising and descending with rapidity. An intelligent spectator, M. Dutour, who was admirably placed for observing, saw the column formed by the tornado terminated at its lower extremity by a cap of fire; while this was not seen by a shepherd, Oliver, who was on the very spot, but enveloped in a cloud of dust.

"To the south-east of the tornado, on the side exposed to it, the trees were shattered, while those on the other side of it preserved their sap and verdure. The portion attacked appeared to have experienced a radical change, while the rest were not affected. The tornado having descended into the valley at the extremity of Fontenay, approached some trees situated along the bed of a rivulet, which was without water though moist. After having there broken and uprooted every tree which it encountered, it crossed the valley and advanced towards some other trees, which it also destroyed. In the next place, hesitating a few moments as if uncertain as to its route, it halted immediately under the first thunder cloud. This, although previously stationary, now began as if repelled by the tornado to retreat towards the valley to the west of Chatenay. The tornado after stopping as I have described, would infallibly on its part, have moved on towards the west to a wood in that direction, if the other thunder cloud had not prevented it by its repulsion. Finally it advanced to the park of the castle of Chatenay, overthrowing every thing in its path. On entering this park, which is at the summit of hill, it desolated one of the most agreeable residences in the neighbourhood of Paris. All the finest trees were uprooted, the youngest only, which were without the tornado, having escaped. The walls were thrown down, the roofs and chimneys of the castle and farm house carried away, and branches, tiles and other moveable bodies were thrown to a distance of more than five hundred yards. Decending the hill towards the north, the tornado stopped over a pond killed the fish, overthrew the trees, withering their leaves, and proceeded slowly along an avenue of willows, the roots of which entered the water, and being during this part of its progress much diminished in size and force, it proceeded slowly over a plain, and finally at the distance of more than a thousand yards from Chatenay, divided into two parts, one of which disappeared in the clouds, the other in the ground.

" In this hasty account I have, with the intention of returning to this portion of the subject, omitted to speak particularly of it effects upon the trees. All those which came within the influence of the tornado, presented the same aspect; their sap was vaporized, and their ligneous fibres had become as dry as if kept for forty-eight hours in a furnace heated to ninety degrees above the boiling point. Evidently there was a great mass of vapour instantaneously formed, which could only make its escape by bursting the tree in every direction; and as wood has less cohesion in a horizontal longitudinal, than in a transverse direction, these trees were all, throughout one portion of their trunk, cloven into laths. Many trees attest, by their condition, that they served as conductors to continual discharges of electricity, and that the high temperature produced by this passage of the electric fluid, instantly vaporized all the moisture which they contained, and that this instantaneous vaporization burst all the trees open in the direction of their length, until the wood, dried up and split, had become unable to resist the force of the wind which accompanied the tornado. In contemplating the rise and progress of this phe nomenon, we see the conversion of an ordinary thundergust into a tornado;* we behold two masses of clouds opposed to each other, of which the upper one, in consequence of the repulsion of the similar electricities with which both are charged, repelling the lower towards the ground, the clouds of the latter descending and communicating with the earth by clouds of dust and by the trees. This communication once formed, the thunder immediately ceases, and the discharges of electricity take place by means of the clouds which have thus descended and the trees. These trees traversed by the electricity, have their temperature, in consequence, raised to such a point that their sap is vaporized, and their fibers sundered by its effort to escape. Flashes and fiery balls and sparks accompanying the tornado, a smell of sulphur remains for several days in the houses, in which the curtains are found discoloured. Every thing proves that the tornado is nothing else than a a conductor formed from the clouds, which serves for a passage for a continual discharge of electricity from those above, and that the difference between an ordinary thunder-storm and one accompanied by a tornado, consists in the presence of a conductor of clouds, which seems to maintain the combat between the upper portion of the tornado and the ground beneath. At Chatenay this conductor was formed by the influence of an upper thunder cloud, which forced the lower portion of an inferior cloud to descend and come into contact with the terrestrial surface."

Peltier concurs with me in the opinion that the tornado supersedes lightning, by affording a conducting communication between the terrestrial surface and thunder cloud: but he conceives that the cloud, by its descent, becomes the conductor, through which the electric discharge is accomplished: whereas, agreeably to the explanation which I suggested, a vertical blast of air, and every body carried aloft, contributes to form the means of communication. Agreeably to this suggestion, the electic fluid does not pass by conduction, but "convection," as explained in my letter of the 26th ult. That the idea of the parisian savan, that the cloud acts as a conductor, is untenable must be evident, since the light matter of which a cloud is constituted could not be stationary, between the earth and sky, in opposition to that upward aerial current of which the violence is proved to be sufficient to elevate not only water, but other bodies specifically much heavier than this liquid.

---

* See 5th vol. of the American Philosophical transactions, or Silliman's Journal for 1837, vol. 32, page 154.

So much of the narrative of Peltier as relates to the repulsion between the thunder clouds, is inconsistent with any other facts on record respecting tornadoes which have come within my knowledge. It should be recollected that this part of the story does not depend upon the observation of the author, and may be due to the imagination of the witnesses whom he examined. The most important part of his evidence, is that respecting the effect upon the trees, which appears to me to demonstrate that they were the medium of a tremendous electrical current.

In my memoir I noticed the injury done to the leaves of trees, and stated my conviction that "*as it was inconceivable that mechanical laceration could have thus extended itself equally among the foilage, a surmise may be warranted that the change was effected by electricity associated with the tornado.*"

LVIII.—*Description of a new Voltameter.* By MARTYN ROBERTS, Esq. In a letter to the Editor.

MY DEAR SIR.—If you think the following account of an nstrument worthy of a place in your Annals of Electricity, you are at liberty to insert it. I contrived the instrument last winter, and found it exceedingly useful in comparing the decomposing power of different electric currents. I brought it before the Royal Society of Edinburgh, where it was much approved of.

The usual way of measuring the quantity of gas developed by the poles of a galvanic battery, is by an instrument called a voltameter, of which there are many forms; but to all there is an objection, viz., the trouble, and often difficulty of refilling the tube with the liquid to be decomposed. The change I have made in the form makes it a very simple instrument, giving great facility of manipulation, which you will allow is of importance in all electrical experiments. My voltameter fig. 5. pl. IX. is a glass tube, bent like the letter U, and sunk into a wooden stand, as deep as the dotted lines in the figure. One leg *a* will contain about three cubic inches of gas, and on its length, is cut a scale dividing it into inches and tenths, cubic; on the summit of the other leg *b* is a reservoir *c* which will contain something more than three cubic inches.

About an inch above the lowest point of the curvature of the tube, and in the leg *a* two holes are bored in the glass, and in these are cemented two short pieces of No. 6 platina wire. *d. d.* The ends of these wires in the tube, must be close to each other, but must not touch. The outward ends of these wires terminate in two binding screws, *s*, *s*, for the purpose of attaching to them the wires of a battery. On the summit of the leg *a* is a stop-cock

To use the instrument, fill both legs with dilute sulphuric acid to the level of the stop-cock, or rather to zero on the scale. Shut the stop-cock, and fasten the battery wires in the binding screws: the decomposition of the water now commences, the gas rises in the leg *a* and the liquid is raised into the reservoir c and this will continue until the liquid is depressed in the leg *a* below the platina wires. The number of inches and tenths, of gas produced in a given time is marked by the scale, and gives, of course, the comparative power of the battery as usual. But now if you wish to repeat the experiment, you have only to open the stop-cock, the gas rushes out, and the apparatus is instantly ready for another trial.

I remain, my dear Sir, yours truly,

MARTYN J. ROBERTS.

LIX.—*On an Air Electrometer;* by B. W. Coward, Esq. In a Letter to the Editor. See fig. 4. Pl. IX.

Dear Sir,—The instrument consists of a glass cylinder, three inches diameter, by eight inches in length, on each end of which a brass cap is cemented air tight; passing through the upper cap, and near the edge is a glass tube B blown with a funnel-shaped end (for the purpose of exposing a greater surface,) and bent so as to leave a short parallel arm of about two inches and a half. To the long arm of this tube, a narrow graduated scale of ivory is affixed by means of fine wire. C and D are brass wires and balls placed in the centre of the caps, the upper one sliding in a collar of leather. In order to use this instrument, the tube B must be filled to about the height of two inches, with a fluid, on the surface of which in the long arm must rest a light guage made of ivory, and sliding so freely as to require very slight springs made of quill, to restrain it by thin pressure in any part of the tube.

Now it is evident if a charge be passed through the cylinder, the air in it will be displaced, and pressing down the fluid in the short arm, it will rise in the long one, and of course the guage with it, which by the springs, will be restrained at its maximum height. The gauge is represented at E.

The advantages to be derived from this construction of the instrument, I conceive to be,—

1st. The appearance is more elegant.

2nd. It is more easily affected.

3rd. There is no slopping about of a large quantity of fluid in the bottom of the cylinder.

4th. Should the tube require cleaning, or the fluid replenishing, it is easily effected.

5th. The permanent indication afforded by the guage, of height to which the fluid has risen.

LX.—*The Aurora Borealis, of September 3d*, 1839.

A very singular aurora borealis appeared at London, on the evening of the 3rd of September, 1839. It first made its appearance about a quarter before nine o'clock, and continued nearly the whole of the night. I was walking from Brixton to Peckham, between nine and ten, and kept the aurora in view the whole of the time. I first saw it when passing Brixton church, then about nine o'clock; its appearance was that of a yellowish light, at a small altitude above the northern horizon. In the course of a few minutes, a few faint straggling streams glided upwards to a considerable height; and soon afterwards several groups of brilliant streaks of red and white light shot over an immense track of the northern heavens, to nearly the zenith. Besides these streamers, there were also splendid blushes of alternate stationary and moving red and white light. The sky was partially covered with thin vapoury clouds, which had an obvious influence on the colour, and the apparent horizontal motion of the light, which light also was easily distinguished to be behind or beyond these thin clouds of vapour; and assumed a deeper tinge of redness as the vapour became more dense between it and the spectator As this was the first time of my observing this red light during the display of an aurora, I became very anxious to know its cause, for I never yet saw the electrical light in artificially attenuated air any thing like the colour of the light which I observed on this occasion. It was sometimes of a deep crimson, at other times of an almost fiery red, then pink, very light pink, next the white colour of the usual aurora, and so on for several alternate successions. And at other times the aurora would seem to reverse the order of colours, beginning with the ordinary white light, and passing through the different red tints down to the perfect crimson; and then return gradually to the ordinary white. I had several opportunities of observing these curious changes in the colour of the light before I arrived at Camberwell. Just before I entered the grove at Camberwell, then about half-past nine, the northern sky was illuminated through an immense horizontal range, with a splendid red light, but when I arrived in the church yard, about five minutes afterwards, the red light had nearly disappeared, only a small portion remaining on the northern edge of a thin fleece of vapour, at a considerable altitude above the western horizon; being replaced by several splendid groups of the usual white streamers. From this time till a little before ten the aurora languished very considerably, but about five minutes before ten it re-appeared with all its former splendour, with the exception of the red colour. This last sudden display presented many exceedingly fine groups of intense streamers which shot upwards to the zenith, and covered an immense space in the heavens, but lasted only a few minutes before they vanished and appeared to leave the night in comparative darkness. I watched the aurora till about half-past ten, but as at that time there appeared no reason for its continuance much longer, I ceased my observations. I understand, however, that the aurora re-appeared in great splendour, and continued till three o'clock next morning.

I never, before, observed an aurora borealis expand to so great a horizontal range as that which I have now partly described. Lyra and Capella were excellently situated for giving a good idea of the horizontal extent of the aurora, the former star being just within its western, and the latter just within its eastern margin. The thin vapoury clouds presently clearing away, these two conspicuous stars were afterwards noticed to be within the limits of the auroral beams. Before ten o'clock the sky had become pretty clear, and the stars shone in every part of the visible heavens. I did not observe any meteoric stars. WILLIAM STURGEON.

LXI.—*American Philosophical Society.*

Professor Bache, in behalf of Professor Alexander, of Princeton, made a verbal communication of a description of the aurora borealis, of September 3rd, 1839, as it appeared at Princeton,

At about ten or fifteen minutes past eight, P. M. an ill-defined, but considerably bright light was seen to extend for some distance above the horizon, in a direction nearly due east; it was similar, in intensity and appearance, to a lunar twilight. Soon after this, a continuous arch or zone of light was manifest, extending from the same spot to the opposite, or nearly opposite portion of the western horizon. This soon separated in two parts,* and, after a short interval, beams of light shot up from the eastern portion of the arch which were speedily multiplied in every direction around the observer, except within about thirty degrees of the *true* (or, it might be, *magnetic*) south.

---

* Two arches, it is believed, were at this time formed, and either separated throughout their entire extent, or united only near their extremities; but this my notes do not explicitly state.

A corona was soon formed, which was at first quite indistinct, and was not continuous for any great length of time, during the existence of the aurora, except at the period of its greatest brilliancy. At about twenty minutes past eight, this corona was situated in a line with, and about midway between α Aquilæ and α Lyræ. This may be considered as a very tolerable approximation to its position, though, from the apparent intersection, or, as it might almost be termed, interweaving of the beams which composed it, it was not often easy to fix upon the place of its centre with much precision, if indeed that which seemed its centre, did not really change its place; since, at times, it seemed to occupy a position very sensibly lower than that which the preceding observation would indicate.

At about half past eight, the appearance of the aurora was superb. The radiations which extended from the corona, nearly reached the horizon in every direction, with the exception of those which tended toward the southern space before mentioned, which, it is believed, was even at this time bounded by something like an arch, that was convex towards the zenith. The aurora was often party-coloured; frequently of a rose-red, especially in spots, in that portion of the sky which might be supposed to be near the plane of the dipping needle; and also about the centre of the corona. It was in the part of the heavens here described, that the arch of greatest intensity could most commonly, if not uniformly, be traced: though the crown of it frequently faded away, or became excessively faint.

Between the spots, of red light, or beams of the same tint, others were observed, which, either from the effect of the first mentioned colour, or something peculiar to themselves, appeared of a colour approaching to a bottle-green.

At times, again, when the corona was deficient, the appearance of what remained on each side of the vacant spot, was not unlike that of two immense comets; their heads some small distance asunder, and their tails turned eastward and westward.

The light of the corona, when most perfect, was quite dense, not only at the central point, but also near to what seemed to be the outer limits of its radiations, at which the tint commonly exhibited the nearest approach to white.

Two meteors or shooting stars were seen, which in both cases *appeared* to pass *between* the aurora and the eye of the observer; one nearly in the direction of the arch of greatest intensity, and the other almost perpendicular to it. The precise times of their appearance were not noted, though they fell within that period in which the phenomena already described were exhibited.

The corona formed again at nine; and, though again broken, was imperfectly visible after that time.

At half past nine, the eastern portion of the sky became tinted with intense red and green; but at half past ten, little else remained than the appearance of bright horizontal beams of white colour in the north.

If it be admitted that the centre of the aurora was precisely midway between α Aquilæ and α Lyræ, at twenty minutes past eight, its azimuth must have been 1° 14′ 42″ E. of S., and its altitude 73° 27′ 6″; the latitude of the observer being 49° 20′ 47″ N. The point thus designated, would be very nearly in the direction of the dipping needle; the dip being, by observation, 72° 47′ 6″ (72° 47. 1′) and the variation (though not accurately determined,) some 4° W. or that of the S. end of the needle, of course, the same extent to the east. The degrees of azimuth, reckoned on a parallel to the horizon at an altitude of 72° and more, being small, the deviation from the direction of the dipping needle, measured on the arc of a great circle, would be scarcely more than 1° towards the N. W.

Professor Bache stated that his own observations near Philadelphia, of the altitude of the apparent converging point of the auroral beams, at nine P. M. made it but about 69°. He had witnessed a case of the appearance of a dark spot of irregular shape, between two beams of light, which was certainly not a cloud, as the stars were not at all obscured by it, and which he supposed to be the phenomenon referred to recently by Professor Lloyd. No mottled clouds, such as usually attend the aurora, were visible during the period between nine and ten o'clock, when he had been able to observe. Professor Bache stated that he did not place much stress upon his measurements, as he had been prevented from sustained observation by indisposition. There had been, in the newspapers, an account of an auroral display visible at London, on the morning of the fourth of September, at about the same absolute time as at Princeton, according to Professor Alexander's observations. It was said to have been accompanied by a very unusual number of shooting stars, compared in one statement of the splendid display of November 13th, 1833.

LXII.—*A New Method of Illuminating Microscopic Objects.* By the REV. J.B. READE, M.A., of Caius College, Cambridge.*

In Dr. Goring's valuable memoir of the Verification of Microscopic Phenomena, it is observed, "the verification of the real nature, form, and construction, of a vast variety of objects which elude the sense of touch by their extreme minuteness, can only be made out by an attentive study of their appearances, *under a variety of methods of illumination*."† The methods of illumination at present adopted are four in number, and consist in the application of *direct* and *oblique reflected light*, and *direct* and *oblique transmitted light.*

The first two methods are applicable to opaque objects, but for the examination of transparent objects, all the methods are available. The two latter, however, it is well known, are those most commonly used.

Now, when microscopic objects, not opaque, are viewed with oblique reflected light—the flame of the candle being placed higher than the stage of the instrument, and its light condensed upon the object—it is invariably found that the maximum of condensed light which can be obtained by this method is sufficient for the full developement, of many important characters. If, again, transmitted light, either direct or oblique, be substituted for reflected light, obstacles of a still more serious nature greatly interfere with accurate investigation. Delicate tints are lost; colours naturally bright, or even brilliant, are all but absorbed; the texture and construction of objects are erroneously represented; and, in fact, nothing is seen, in many cases, but a magnified image of the object in mere black and white. Nor is this all; for besides this defective representation, the eye of the observer is always subject to much painful excitement, arising from the *intense illumination of the whole field of view.* And here, in fact, lies the great practical inconvenience of the present method; for, to take a common case—an object about 1-300th of an inch in diameter being placed in the middle of the field of view, the diameter of which is about 1-12th of an inch, and consequently being 1-625th part of the area of the field of view, the eye has to contend with 624 parts of the bright light, which are not brought to bear upon the illumination of the object. Hence, a method by which this intense glare shall be wholly removed, and that without the loss of a single effective ray, must evidently be superior to the one usually employed, in the ratio of at least 600 to 1.

---

* From Goring and Pritchard's Micrographia.

† Microscopic Cabinet, p. 183.

Being lately engaged in the examination of a few *test objects*, I happened to notice that the feathers of the *Lycæna argus*, when held above the flame of a candle, exhibited at a certain angle all their peculiar tints, and at the same time the flame was not visible to the eye. It then occurred to me, that by preserving the same angle under the microscope, the advantage of amplification would also be accompanied by the natural colours of the object. The requisite angle was readily obtained by making the axis of the microscope coincide with the line from the object to the eye, while the candle and the object retained their relative positions. The result accorded with my anticipation, and I was gratified by the exhibition of the most brilliant diamond tints, sparkling with exquisite lustre *on a jet black ground*. This new method of illuminating microscopic objects, it is at once apparent, consists in obtaining *oblique refracted light*.

On submitting a series of objects to the same illumination, I was soon convinced of the value of the discovery; and I scarcely know which to admire most—whether the very natural appearances of objects, adorned, as they invariably are, by the presence of their most delicate colouring, or the personal comfort of the observer, arising from the absence of all superfluous light. To illustrate the two methods by a reference to the telescope, it may be observed, that the discomfort of viewing spots on the sun not unaptly corresponds with the view of microscopic objects on an illuminated field; while the removal of all inconvenient and ineffective light from the field of the microscope corresponds with the clear and quiet view of stars on the dark blue vault of the firmament.

The most practicable mode of obtaining the illumination now described is to fix the object on the stage of the microscope, in the usual way, the axis of which must be inclined to the table, at about an angle of 45°, and then to place the candle about two inches below the stage, and about one or two inches to the right or left of it; but this lateral distance must be varied, according to the nature of the object and the angle of aperture of the instrument. It must be carefully borne in mind that the illumination will not be correct unless the field of view be *wholly darkened*.

To obtain this kind of illumination with facility and effect, it will be necessary to make some alterations in the construction of the instrument: as, for instance, in order to apply condensed light, the arm of the condenser must be placed in a ball-and-socket joint, or some similar contrivance must be adopted; for when it is perpendicular to the axis of the microscope, its introduction diverts the course of the rays from the candle to the stage, and not unfrequently illuminates the field of view. The mirror also cannot be made available in its present position, for this kind of illumination, because light, when reflected from it, must of necessity illuminate the field. It must therefore be fixed on an extended and jointed arm; and when so constructed, microscopic objects may be viewed even in the day-time by oblique refracted light. Again, a very remarkable microscopic effect will be produced by giving a small vertical angular motion either to the body of the instrument or to the stage, as in Goring's Engiscope. By this means, the plane of the object which, owing to the present construction, is of necessity parallel to the diameter of the object-glass, may be inclined to it at different angles; and we shall thus obtain *oblique vision* as well as *oblique illumination*. These two conditions are absolutely necessary for obtaining, in many instances, the true effect of coloured objects even with the naked eye, and the introduction of magnifying powers between the object and the eye does not render these two conditions a whit the less necessary.

The effect of this new method of illumination may be tried with advantage on various subjects of the larger kind, as cuttings of wood, scales of fish, and wings of insects.* We may also apply it, with peculiar interest, to the investigation of the elementary organs of plants; animal tissues; mosses; corallines; crystals; and the scales of insects of the orders Lepidoptera and Thysanura. In each of all these some striking and hitherto unperceived character will be developed, and the observer will rise from his pursuit with a more thorough persuasion that the Being whose word is power, and by whom his own body "is fearfully and wonderfully made," has equally exhibited the matchless efforts of His skill in the exquisite polish of an insect's joints; in the opening of a leaf; and the pencilling of a flower.—To be a theoretical atheist is impossible.

Peckham, Nov. 1836.

---

* Among the various objects which shew the superiority of this kind of illumination over transmitted light, the spiral vessels of the hyacinth and the pollen of the convolvolus major are the most decided.—A. P.

LXIII. *The reason why a small pair of plates introduced into a circle of large pairs reduces the action of the whole battery to the same standard as if it were composed of all small pairs.*

By W. H. Halse, Esq. In a Letter to the Editor.

Mr. Editor,—In the last number of "The Annals" I promised to explain my theory of the action of a small pair of plates when introduced into a circle of larger ones; as I considered that the theory commonly given as an explanation of it, was not founded on fact, viz., "that the positive electricity produced on the large plates more than that produced by the small plates, is instantly neutralized by an equal quantity of negative electricity, and that this extra quantity is continually being produced and as often neutralized."—Now it must be evident that this cannot be correct, for if it were so, double as much zinc must be dissolved from a four inch plate as there would be from a two inch plate, and twice the quantity of hydrogen gas evolved by the large copper. Experiment will prove that this is not the case, for in a compound circle consisting of twenty pairs of plates varying from one inch to eight inches in size, it will be found that the quantity of gas liberated from the coppers of the eight inch pairs will be no more than that evolved from the one inch pairs and the zinc dissolved is also equal. Although this theory is received by many persons as correct—and even by some lecturers on galvanism, for I have heard them give this explanation of it.—I am convinced that M. De la Rive did not mean it in this light, for he must undoubtedly have considered the extra productions and the extra re-compositions of the two electricities as due to chemical action on the plates beyond what was *necessary for the developement of the electric current circulating through unequal pairs;* the objection which I have advanced therefore can have no effect against his theory as he intended it to be understood, but only against that as received and advocated by those who are ignorant of his meaning. I, however, now give an explanation of its action according to my view of the subject and which I think will explain every effect.

The particles which compose the fluid contents of the battery contain both positive and negative electricity; the zinc contains both also ; but the positive of the zinc and the negative of the fluid have an appetency to unite, and the particles of oxygen and zinc unite in consequence of it; therefore the negative electricity which was combined with the positive in the particle of zinc, remains on the mass of zinc and the positive which was combined with the negative in the particle of oxygen remains in the fluid, *thus the zinc plate contains an excess of negative and the liquid an excess of positive elec-*

tricity. The negative, therefore, proceeds from the zinc and the positive from the fluid, both travelling in different directions, viz.,—the positive from the fluid in contact with the zinc towards the copper in the same cell and the negative from the mass of zinc along the connecting wire to the copper in contact with it. Having thus laid the foundation of my theory, I will suppose a battery working, in which a pair of plates half the size of the others is introduced into the circle; say three pairs of *simple* circles united, and this small pair shall be the second pair. At the commencement of its action each pair of plates produces electricity according to its size, *but only at the moment of uniting the two poles.* The negative on the small zinc is conveyed by the connecting wire to the copper of the first pair, which at the same time attracts from the liquid in which it is immersed (viz. the first cell) an exact quantity of positive to neutralize itself, but as the liquid contains twice as much positive as is sufficient for the neutralization of the negative, the liquid of the first pair therefore remains positive. The negative electricity accumulated on the zinc of the third pair being twice as much as the positive contained in the liquid in which the small copper is immersed (second cell) and which copper is connected with this zinc, only one half of it escapes from the zinc by the connecting wire to this copper, and consequently the mass of zinc in this third cell remains negative, on account of there not being a sufficient quantity of positive to neutralize it.

Now as the zinc plates are positive with respect to the fluids in which they are immersed (that is that their positive electricity has an appetency to unite with the negative of the fluid) it shews *that the action of the acid on the first and third pairs must be diminished.* because, in number one, the fluid is positive by the accumulation of that kind as before stated, and as the zinc is naturally positive, and as two positives will not unite (supposing they were equal) it is evident that the chemical action on the zinc plate of number one must be diminished; the same in number three—the fluid is naturally negative, and the zinc naturally positive, (with respect to each other) but as there is an accumulation of negative electricity on the zinc of this pair as before stated, it prevents its associating with the negative oxygen of the fluid, so that the action is also diminished on this pair, but in the cell of the small pair the action goes on uninterruptedly, because there is no accumulation of either electricity on the zinc or in the fluid, therefore it will be perceived that the accumulation of the positive in the liquid of the first pair and of negative on the zinc of the third pair ACT A SIMILAR PART, AS A REGULATOR DOES TO A STEAM ENGINE, and in consequence of which there is no more electricity produced from the large pairs (the commencement of the action excepted) than from the small pair; the greater the quantity of positive accumulated in the fluids and the more negative on the zincs, the less the amount of chemical action in these cells—It thus necessarily follows, that if this be correct, the prevailing opinion that the excess of either sort of electricity is immediately neutralized by its opposite must be incorrect, because no excess of either can be generated save in the first instance, but which is not neutralized but acts the purpose of regulating the action of the acid on the zinc—for example—supposing the large plates are ten inches square, and the small pair only one-fourth the size, and supposing that the large pair is capable of producing four times the quantity of electricity that the small pair will, then the action of the acid on one inch of the small pair will be as much as its action on four inches of the large pair or four times as much on an equal surface, that is, *that the same quantity of atoms both of oxygen and zinc will unite in the cell with the small pair, as will unite in the cell with the large pair*—the degree of action being regulated by this small pair, which has the effect for the above reasons of reducing the action of the battery to the same standard, as if it were composed of an equal number of these small plates, proving how necessary it is when we want large quantities of electricity and of a high tension, to be particular not to introduce a faulty pair into the circle. According to this theory, if there are five or ten pairs of plates increasing in size from one to ten inches, and the cells supplied with dilute acid sp. gr. 1068 (the zincs being all amalgamated) the hydrogen gas evolved from the ten inch coppers ought to be no more than that evolved by the one inch coppers in an equal time, and the quantity of zinc dissolved in each cell ought also to be equal. *Experiment will prove that such is the case.* I am well convinced that the above will be very difficult to be understood by the general reader, I therefore suggest the necessity of his placing before him a drawing of three simple circles united with each other, the centre one being half the size of the others, and I have no doubt that with a little attention—particularly to the first part of this letter,—that my theory will be fully comprehended, and that he will coincide with me in believing it to be the most plausible one of any that has as yet been advanced; still as it clashes with existing opinions—perhaps prejudices—of course I must expect the general attendants inseparable from innovators, viz. abuse and ridicule; but no matter, "*felix qui potint rerum cognoscere causas.*"

In my last letter I also stated that I had discovered a plan to increase the intensity of the shocks; I therefore now present you with the method :—

A method to increase the intensity of the Shock Apparatus.

In order to introduce the process more readily you will be pleased to place before you either a shock apparatus having two coils or else the sketch of one, (fig. 6. pl IX.) and let the two screws connected with the terminations of the primary coil be marked No. 1 P. and No 2 P. and let the two terminations of the secondary coil be marked No. 1 S. and No. 2 S., thus we shall have one side of the apparatus marked No. 1 P. and No. 1 S., and the other side, No. 2 P. and No. 2 S. I am thus particular in my explanations that I may be better understood by all your readers. Now let a wire pass from the screw No. 1 S. to No. 2 P. that they may be connected with each other. Next let a wire pass through the screw No. 1 P sufficiently long to be in contact with one pole of the battery and also in contact with one of the handles; the other handle is to communicate with No. 2 S, whilst contact with the battery is broken by means of a wire connected with No. 2 P. Thus will the shock be considerably increased and about one half the wire now used for the secondary coil may be saved.

It will at first sight appear to the operator that the primary and secondary coils are connected with each other so as to form one continuous coil, and that the increase of the shock is gained on that account; but with a little consideration it will be evident that this is not the case; it is true that one end of the primary coil is connected with one end of the secondary coil, but in order that it should be the effect of one continuous coil, contact with the battery must be broken by means of a wire connected with No. 2 S instead of No. 2 P, and if this be done, it will be found that the shock is very trifling; therefore it is clear that the increase of the shock is owing to some other cause which I think may be explained as follows :—

It is well known that a powerful shock can be obtained by the use of a primary wire alone; but, that the shock is very considerably increased by coiling round it a secondary wire; in that case however, this latter wire does not communicate either with the battery, or with the primary coil, the current giving the shock being what is termed "the induced current," and which depends on the primary wire for its production,—the shock being regulated by the power of the primary coil, and also by the length of wire forming the secondary coil, but the shock *felt* certainly all proceeds from this latter coil, and the shock which the primary coil was capable of giving, as certainly lost. Now it occurred to me that some means may be adopted to unite this latter shock with the secondary one, so that both may be brought into operation, and be made to pass through the body at the same time, and this is evidently effected by the before-mentioned means, for the induced current is obtained in an indirect manner, viz.: instead of the two extremities of this wire being in direct contact with the body as is the common method, only one end is in contact, whilst the other has to pass through the battery itself, and then comes to the body by means of the handle connected with the primary wire screw, No. 1, P; it is in this manner that the secondary current is obtained. The primary current is obtained by its passing from the battery through the screw No. 2, P, across to the screw No. 1, S, then circulating through the whole length of the secondary coil, and coming into contact with the body by the handle fastened to the screw No. 2, S, the other handle communicating with No. 1, P, and also with the battery; it will be thus perceived *that the increased effect is owing to the union of the primary current with the secondary current, and both being made to pass through the body at once.* With a sketch of a shock apparatus placed before you, I have no doubt my observations will be readily understood, for after the screws are marked and lines drawn to represent the wires, the whole will appear very plain. I have just constructed a small apparatus on this principle, having only four hundred feet of secondary wire, which is so powerful, that no one would be inclined to take the shock a second time, even with a single pair of half pint cylinders.

I remain, Mr. Editor,
Your obedient Servant,
W. H. HALSE.

Brent, near Ashburton.

LXIV.—*An Analysis of Mr. Harris's Investigation of Mr. Sturgeon's fourth Memoir.*

Dear Sir,—In my letter of 2nd December last, published in No. 22 of these *Annals*, (see page 332,) I promised to analyze your *excellent investigation* of my fourth Memoir; and now that I am about to enter on that important task, I must request your very patient attention, because, although I may have but little to do in this performance, I am desirous of doing that little well. Hence I shall not be enabled to gallop over your ***excellent investigation*** as fast as you have done over my memoir, nor shall I find time to follow your example in those ***elegant*** exultations in which you have so frequently enjoyed yourself. My business, on this occasion, will be merely that of separating and classifying, the various materials of which your *investigation* is composed, and examining how far they are concerned in the great question at issue.

You commence your "investigation" in about the usual way of introduction; and if you had not perverted the meaning of my motives for submitting my fourth memoir to the "consideration of all the *learned* scientific bodies in Europe and America," and committed some other inaccuracies, which I will presently point out, your first paragraph might have been all very well. Now, instead of my saying that my memoir merited the consideration of those *learned* bodies, I have said, pretty clearly, that it "is *the subject of marine lightning conductors* which requires such general attention and rigorous investigation":* and had I denominated those scientific bodies to which I have submitted the consideration of my memoirs, *learned*, as you have done, I should have been guilty of an unpardonable insult to all other scientific bodies; because the adjective *scientific* implies *learned;* and I have a right to consider that *all scientific* bodies are *learned bodies.* Now I will not say that your perversion of my statement was *intentional,* I shall therefore place these two items under the head *Mistake.*

There are also other *mistakes* in your first-compound paragraph; for I have nowhere said "that a metallic rod whilst transmitting a charge, is *always* productive of *powerful* lateral explosions;" nor have I anywhere said that "this effect, in the case of *a* lightning rod, is a very fearful circumstance." You must necessarily have observed, that the "fearful circumstances" which you mention are pointed out as applicable to *your own* lightning rod only; for it was that alone which I was investigating. Hence I find that your first paragraph is a compound of four pure *mistakes.*

Your 2nd paragraph is of no import, only as a connecting lit' between the first and third.

Paragraph 3 contains one *Mistake*: for I have nowhere "spoken in a slighting way" of you; but, on the contrary, I have always spoken of you in the most handsome manner. Read my memoir over again, and pay particular attention to paragraphs 215, 216, 217, and also my letter to you, dated September 12th, 1839, page 191 of this volume.

Hence you will find that, as a gentleman, I have paid every respect to you; but as to your experiments performed before the Navy Board, &c. you must necessarily permit that extent of criticism which is allowable on performances of so singular a character; and I must repeat that on this point I have done no more than to place those experiments in a proper light, in order that they may not have that *deceptive* influence on the readers of the "Annals of Electricity" as they probably had on the Navy Board, from a want of that explanation which, in my opinion, you ought *officially* to have given them.

---

* See last paragraph of my letter to you, p. 192 of this volume.

Paragraph 4, is plain enough, and is a good epitome of some of Mr. Stephen Grey's discoveries; and will, no doubt, be very useful to those readers who were not previously aware of *copper being a conductor of electricity:* and that *glass is an insulator.* Further than this I cannot perceive any utility of paragraph 4: and certainly it contains nothing applicable to my experiments. I think it may properly be placed under the head *Neutral.*

Paragraph 5, is of a somewhat curiously complicated character. It is a compound of *mistakes, exultation*, and a *desire to lead your readers astray.* In this paragraph you are attempting to confound some of my experiments with those which you have described in paragraph 6, and as they have "really nothing" to do with each other, you are leading your readers from one of the items of the main subject; and as I have never made any "*claims* to have" my memoir "considered by all the *learned* societies of Europe and America," you have, whilst exulting, tumbled into a, what? *Mistake.*

In paragraph 6, your statements are occasionally inconsistent with themselves, and even contradictory of each other. In one part you say, "as the charge accumulates on the inner surface, a corresponding quantity of electricity is forced off from the outer, and without this double effect takes place, we fail to accumulate a charge." And to *rivet* this statement firmly as a fact into our noddles; and "to render it evident" to our senses, you describe an illustrative experiment under the head (*e*). Now we have hardly had time to see this illustrative experiment, before you tell us it is all a deception; and that if we will read on through the long sentence (*f*) we shall find another *illustrative* experiment "which is sufficient to show, that the accumulated electricity is *never* exactly *balanced* between the opposite coatings, &c.," so that one of these statements is an obvious contradiction of the other. You seem to have some vague idea respecting the condition of a charged jar: but you do not appear to be aware that the electric forces on the opposite sides of a charged jar may either be *equal* or *unequal* according to circumstances. I mention this fact with no other view than that of bringing it to *your* notice as a probable interesting novelty. The remainder of article 6 is an acknowledgment that a lateral spark does take place, with an attempt to give the phenomena a different explanation to that given in my memoir: an explanation by no means new to the *partial* experiments which you have described, but perfectly inapplicable to my experiments, described in paragraph 202, of my fourth memoir, page 176 of this volume.

Paragraghs 7 and 8, are curious specimens of your mode of *reasoning* on philosophical subjects, and nothing more.

Paragraph 9 is simply an extract from my fourth memoir.

I will not take upon myself to say that, in paragraph 10, you have "laboured hard to invalidate* my experiments," but you must permit me to point out an error or two into which you have fallen in *your version* of them.

I have not said that "a spark *is* felt at every discharge," but that "a pungent spark *would be* felt" if "the knuckle were to be presented to the conducting rod, &c." Now, in consequence of this *little* mistake, (I hope it was not an intentional perversion) you have been led into a very serious error, by supposing that my experiments (I suppose you mean *results*) "could only be produced by continuing to work the machine in connexion with the jar," during the time that the discharges were made. If, instead of taking upon yourself to make such an assertion, you had condescended to *repeat* my experiments, you would soon have been convinced that there is no need of keeping the machine in motion, nor any necessity for keeping the jar in connexion with the wire conductor, in order to produce the lateral discharge which I have described.

Paragraph 11, is descriptive of experiments which have no bearing whatever on my memoir. The best epitome of them that I have seen is given by Cavallo, in the following note: "If a charged jar be insulated, and discharged with an insulating discharging rod, after the discharge both the sides of the jar, together with the discharging rod, will be found possessed of the electricity contrary to the electricity of that side of the jar which was touched last before the discharge: which shows that one side of a charged electric *may* contain a greater quantity of electricity than that, which is sufficient to balance the contrary electricity of the opposite side. This redundant electricity should be carefully considered in performing experiments of a *delicate* nature.

As, however, you may possibly wish me to notice a few items in paragraph 11, I will do so, and begin with the item *(p)*: in which you, with a very proper motive, introduce your *unit* jar. Take a friend's advice on this instrument, and never again depend on its indications as a *measurer* of *quantities* of the electric fluid, for it happens to be no measurer of either *quantity* or *intensity*. Let me endeavour to show you why. With a constant *surface* of the same coated glass, the intensity of the charge is *as the quantity deposited*, when the resistance is constant; but under *no other* circumstance. Now the resistance which a jar offers to the introduction of new or succeeding quantities, whilst charging, becomes gradually greater and greater as the charge advances, and consequently requires, from the unit jar, a continually *increasing* charge for every successive spark transmitted during the whole process of charging. Hence, you see, that *your* supposed *units* are continually varying: and *your* jar no measurer of electric action. You might have known this fact by placing the knob of a jar at a short distance from the prime conductor, where you would have seen that the sparks became less and less frequent as the charge advanced.

---

* See page 175 of this volume.

Items *(r)* and *(s)* are additional specimens of *your* mode of reasoning; and item *(t)* is not amiss. The two latter evince a *growing* propensity to *percussion* powder, but by no means any more applicable in this *investigation* than in your *illustration* of the effects of lightning on a ship's mast, by the wood-splitting apparatus. Your readers can have no doubt of your being eminently successful in the wood-splitting business after reading item *(s)*: "whereas in passing the *slightest* spark, it (the percussion powder) inflames directly;" and they must necessarily acknowledge, that nothing less than the most penetrating mind could ever have assimilated the effects of "the slightest spark," with those of a flash of lightning, on a ship's mast.

Paragraph 12, is simply an epitome of *your* views of your described experiments.

Now comes paragraph 13 with all its singular misrepresentations, misinterpretations, &c., but I will notice one item only, because it is the cleverest of the whole, and because by means of that item I shall be enabled to show you how easily you may be led into a mistake. I will premise, by acknowledging that Earl Stanhope was perfectly correct by supposing that a highly charged electric cloud would displace a portion of the earth's electric fluid; and that a *positively* charged cloud might thus cause a track of country beneath it, to become negatively electric; and so long as only *this* phenomenon occurred, your interpretation would be perfectly correct also; and there would be no need of any lightning conductor, because no lightning could possibly happen. But you know that lightning does occasionally happen: and you ought also to have known, that if a flash of lightning from the supposed cloud Fig. 6, Plate VII, were *to* strike the supposed conductor *c c*, that neither that conductor nor the object beside it, would any longer be in the *same* electric condition that they were in *prior* to the discharge. The electric fluid constituting the flash would no longer be in the cloud, but would now be in the conductor *cc*, which would become highly *electro-positive* during the transit of fluid through the metal; and would thus cause electro-displacements, and consequent electric flashes amongst all those vicinal conducting bodies which were sufficiently near to each other to be within their respective spheres of action. Hence you see that *your* reasoning only applies to the state of the clouds and objects beneath them *prior* to the flash of lightning: and at a time when no conductor is needed: but my reasoning applies to the electric condition of a conductor whilst in the *absolute capacity of transmitting the lightning.* I hope you now perceive the distinction; as to the importance of our respective views I must leave you and others to judge.

In your 14th paragraph you obviously rely a great deal more upon the opinion of other persons than upon any knowledge of your own. I, on the contrary, do no such thing. I shall always venerate Priestly as a philosopher, but I must speak on points of doctrine according to my own experience; and although that eminent electrician did not observe the electrical effects of *lateral explosions* on vicinal bodies, I, and others, have observed them. And if *you* did not know of this fact before, I should advise you to read carefully my fifth memoir, which will very soon appear in these annals; and afterwards try to repeat some of the experiments which you will there find described: and in the mean time you may read the *Library of Useful Knowledge:* and if the facts stated there should happen to be "unfortunate for your *whole* doctrine," I hope that you will not blame me for it.

Paragraph 15 declares that you have no knowledge whatever of the "radiation of electric matter from conductors carrying the primitive discharge." Here again I must crave your indulgence not to blame me for your not being acquainted with this fact. I sincerely wish you had a better practical acquaintance with electrical phenomena than that which your *investigation* indicates, as it would have saved much trouble in bringing many common place facts to your notice. Item (*v*) is akin to item (*p*); and the *air electro-thermometer* on a par with the *unit jar.* Let me give you another piece of wholesome advice. Never again trust to an *electro-measuring apparatus,* until you have first become well acquainted with its *capabilities* and the *principles* of its action.

Paragraph 16 is a neutral: and paragraph 18 is simply a short extract: and paragraph 19 may be dismissed under the same head.

Paragraph 19, though mostly quotations, is a very important paragraph, because it is made the basis of all the unelectric matter of all the succeeding paragraphs in the *investigation:* and I am very glad that I am so near the end of it, for I am heartily tired of wading through such a mass of material as your *investigation* as composed of. Now, Sir, this unfortunately mischievous word *infinitely*, has given you a great deal of unnecessary trouble, for which I am exceedingly sorry. Had I said *immensely* instead of *infinitely*, *I* should have been more correct, and you would have had less trouble.

I do not find any thing further in your *investigation* excepting some trifling unelectro-invective, which I have neither time nor inclination to notice. I sincerely wish you had been more serious and more scientific in your *investigation*, as it would then have been a pleasure to reason with you, and point out many other facts which bear upon the great question at issue.

I am, dear Sir,

Yours very truly,

WILLIAM STURGEON.

To W. Snow Harris, Esq.

ANNALS OF ELECTRICITY

VOL IV PLATE

Fig.3

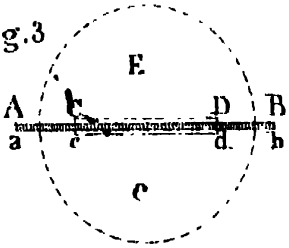


Fig.1

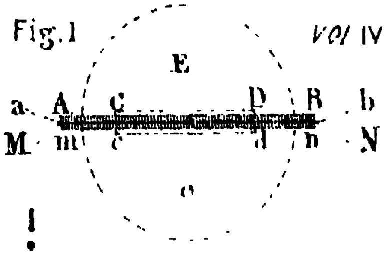


Fig.2

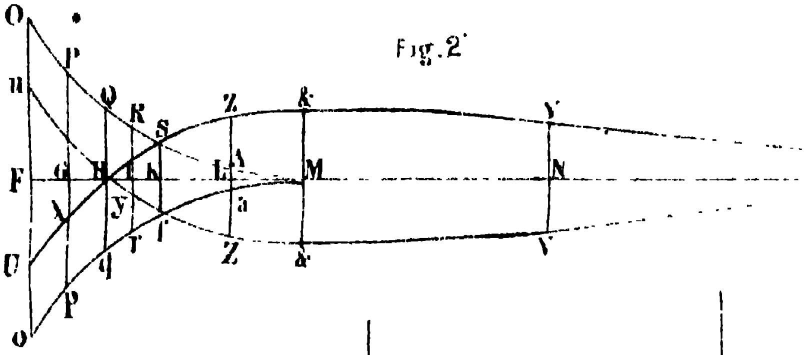


Fig. 4

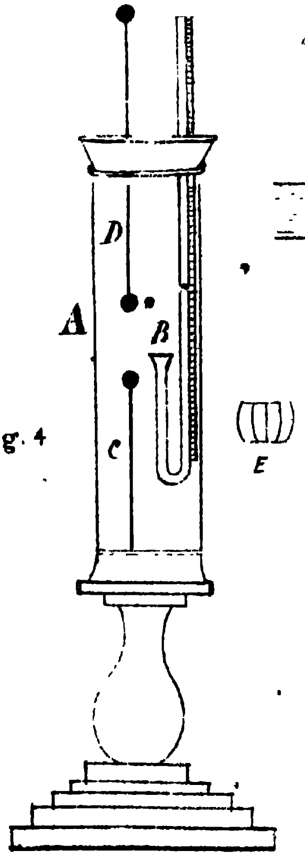


Fig 6

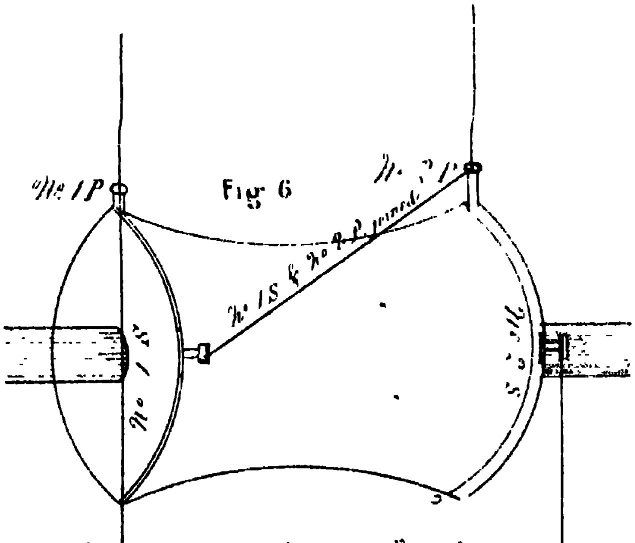
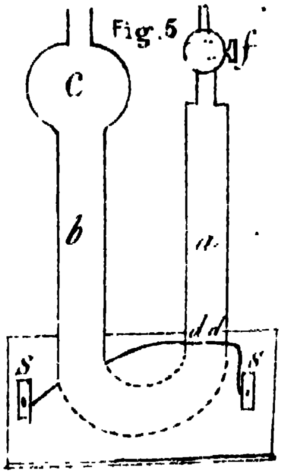


Fig. 5



TO BE

PUBLISHED WEEKLY, price 3d.,

THE

BRITISH AND FOREIGN SCIENTIFIC MAGAZINE,

AND

Journal of Scientific Inventions.

There needs no apology for introducing to the public a journal which will convey to them a fund of useful information collected from every part of the civilized world. The want of such a work in this country, is severely felt even by our scientific men; whilst the general reader is kept sadly in the rear with respect to the scientific intelligence, and the progress of science in foreign countries. The BRITISH AND FOREIGN SCIENTIFIC MAGAZINE, &c. is intended to supply this desideratum, as far as its pages will allow, by devoting a portion of each number to foreign scientific matter. It will also open a new field for British scientific enterprise and competition; and convey the intelligence of British science, arts and inventions,—manufactures and scientific productions generally, to the remotest shores of the earth. The Editor, therefore, invites every scientific man, whatever may be the nature of his profession, mechanics, and handicraftsmen of every denomination, to avail themselves of this medium, by which their discoveries, inventions, &c. may be made known to the scientific and general reader of all nations.

The work will be printed on excellent paper, with a new type cast expressly for the purpose. Each number will be accompanied with a well-executed lithographic plate, with illustrative figures of some article, or articles, which the number may contain: and enveloped in a neat cover; on which will appear, the Title of the Work, Notice to Correspondents, Advertisements, &c.

Correspondents are requested to address their Communications, (post paid) to the Editor of the BRITISH AND FOREIGN SCIENTIFIC MAGAZINE, &c., *at Sherwood and Co.'s, Paternoster Row, London, at which place Advertisements will be also received*

No. 1, will appear on Saturday, the 13th of July, 1839.

Published by Sherwood, Gilbert, and Piper, Paternoster Row; and
may be had of all Booksellers in town and country.

A. Brown, Printer, High Street, Deptford.

THE ANNALS

OF

ELECTRICITY, MAGNETISM,

AND CHEMISTRY;

AND

Guardian of Experimental Science.

JULY, 1839.

I. *Experimental Researches in Electricity.—Eleventh Series. By* Michael Faraday, *Esq., D.C.L., F.R.S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Florence, Copenhagen, Berlin, &c., &c.**

Received November 30—Read December 21, 1837.

§. 18. *On Induction.* ¶ i. *Induction an action of contiguous particles.* ¶ ii. *Absolute charge of matter.* ¶ iii. *Electrometer and inductive apparatus employed.* ¶ iv. *Induction in curved lines.* ¶ v. *Specific inductive capacity.* ¶ vi. *General results as to induction.*

¶ i. *Induction an action of contiguous particles.*

1161. The science of electricity is in that state in which every part of it requires experimental investigation; not merely for the discovery of new effects, but, what is just now of far more importance, the development of the means by which the old effects are produced, and the consequent more accurate determination of the first principles of action of the most extraordinary and universal power in nature:—and to those philosophers who pursue the inquiry zealously yet cautiously, combining experiments with analogy, suspicious of their preconceived notions, paying more respect to a fact than a theory, not too hasty to generalize, and above all things, willing at every step to cross-examine their own opinions, both by reasoning and experiment, no branch of knowledge can afford so fine and ready a field for discovery as this. Such is most abundantly shown to be the case by the progress which electricity has made in the last thirty years: Chemistry and Magnetism have successively acknowledged its overruling influence; and it is probable that every effect depending upon the powers of inorganic matter, and perhaps most of those related to vegetable and animal life, will ultimately be found subordinate to it.

---

* From the Transactions of the Royal Society.

1162. Amongst the actions of different kinds into which electricity has conventionally been subdivided, there is, I think, none which excels or even equals in importance that called *Induction*. It is of the most general influence in electrical phænomena appearing to be concerned in every one of them, and has in reality the character of a first, essential, and fundamental principle. Its comprehension is so important, that I think we cannot proceed much further in the investigation of the laws of electricity without a more thorough understanding of its nature; how otherwise can we hope to comprehend the harmony and even unity of action which doubtless governs electrical excitement by friction, by chemical means, by heat, by magnetic influence, by evaporation, and even by the living being?

1163. In the long-continued course of experimental inquiry in which I have been engaged, this general result has pressed upon me constantly, namely, the necessity of admitting two forces, or two forms or directions of a force (516. 517.), combined with the impossibility of separating these two forces (or electricities) from each other, either in the phænomena of statical electricity or those of the current. In association with this, the impossibility under any circumstances, as yet, of absolutely charging matter of any kind with one or the other electricity dwelt on my mind, and made me wish and search for a clearer view than any that I was acquainted with, of the way in which electrical powers and the particles of matter are related; especially in inductive actions upon which almost all others appeared to rest.

1164. When I discovered the general fact that electrolytes refused to yield their elements to a current when in the solid state though they gave them forth freely if in the liquid condition (380. 394. 402.), I thought I saw an opening to the elucidation of inductive action and the possible subjugation of many dissimilar phænomena to one law. For let the electrolyte be water, a plate of ice being coated with platina foil on its two surfaces, and these coatings connected with any continued source of the two electrical powers, the ice will charge like a Leyden arrangement, presenting a case of common induction, but no current will pass. If the ice be liquefied, the induction will fall to a certain degree, because a current can now pass; but its passing is dependent upon a *peculiar molecular arrangement* of the particles consistent with transfer of the elements of the the electrolyte in opposite directions, the degree of discharge and the quantity of elements evolved being exactly proportioned to each other (377. 783.). Whether the charging of the metallic coating be effected by a powerful electrical machine, a strong and large voltaic battery, or a single pair of plates, makes no difference in the principle, but only in the degree of action (360.). Common induction takes place in each case if the electrolyte be solid, or if fluid chemical action and decomposition ensue, provided opposing actions do not interfere: and it is of high importance occasionally thus to compare effects in their extreme degrees, for the purpose of enabling us to comprehend the nature of an action in its weak state, which may be only sufficiently evident to us in its stronger condition. As, therefore, in the electrolyte, *induction* appeared to be the *first* step, and *decomposition* the *second* (the power of separating these steps from each other by giving the solid or fluid condition being in our hands); as the induction was the same in its nature as that through air, glass, wax, &c. produced by any of the ordinary means; and as the whole effect in the electrolyte appeared to be an action of the particles thrown into a peculiar or polarized state, I was led to suspect that common induction itself was in all cases an *action of contiguous particles*, and that electrical action at a distance (i. e. ordinary inductive action) never occurred except through the intermediate influence of the intervening matter.

1165. The respect which I entertain towards the names of Epinus, Cavendish, Poisson, and other most eminent men, all of whose theories I believe consider induction as an action at a distance and in straight lines, long indisposed me to the view I have just stated; and though I always watched for opportunities to prove the opposite opinion, and made such experiments occasionally as seemed to bear directly on the point, as, for instance, the examination of electrolytes, solid and fluid, whilst under induction by polarized light (951, 955.), it is only of late, and by degrees, that the extreme generality of the subject has urged me still further to extend my experiments and publish my view. At present I believe ordinary induction in all cases to be an action of contiguous particles, consisting in a species of polarity, instead of being an action of either particles or masses at sensible distances: and if this be true, the distinction and establishment of such a truth must be of the greatest consequence to our further progress in the investigation of the nature of electric forces. The linked condition of electrical induction with chemical decomposition; of voltaic excitement with chemical action; the transfer of elements in an electrolyte; the original cause of excitement in all cases; the nature and relation of conduction and insulation; of the direct and lateral or transverse action constituting electricity and magnetism; with many other things more or less incomprehensible at present, would all be affected by it, and perhaps receive a full explication in their reduction under one general law.

1166. I searched for an unexceptionable test of my view not merely in the accordance of known facts with it, but in the consequences which would flow from it if true; especially in those which would not be consistent with the theory of action at a distance. Such a consequence seemed to me to present itself in the direction in which inductive action could be exerted. If in straight lines only, though not perhaps decisive, it would be against my view; if in curved lines also, that would be a natural result of the action of contiguous particles, but I think utterly incompatible with action at a distance, as assumed by the received theories, which according to every fact and analogy we are acquainted with, is always in straight lines.

1167. Again, if induction be an action of contiguous particles, and also the first step in the process of electrolyzation (1164, 949,), there seemed reason to expect some particular relation of it to the different kinds of matter through which it would be exerted, or something equivalent to a specific electric induction for different bodies, which, if it existed, would unequivocally prove the dependance of induction on the particles; and though this, in the theory of Poisson and others, has never been supposed to be the case, I was soon led to doubt the received opinion, and have taken great pains in subjecting this matter to close experimental examination.

1168. Another ever-present question on my mind has been whether electricity has an actual and independent existence as a fluid or fluids, or was a mere power of matter, like what we conceive of the attraction of gravitation. If determined either way it would be an enormous advance in our knowledge; and as having the most direct and influential bearing on my notions, I have always sought for experiments which would in any way tend to elucidate that great question. It was in attempts to prove the existence of electricity separate from matter, by giving an independent charge of either positive or negative power to some substance, and the utter failure of all such attemps, whatever substance was used or whatever means of exciting or *evolving* electricity were employed, that first drove me to look upon induction as an action of the particles of matter, each having *both* forces developed in it in exactly equal amount. It is this circumstance, in connexion with others, which makes me desirous of placing the remarks on absolute charge first, in the order of proof and argument, which I am about to adduce in favour of my view, that electric induction is an action of the contiguous particles of the insulating medium or *di-electric*.

¶ ii. *On the absolute charge of matter.*

1169. Can matter, either conducting or non-conducting, be charged with one electric force independently of the other in the least degree, either in a sensible or latent state?

1170. The beautiful experiments of Coulomb upon the equality of action of *conductors*, whatever their substance, and the residence of *all* the electricity upon their surfaces,* are sufficient, if properly viewed, to prove that *conductors cannot be bodily charged*; and as yet no means of communicating electricity to a conductor so as to relate its particles to one electricity, and not at the same time to the other in exactly equal amount, has been discovered.

1171. With regard to electric or non-conductors, the conclusion does not at first seem so clear. They may easily be electrified bodily, either by communication (1247.) or excitement; but being so charged, every case in succession, when examined, came out to be a case of induction, and not of absolute charge. Thus, glass within conductors could easily have parts not in contact with the conductor brought into an excited state; but it was always found that a portion of the inner surface of the conductor was in an opposite and equivalent state, or that another part of the glass itself was in an equally opposite state, an *inductive* charge and not an *absolute* charge having been acquired.

1172. Well-purified oil of turpentine, which I find to be an excellent liquid insulator for most purposes, was put into a metallic vessel, and being insulated, was charged, sometimes by contact of the metal with the electrical machine, and at others by a wire dipping into the fluid within; but whatever the mode of communication, no electricity of one kind was retained by the arrangement, except what appeared on the exterior surface of the metal, that portion being there only by an inductive action through the air around. When the oil of turpentine was confined in glass vessels, there were at first some appearances as if the fluid did receive an absolute charge of electricity from the charging wire, but these were quickly reduced to cases of common induction jointly through the fluid, the glass, and the surrounding air.

---

* Mémoires de l'Academie, 1786, pp. 67, 69, 72; 1787. p. 452.

1173. I carried these experiments on with air to a very great extent. I had a chamber built, being a cube of twelve feet in the side. A slight cubical wooden frame was constructed, and copper wire passed along and across it in various directions, so as to make the sides a large net-work, and then all was covered in with paper, placed in close connexion with the wires, and supplied in every direction with bands of tin-foil, that the whole might be brought into good metallic communication, and rendered a free conductor in every part. This chamber was insulated in the lecture-room of the Royal Institution; a glass tube about six feet in length was passed through its side, leaving about four feet within and two feet on the outside, and through this a wire passed from the large electrical machine (290) to the air within. By working the machine, the air within this chamber could be brought into what is considered a highly electrified state (being, in fact, the same state as that of the air of a room in which a powerful machine is in operation) and at the same time the outside of the insulated cube was everywhere strongly charged. But putting the chamber in communication with the perfect discharging train described in a former series (292.), and working the machine so as to bring the air within to its utmost degree of charge, if I quickly cut off the connexion with the machine, and at the same moment or instantly after insulated the cube, the air within had not the least power to communicate a further charge to it. If any portion of the air was electrified, as glass or other insulators may be charged (1171), it was accompanied by a corresponding opposite action *within* the cube, the whole effect being merely a case of induction. Every attempt to charge air bodily and independently with the least portion of either electricity failed.

1174. I put a delicate gold-leaf electrometer within the cube, and then charged the whole by an *outside* communication, very strongly, for some time together; but neither during the charge or after the discharge did the electrometer or air within show the least sign of electricity. I charged and discharged the whole arrangement in various ways, but in no case could I obtain the least indication of an absolute charge; or of one by induction in which the electricity of one kind had the smallest superiority in quantity over the other. I went into the cube and lived in it, and using lighted candles, electrometers, and all other tests of electrical states, I could not find the least influence upon them, or indication of anything particular given by them, though all the time the outside of the cube was powerfully charged, and large sparks and brushes were darting off from every part of its outer surface. The conclusion I have come to is, that non-conductors, as well as conductors, have never yet had an absolute and independent charge of one electricity communicated to them, and that to all appearance such a state of matter is impossible.

1175. There is another view of this question which may be taken under the supposition of the existence of an electric fluid or fluids. It may be impossible to have the one fluid or state in a free condition without its producing by induction the other, and yet possible to have cases in which an insulated portion of matter in one condition being uncharged, shall, by a change of state, evolve one electricity or the other: and though such evolved electricity might immediately induce the opposite state in its neighbourhood, yet the mere evolution of one electricity without the other in the *first instance*, would be a very important fact in the theory which assumes a fluid or fluids; these theories as I understand them assigning not the slightest reason why such an effect should not occur.

1176. But on searching for such cases I cannot find one. Evolution by friction, as is well known, gives both powers in equal proportion. So does evolution by chemical action, notwithstanding the great diversity of bodies which may be employed, and the enormous quantity of electricity which can in this manner be evolved (371. 376. 861. 868.). The more promising cases of change of state, whether by evaporation, fusion, or the reverse processes, still give both forms of the power in *equal* proportion; and the cases of splitting of mica and other crystals, the breaking of sulphur, *&c*, &c., are subject to the same limitation.

1177. As far as experiment has proceeded, it appears, therefore, impossible either to evolve or make disappear one electric force without equal and corresponding change in the other. It is also equally impossible experimentally to charge a portion of matter with one electric force independently of the other. Charge always implies *induction*, for it can in no instance be effected without; and also the presence of the *two* forms of power, equally at the moment of development and afterwards. There is no *absolute* charge of matter with one fluid; no latency of a single electricity. This though a negatively result is an exceedingly important one, being probably the consequence of a natural impossibility, which will become clear to us when we understand the true condition and theory of the electric power.

1178. The preceding considerations already point to the following conclusions: bodies cannot be charged absolutely, but only negatively, and by a principle which is the same with that of *induction*. All *charge* is sustained by induction. All phænomena of *intensity* include the principle of induction. All *excitation* is dependent on or directly related to induction. All *currents* involve previous intensity and therefore previous induction. INDUCTION appears to be the essential function both in the first development and the consequent phænomena of electricity.

¶ iii. *Electrometer and inductive apparatus employed.*

1179. Leaving for a time the further consideration of the preceding facts until they can be collated with other results bearing directly on the great question of the nature of induction, I will now describe the apparatus I have had occasion to use; and in proportion to the importance of the principles sought to be established is the necessity of doing this so clearly as to leave no doubt of the results behind.

1180. *Electrometer.* The measuring instrument I have employed has been the torsion balance electrometer of Coulomb, constructed, generally, according to his instructions,* but with certain variations and additions, which I will briefly describe. The lower part was a glass cylinder eight inches in height and eight inches in diameter; the tube for the torsion thread was seventeen inches in length. The torsion thread itself was not of metal, but glass, according to the excellent suggestion of the late Dr. Ritchie.† It was twenty inches in length, and of such tenuity that when the shell lac lever and attached ball, &c. were connected with it, they made about ten vibrations in a minute. It would bear torsion through four revolutions, or 1440°, and yet when released, return accurately to its position; probably it would have borne considerably more than this without injury. The repelled ball was of pith, gilt, and was 0·3 of an inch in diameter. The horizontal stem or lever supporting it was of shell lac, according to Coulomb's direction, the arm carrying the ball being 2·4 inches long and the other only 1·2 inches: to this was attached the vane, also described by Coulomb, which I found to answer admirably its purpose of quickly destroying vibrations. That the inductive action within the electrometer might be uniform in all positions of the repelled ball and in all states of the apparatus, two bands of tin foil, about an inch wide each, were attached to the inner surface of the glass cylinder, going entirely round it at a distance of 0·4 of an inch from each other, and at such a height that the intermediate clear surface was in the same horizontal plane with the lever and ball. These bands were connected with each other and with the earth, and, being perfect conductors, always exerted a uniform influence on the electrified balls within, which the glass surface, from its irregularity of condition at different times, I found, did not. For the purpose of keeping the air within the electrometer in a constant state as to dryness, a glass dish, of such size as to enter easily within the cylinder, had a layer of fused potash placed within it, and this being covered with a disc of fine wire gauze to render its inductive action uniform at all parts, was placed within the instrument at the bottom and left there.

---

* Mémoires de l'Academie, 1785, p. 570.

† Phil. Trans., 1830.

1181. The moveable ball used to take and measure the portion of electricity under examination, and which may be called the *repelling*, or the *carrier*, ball, was of soft alder wood, well and smoothly gilt. It was attached to a fine shell lac stem, and introduced through a hole into the electrometer according to Coulomb's method: the stem was fixed at its upper end in a block or vice, supported on three short feet: and on the surface of the glass cover above was a plate of lead with stops on it, so that when the carrier ball was adjusted in its right position, with the vice above bearing at the same time against these stops, it was perfectly easy to bring away the carrier ball and restore it to its place again very accurately, without any loss of time.

1182. It is quite necessary to attend to certain precautions respecting these balls. If of pith alone they are bad; for when very dry, that substance is so imperfect a conductor that it neither receives nor gives a charge freely, and so, after contact with a charged conductor, is liable to be in an uncertain condition. Again, it is difficult to turn pith so smoothly as to leave the ball, even when gilt, sufficiently free from irregularities of form, as to retain its charge undiminished for a considerable length of time. When therefore the balls are finally prepared and gilt they should be examined, and being electrified, unless they can hold their charge with very little diminution for a considerable time, and yet be discharged instantly and perfectly by the touch of an uninsulated conductor, they should be dismissed.

1183. It is, perhaps, unnecessary to refer to the graduation of the instrument, further than to explain how the observations were made. On a circle or ring of paper on the outside of the glass cylinder, fixed so as to cover the internal lower ring of tin foil, were marked four points corresponding to angles of 90°; four other points exactly corresponding to these points being marked on the upper ring of tin foil within. By these and the adjusted screws, on which the whole instrument stands, the glass torsion thread could be brought accurately into the centre of the instrument and of the graduations on it. From one of the four points on the exterior of the cylinder a graduation of 90° was set off, and a corresponding graduation was placed upon the upper tin foil on the opposite side of the cylinder within; and a dot being marked on that point of the surface of the repelled ball nearest to the side of the electrometer, it was easy, by observing the line which this dot made with the lines of the two graduations just referred to, to ascertain accurately the position of the ball. The upper end of the glass thread was attached, as in Coulomb's original electrometer, to an index, which had its appropriate graduated circle, upon which the degree of torsion was ultimately to be read off.

1184. After the levelling of the instrument and adjustment of the glass thread, the blocks which determine the place of the *carrier ball* are to be regulated (1181) so that, when the carrier arrangement is placed against them, the centre of the ball may be in the radius of the instrument corresponding to 0° on the lower graduation or that on the side of the electrometer, and at the same level and distance from the centre as the *repelled ball* on the suspended torsion lever. Then the torsion index is to be turned until the ball connected with it (the repelled ball) is accurately at 30°, and finally the graduated arch belonging to the torsion index is to be adjusted so as to bring 0° upon it to the index. This state of the instrument was adopted as that which gave the most direct expression of the experimental results, and in the form having fewest variable errors; the angular distance of 30° being always retained as the standard distance to which the balls were in every case to be brought, and the whole of the torsion being read off at once on the graduated circle above. Under these circumstances the distance of the balls from each other was not merely the same in degree, but their position in the instrument, and in relation to every part of it, was actually the same every time that a measurement was made; so that all irregularities arising from slight difference of form and action in the instrument and the bodies around were avoided. The only difference which could occur in the position of anything within, consisted in the deflexion of the torsion thread from a vertical position, more or less, according to the force of repulsion of the balls; but this was so slight as to cause no interfering difference in the symmetry of form within the instrument, and gave no error in the amount of torsion force indicated on the graduation above.

1185. Although the constant angular distance of 30° between the centres of the balls was adopted, and found abundantly sensible, for all ordinary purposes, yet the facility of rendering the instrument far more sensible, by diminishing this distance was at perfect command; the results at different distances being very easily compared with each other either by experiment, or, as they are inversely as the squares of the distances, by calculation.

1186. The Coulomb balance electrometer requires experience to be understood; but I think it a very valuable instrument in the hands of those who will take pains by practice and attention to learn the precautions needful in its use. Its insulating condition varies with circumstances, and should be examined before it is employed in experiments. In an ordinary and fair condition, when the balls were so electrified as to give a repulsive torsion force of 400° at the standard distance of 30° it took nearly four hours to sink to 50° at the same distance; the average loss from 400° to 300° being at the rate of 2°·7 per minute, from 300° to 200° of 1°·7 per minute, from 200° to 100° of 1°·3 per minute, and from 100° to 50° of 0°·87 per minute. As a complete measurement by the instrument may be made in much less than a minute, the amount of loss in that time is but small, and can easily be taken into account.

1187. *The inductive apparatus.*—My object was to examine inductive action carefully when taking place through different media, for which purpose it was necessary to subject these media to it in exactly similar circumstances, and in such quantities as should suffice to eliminate any variations they might present. The requisites of the apparatus to be constructed were, therefore, that the inducting surfaces of the conductors should have a constant form and state, and be at a constant distance from each other; and that either solids, or fluids, or gases might be placed and retained between these surfaces with readiness and certainty, and for any length of time.

1188. The apparatus used may be described in general terms as consisting of two metallic spheres of unequal diameter, placed, the smaller within the larger, and concentric with it; the interval between the two being the space through which the induction was to take place. A section of it is given (fig. 1, Plate I.) *a, a,* are the two halves of a brass sphere, with an air-tight joint at *b,* like that of the Magdeburg hemispheres, made perfectly flush and smooth inside so as to present no irregularity; *c* is a connecting piece by which the apparatus is joined to a good stop-cock *d,* which is itself attached either to the metallic foot *e,* or to an air pump. The aperture within the hemisphere at *f* is very small; *g* is a brass collar fitted to the upper hemisphere, through which the shell lac support of the inner ball and its stem passes; *h* is the inner ball, also of brass; it screws on to a brass stem *i*, terminated above by a brass ball B; *l*, *l* is a mass of shell lac, moulded carefully on to *i*, and serving both to support and insulate it and its balls *h*, B. The shell-lac stem *l* is fitted into the socket *g*, by a little ordinary resinous cement, more fusible than shell lac, applied at *m m* in such a way as to give sufficient strength and render the apparatus air-tight there, yet leave as much as possible of the lower part of the shell-lac stem untouched, as an insulation between the ball *h* and the surrounding sphere *a*, *a*. The ball *h* has a small aperture at *n*, so that when the apparatus is exhausted of one gas and filled with another, the ball *h* may itself also be exhausted and filled, that no variation of the gas in the interval *o* may occur during the course of an experiment.

1189. The inner ball has a diameter of 2.33 inches, and the surrounding sphere an internal diameter of 3.57 inches. Hence the width of the intervening space, through which the induction is to take place, is 0.62 of an inch; and the extent of this place or plate, i.e. the surface of a medium sphere, may be taken as twenty-seven square inches, a quantity considered as sufficiently large for the comparison of different substances. Great care was taken in finishing well the inducing surfaces of the ball *h* and sphere *a*, *a*; and no varnish or lacquer was applied to them, or to any part of the metal of the apparatus.

1190. The attachment and adjustment of the shell-lac stem was a matter requiring considerable care, especially as, in consequence of its cracking, it had frequently to be renewed. The best lac was chosen and applied to the wire *i*, so as to be in good contact with it everywhere, and in perfect continuity throughout its own mass. It was not thinner than is given by proportion in the drawing, for when less it frequently cracked within a few hours after its cooling. I think that very slow cooling or annealing improved its quality in this respect. The collar *g* was made as thin as could be, that the lac might be as large there as possible. In order that at every re-attachment of the stem to the upper hemisphere the ball *h* might have the same relative position, a gauge *p* (fig. 2) was made of wood, and this being applied to the ball and hemisphere whilst the cement at *m* was still soft, the bearings of the ball at *q q*, and the hemisphere at *r r*, were forced home, and the whole left until cold. Thus all difficulty in the adjustment of the ball in the sphere was avoided.

1191. I had occasion at first to attach the stem to the socket by other means, as a band of paper or a plugging of white silk thread; but these were very inferior to the cement, interfering much with the insulating power of the apparatus.

1192. The retentive power of this apparatus was, when in good condition, better than that of the electrometer (1186), i. e. the proportion of loss of power was less. Thus when the apparatus was electrified, and also the balls in the electrometer, to such a degree, that after the inner ball had been in contact with the top of *k* of the ball of the apparatus, it caused a repulsion indicated by 600° of torsion force, then in falling from 600° to 400° the average loss was 8°.6 per minute; from 400° to 300° the average loss was 2°.6 per minute; from 300° to 200° it was 1°.7 per minute; from 200° to 170° it was 1° per minute. This was after the apparatus had been charged for a short time; at the first instant of charging there is an apparent loss of electricity, which can only be comprehended hereafter (1207. 1250.).

1193. When the apparatus loses its insulating power suddenly, it is almost always from a crack near to or within the brass socket. These cracks are usually transverse to the stem. If they occur at the part attached by common cement to the socket, the air cannot enter, and being then as a vacua, they conduct away the electricity and lower the charge, as fast almost as if a piece of metal had been introduced there. Occasionally stems in this state, being taken out and cleared from the common cement, may, by the careful application of the heat of a spirit lamp, be so far softened and melted as to renew perfect continuity of the parts; but if that does not succeed in restoring things to a good condition, the remedy is a new shell-lac stem.

1194. The apparatus when in order could easily be exhausted of air and filled with any given gas; but when that gas was acid or alkaline, it could not properly be removed by the air-pump, and yet required to be perfectly cleared away. In such cases the apparatus was opened and cleared; and with respect to the inner ball *h*, it was washed out two or three times with distilled water introduced at the screw hole, and then being heated above 212°, air was blown through to render the interior perfectly dry.

1195. The inductive apparatus described is evidently a Leyden phial, with the advantage, however, of having the dielectric or insulating medium changed at pleasure. The balls *h* and B, with the connecting wire *i*, constitute the charged conductor, upon the surface of which all the electric force is resident by virtue of induction (1178). Now though the largest portion of this induction is between the ball *h* and the surrounding sphere *a a*, yet the wire *i* and the ball B determine a part of the induction from their surfaces towards the external surrounding conductors. Still, as all things in that respect remain the same, whilst the medium within at *o o* may be varied, any changes exhibited by the whole apparatus will in such cases depend upon the variations made in the interior; and it was these changes I was in search of, the negation or establishment of such differences being the great object of my inquiry. I considered that these differences, if they existed, would be most distinctly set forth by having two apparatus of the kind described, precisely similar in every respect; and then, different insulating media being within, to charge one and measure it, and after dividing the charge with the other, to observe what the ultimate conditions of both were. If insulating media really had any specific differences in favouring or opposing inductive action through them, such differences, I conceived, could not fail of being developed by such a process.

1196. I will wind up this description of the apparatus, and explain the precautions necessary in their use, by describing the form and order of the experiments made to prove their equality when both contained common air. In order to facilitate reference I will distinguish the two by the terms App. i. and App. ii.

1197. The electrometer is first to be adjusted and examined (1184), and the app. i. and ii. are to be perfectly discharged. A Leyden phial is to be charged to such a degree that it would give a spark of about one sixteenth or one twentieth of an inch in length between two balls of half an inch diameter; and the carrier ball of the electrometer being charged by this phial, is to be introduced into the electrometer, and the lever ball brought by the motion of the torsion index against it; the charge is thus divided between the balls, and repulsion ensues. It is useful then to bring the repelled ball to the standard distance of 30° by the motion of the torsion index, and observe the force in degrees required for this purpose; this force will in future experiments be called *repulsion of the balls.*

1198. One of the inductive apparatus, as for instance, app. i., is now to be charged from the Leyden phial, the latter being in the state it was in when used to charge the balls; the carrier ball is to be brought into contact with the top of its upper ball (*k.* fig. 1), then introduced into the electrometer, and the repulsive force (at the distance of 30°) measured. Again, the carrier should be applied to the app. i. and the measurement repeated; the apparatus i. and ii. are then to be joined, so as to *divide* the charge, and afterwards the force of each measured by the carrier ball, applied as before, and the results carefully noted. After this both i. and ii. are to be discharged; then app. ii. charged, measured, divided with app. i., and the force of each again measured and noted. If in each case the half charges of app. i. and ii. are equal, and are together equal to the whole charge before division, then it may be considered as proved that the two apparatus are precisely equal in power, and fit to be used in cases of comparison between different insulating media or *dielectrics*.

1199. But the *precautions* necessary to obtain accurate results are numerous. The apparatus i. and ii. must always be placed on a thoroughly uninsulating medium. A mahogany table, for instance, is far from satisfactory in this respect, and therefore a sheet of tin foil, connected with an extensive discharging train (292.), is what I have used. They must be so placed also as not to be too near each other, and yet equally exposed to the inductive influence of surrounding objects; and these objects, again, should not be disturbed in their position during an experiment, or else variations of induction upon the external ball B of the apparatus may occur, and so errors be introduced into the results. The carrier ball, when receiving its portion of electricity from the apparatus, should always be applied at the same part of the ball, as, for instance, the summit *k*, and always in the same way; variable induction from the vicinity of the head, hands, &c. being avoided, and the ball after contact being withdrawn upwards in a regular and constant manner.

1200. As the stem had occasionally to be changed (1190.), and the change might occasion slight variations in the position of the ball within, I made such a variation purposely, to the amount of an eighth of an inch (which is far more than ever could occur in practice), but did not find that it sensibly altered the relation of the apparatus, or its inductive condition *as a whole*. Another trial of the apparatus was made as to the effect of dampness in the air, one being filled with very dry air, and the other with air from over water. Though this produced no change in the result, except an occasional tendency to more rapid dissipation, yet the precaution was always taken when working with gases (1290.) to dry them perfectly.

1201. It is essential that the interior of the apparatus should be *perfectly* free from dust or small loose particles, for these very rapidly lower the charge and interfere on occasions when their presence and action would hardly be expected. To breathe on the interior of the apparatus and wipe it out quietly with a clean silk handkerchief, is an effectual way of removing them; but then the intrusion of other particles should be carefully guarded against, and a dusty atmosphere should for this and several other reasons be avoided.

1202. The shell lac stem requires occasionally to be well wiped, to remove, in the first instance, the film of wax and adhering matter which is upon it; and afterwards to displace dirt and dust which will gradually attach to it in the course of experiments. I have found much to depend upon this precaution, and a silk handkerchief is the best wiper.

1203. But wiping and some other circumstances tend to give a charge to the surface of the shell lac stem. This should be removed, for, if allowed to remain, it very seriously affects the degree of charge given to the carrier ball by the apparatus (1232). This condition of the stem is best observed by discharging the apparatus, applying the carrier ball to the stem, touching it with the finger, insulating and removing it, and examining whether it has received any charge (by induction) from the stem; if it has, the stem itself is in a charged state. The best method of removing the charge I have found to be, to cover the finger with a single fold of a silk handkerchief, and breathing on the stem, to wipe it immediately after with the finger, the ball B and its connected wire, &c. being at the same time *uninsulated:* the wiping place of the silk must not be changed; it then becomes sufficiently damp not to excite the stem, and is yet dry enough to leave it in a clean and excellent insulating condition. If the air be dusty, it will be found that a single charge of the apparatus will bring on an electric state of the outside of the stem, in consequence of the carrying power of the particles of dust; whereas in the morning, and in a room which has been left quiet, several experiments can be made in succession without the stem assuming the least degree of charge.

1204. Experiments should not be made by candle or lamp light except with much care, for flames have great and yet unsteady powers of affecting and dissipating electrical charges.

1205. As a final observation on the state of the apparatus, they should retain their charge well and uniformly, and alike for both, and at the same time allow of a perfect and instantaneous discharge, giving them no charge to the carrier ball, whatever part of the ball B it may be applied to (1218.).

1206. With respect to the balance electrometer all the precautions that need be mentioned, are, that the carrier ball is to be preserved during the first part of an experiment in its electrified state, the loss of electricity which would follow upon its discharge being avoided; and, that in introducing it into the electrometer through the hole in the glass plate above, care should be taken that it do not touch, or even come near to, the edge of the glass.

1207. When the whole charge in one apparatus is divided between the two, the gradual fall, apparently from dissipation, in the apparatus which has *received* the half charge is greater than in the one *originally* charged. This is due to a peculiar effect to be described hereafter (1250. 1251.), the interfering influence of which may be avoided to a great extent by going through the steps of the process regularly and quickly; therefore, after the original charge has been measured, in app. i. for instance, i. and ii. are to be symmetrically joined by their balls B, the carrier touching one of these balls at the same time; it is first to be removed, and then the apparatus separated from each other; app. ii. is next quickly to be measured by the carrier, then app. i.; lastly, ii. is to be discharged, and the discharged carrier applied to it to ascertain whether any residual effect is present (1205.), and app. i. being discharged is also to be examined in the same manner and for the same purpose.

1208. The following is an example of the division of a charge by the two apparatus, air being the dielectric in both of them. The observations are set down one under the other in the order in which they were taken, the left hand numbers representing the observations made on app. i. and the right hand numbers those on app. ii. App. i. is that which was originally charged, and after two measurements, the charge was divided with app. ii.

App. i.	App. ii.
Balls 160°	
	0°
254°	
250	
divided and instantly taken	
	122
124	
1 after being discharged.	
	2 after being discharged.

1209. Without endeavouring to allow for the loss which must have been gradually going on during the time of the experiment, let us observe the results of the numbers as they stand. As 1° remained in app. i. in an undischargeable state, 249° may be taken as the utmost amount of the transferable or divisible charge, the half of which is 124°·5. As app. ii. was free of charge in the first instance, and immediately after the division was found with 122°, this amount *at least* may be taken as what it had received. On the other hand 124° minus 1°, or 123°, may be taken as the half of the transferable charge retained by app. i. Now these do not differ much from each other, or from 124°·5, the half of the full amount of transferable charge; and when the gradual loss of charge evident in the difference between 254° and 250° of app. i. is also taken into account, there is every reason to admit the result as showing an equal division of charge, *unattended by any disappearance of power* except that due to dissipation.

1210. I will give another result, in which app. ii. was first charged, and where the residual action of that apparatus was greater than in the former case.

App. i.	App. ii.	
Balls 150°		
	152°	
	148	
divided and instantly taken		
70°		
	78	
	5	immediately after discharge.
0		immediately after discharge.

1211. The transferable charge being 148°—5°, its half is 71°·5, which is not far removed from 70°, the half charge of i.; or from 73°, the half charge of ii.: these half charges again making up the sum of 143°, or just the amount of the whole transferable charge. Considering the errors of experiment, therefore, these results may again be received as showing that the apparatus were equal in inductive capacity, or in their powers of receiving charges.

1212. The experiments were repeated with charges of negative electricity, with the same general results.

1213. That I might be sure of the sensibility and action of the apparatus, I made such a change in one as ought upon principle to increase its inductive force, i. e. I put a metallic lining into the lower hemisphere of app. i., so as to diminish the thickness of the intervening air in that part, from 0·62 to 0·435 of an inch: this lining was carefully shaped and rounded so that it should not present a sudden projection within at its edge, but a gradual transition from the reduced interval in the lower part of the sphere to the larger one in the upper.

1214. This change immediately caused app. i. to produce effects indicating that it had a greater aptness or capacity for induction than app. ii. Thus, when a transferable charge in app. ii. of 469° was divided with app. i., the former retained a charge of 225°, whilst the latter showed one of 227°, i. e. the former had lost 244° in communicating 227° to the latter: on the other hand, when app. i. had a transferable charge in it of 381° divided by contact with app. ii., it lost 181° only, whilst it gave to app. ii. as many as 194°:—the sum of the divided forces being in the first instance *less*, and in the second instance *greater* than the original undivided charge. These results are the more striking, as only one half of the interior of app. i. was modified, and they show that the instruments are capable of bringing out differences in inductive force from amongst the errors of experiment, when these differences are much less than that produced by the alteration made in the present instance.

¶ iv. *Induction in curved lines.*

1215. Amongst those results deduced from the molecular view of induction (1166.), which, being of a peculiar nature, are the best tests of the truth or error of the theory, the expected action in curved lines is, I think, the most important at present; for, if shown to take place in an unexceptionable manner, I do not see how the old theory of action at a distance and in straight lines can stand, or how the conclusion that ordinary induction is an action of contiguous particles can be resisted.

1216. There are many forms of old experiments which might be quoted as favourable to, and consistent with the view I have adopted. Such are most cases of electro-chemical decomposition, electrical brushes, auras, sparks, &c.; but as these might be considered equivocal evidence, inasmuch as they include a current and discharge (though they have long been to me indications of prior molecular action (1230.)), I endeavoured to devise such experiments for first proofs as should not include transfer, but relate altogether to the pure simple inductive action of statical electricity.

1217. It was also of importance to make these experiments in the simplest possible manner, using not more than one insulating medium or dielectric at a time, lest differences of slow conduction should produce effects which might erroneously be supposed to result from induction in curved lines. It will be unnecessary to describe the steps of the investigation minutely; I will at once proceed to the simplest mode of proving the facts, first in air and then in other insulating media.

1218. A cylinder of solid shell-lac, 0·9 of an inch in diameter and seven inches in length, was fixed upright in a wooden foot (fig. 3.): it was made concave or cupped at its upper extremity so that a brass ball or other small arrangement could stand upon it. The upper half of the stem having been excited *negatively* by friction with warm flannel, a brass ball, B, 1 inch in diameter, was placed on the top, and then the whole arrangement examined by the carrier ball and Coulomb's electrometer (1180. &c.). For this purpose the balls of the electrometer were charged *positively* to about 360°, and then the carrier being applied to various parts of the ball B, the two were uninsulated whilst in contact or in position, then insulated,* separated, and the charge of the carrier examined as to its nature and force. Its electricity was always positive, and its force at the different positions *a*, *b*, *c*, *d*, &c. (fig. 3. and 4.) observed in succession, was as follows:

at *a*	above 1000°
b it was.	149
c	270
d	512
b	130

1219. To comprehend the full force of these results, it must first be understood, that all the charges of the ball B and the carrier are charges by induction, from the action of the excited surface of the shell lac cylinder; for whatever electricity the ball B received by *communication* from the shell lac, either in the first instance or afterwards, was removed by the uninsulating contacts, only that due to induction remaining; and this is shown by the charges taken from the ball in this its uninsulated state being always positive, or of the contrary character to the electricity of the shell-lac. In the next place the charges at *a*, *c*, and *d* were of such a nature as might be expected from an inductive action in straight lines, but that obtained at *b* is *not so*: it is clearly a charge by induction, but *induction* in *a curved line*; for the carrier ball whilst applied to *b*, and after its removal to a distance of six inches or more from B, could not, in consequence of the size of B, be connected by a straight line with any part of the excited and inducing shell-lac.

1220. To suppose that the upper part of the *uninsulated* ball B, should in some way be retained in an electrified state by that portion of the surface which is in sight of the shell-lac, would be in opposition to what we know already of the subject. Electricity is retained upon the surface of conductors only by induction (1178.); and though some persons may not be prepared as yet to admit this with respect to insulated conductors all will as regards uninsulated conductors like the ball B, and to decide the matter we have only to place the carrier ball at *e* (fig. 4.), so that it shall not come in contact with B, uninsulate it by a metallic rod descending perpendicularly, insulate it, remove it, and examine its state: it will be found charged with the same kind of electricity as, and even to a higher degree (1224.) than, if it had been in contact with the summit of B.

---

* It can hardly be necessary for me to say here, that whatever general state the carrier ball acquired in any place where it was uninsulated and then insulated, it retained on removal from that place, notwithstanding that it might pass through other places, that would have given to it, if uninsulated, a different condition.

1221. To suppose, again, that induction acts in some way *through or across* the metal of the ball, is negatived by the simplest considerations; but a fact in proof will be better. If instead of the ball B a small disc of metal be used, the carrier may be charged at, or above the middle of its upper surface; but if the plate be enlarged to about 1½ or 2 inches in diameter, C (fig. 5.), then no charge will be given to the carrier at *f*, though when applied nearer to the edge at *g*, or even *above the middle* at *h*, a charge will be obtained; and this is true though the plate may be a mere thin film of gold-leaf. Hence it is clear that the induction is not *through* the metal, but through the air or dielectric, and that in curved lines.

1222. I had another arrangement, in which a wire passing downwards through the middle of the shell-lac cylinder to the earth, was connected with the ball B (fig. 6.) so as to keep it in a constantly uninsulated state. This was a very convenient form of apparatus, and the results with it were the same as those described.

1223. In another case the ball B was supported by a shell-lac stem, independently of the excited cylinder of shell-lac, and at half an inch distance from it; but the effects were the same. Then the brass ball of a charged Leyden jar was used in place of the excited shell-lac to produce induction; but this caused no alteration of the phænomena. Both positive and negative inducing charges were tried with the same general results. Finally, the arrangement was inverted in the air for the purpose of removing every possible objection to the conclusions, but they came out exactly the same.

1224. Some results obtained with a brass hemisphere instead of the ball B were exceedingly interesting. It was 1·36 of an inch in diameter, (fig. 7.), and being placed on the top of the excited shell-lac cylinder, the carrier ball was applied, as in the former experiments (1218.), at the respective positions delineated in the figure. At *i* the force was 112°, at *k* 108°, at *l* 65°, at *m* 35°; the inductive force gradually diminishing, as might have been expected, to this point.

But on raising the carrier to the position *n* the charge increased to 87°; and on raising it still higher to *o*, the charge still further increased to 105°: at a higher point still, *p*, the charge taken was smaller in amount, being 98°, and continued to diminish for more elevated positions. Here the induction fairly turned a corner. Nothing, in fact, can better show both the curved lines or courses of the inductive action, disturbed as they are from their rectilineal form by the shape, position and condition of the metallic hemisphere; and also a *lateral tension*, so to speak, of these lines on one another: all depending, as I conceive, on induction being an action of the contiguous particles of the dielectric thrown into a state of polarity and tension, and mutually related by their forces in all directions.

1225. As another proof that the whole of these actions were inductive, I may state a result which was exactly what might be expected, namely, that if uninsulating conducting matter was brought round and near to the excited shell-lac stem, then the inductive force was directed towards it, and could not be found on the top of the hemisphere. Removing this matter the lines of force resumed their former direction. The experiment affords proofs of the lateral tension of these lines, and supplies a warning to remove such matter in repeating the above investigation.

1226. After these results on curved inductive action in air I extended the experiments to other gases, using first carbonic acid and then hydrogen: the phænomena were precisely those already described. In these experiments I found that if the gases were confined in vessels they required to be very large, for whether of glass or earthenware, the conducting power of such materials is so great that the induction of the excited shell-lac cylinder towards them is as much as if they were metal; and if the vessels be small, so great a portion of the inductive force is determined towards them that the lateral tension or mutual repulsion of the lines of force before spoken of (1224.), by which their inflection is caused, is so much relieved in other directions, that no inductive charge will be given to the carrier ball in the positions *k*, *l*, *m*, *n*, *o*, *p*, (fig. 7.). A very good mode of making the experiment is to let large currents of the gases ascend or descend through the air, and carry on the experiments in these currents.

1227. These experiments were then varied by the substituiton of a liquid dielectric, namely, *oil of turpentine*, in place of air and gases. A dish of thin glass well covered with a film of shell-lac (1272.), and found by trial to insulate well, had some highly rectified oil of turpentine put into it to the depth of half an inch, and being then placed upon the top of the brass hemisphere, (fig. 7.) observations were made with the carrier ball as before (1224.). The results were the same, and the circumstance of some of the positions being within the fluid and some without, made no sensible difference.

1228. Lastly, I used a few solid dielectrics for the same purpose, and with the same results. These were shell-lac, sulphur, fused and cast borate of lead, flint glass well covered with a film of lac, and spermaceti. The following was the form of experiment with sulphur, and all were of the same kind. A square plate of the substance, two inches in extent and 0·6 of an inch in thickness, was cast with a small hole or depression in the middle of one surface to receive the carrier ball. This was placed upon the surface of the metal hemisphere (fig. 9.) arranged on the excited lac as in former cases, and observations were made at *n*, *o*, *p*, and *q*. Great care was required in these experiments to free the sulphur or other solid substance from any charge it might previously have received, This was done by breathing and wiping (1203.), and the substance being found free from all electrical excitement, was then used in the experiment; after which it was removed and again examined, to ascertain that it had received no charge, but had acted really as a dielectric. With all these precautions the results were the same; and it is thus very satisfactory to obtain the curved inductive action through *solid bodies*, as any possible effect from the translation of charged particles in fluids or gases, which some persons might imagine to be the case, is here entirely negatived.

1229. In these experiments with solid dielectrics, the degree of charge, assumed by the carrier ball at the situations *n*, *o*, *p* (fig. 9.), was decidedly greater than that given to the ball at the same places when air only intervened between it and the metal hemisphere. This effect is consistent with what will hereafter be found to be the respective relations of these bodies, as to their power of facilitating induction through them (1269. 1273. 1277.).

1230. I might quote *many* other forms of experiment, some old and some new, in which induction in curved or contorted lines takes place, but think it unnecessary after the preceding results; I shall therefore mention but two. If a conductor A, (fig. 8.) be electrified, and an uninsulated metallic ball B, or even a plate, provided the edges be not too thin, be held before it, a small electrometer at *c* or at *d*, uninsulated, will give signs of electricity, opposite in its nature to that of A, and therefore caused by induction, although the influencing and influenced bodies cannot be joined by a right line passing through the air. Or if, the electrometers being removed, a point be fixed at the back of the ball in its uninsulated state as at C, this point will become luminous and discharge the conductor A. The latter experiment is described by Nicholson,* who, however, reasons erroneously upon it. As to its introduction here, though it is a case of discharge, the discharge is preceded by induction, and that induction must be in curved lines.

1231. As argument against the received theory of induction and in favour of that which I have ventured to put forth, I cannot see how the preceding results can be avoided. The effects are clearly inductive effects produced by electricity, not in currents but in its statical state, and this induction is exerted in lines of force which, though in many experiments they may be straight, are here curved more or less according to circumstances. I use the term *line of inductive force* merely as a temporary conventional mode of expressing the direction of the power in cases of induction; and in the experiments with the hemisphere (1224.), it is curious to see how, when certain lines have terminated on the under surface and edge of the metal, those which were before lateral to them *expand and open out from each other*, some bending round and terminating their action on the upper surface of the hemisphere, and others meeting, as it were, above in their progress outwards, uniting their forces to give an increased charge in the carrier ball, at an *increased distance* from the source of power, and influencing each other so as to cause a second flexure in the contrary direction from the first one. All this appears to me to prove that the whole action is one of contiguous particles, related to each other, not merely in the lines which they may be conceived to form through the dielectric, between the inductric and the inducteous surfaces, but in other lateral directions also. It is this which gives the effect equivalent to lateral repulsion or expansion in the lines of force I have spoken of, and enables induction to turn a corner (1304.). The power, instead of being like that of gravity, which relates particles together through straight lines, whatever other particles may be between them, is more analogous to that of a series of magnetic needles, or to the condition of the particles considered as forming the whole of a straight or a curved magnet. So that in whatever way I view it, and with great suspicion of the influence of favourite notions over myself, I cannot perceive how the ordinary theory of induction can be a correct representation of that great natural principle of electrical action.

---

* Encyclopædia Britannica, vol. vi. p. 504:

1232. I have had occasion in describing the precautions necessary in the use of the inductive apparatus, to refer to one founded on induction in curved lines (1203.); and after the experiments already described, it will easily be seen how great an influence the shell-lac stem may exert upon the charge of the carrier ball when applied to the apparatus (1218.), unless that precaution be attended to.

1233. I think it expedient, next in the course of these experimental researches, to describe some effects due to *conduction*, obtained with such bodies as glass, lac, sulphur, &c., which had not been anticipated. Being understood, they will make us acquainted with certain precautions necessary in investigating the great question of specific inductive capacity.

(To be continued.)

II. *On the decomposition of water by the agency of growing plants, more particularly the Aquatic Confervæ, the Lemna, a genus of the Monœcia Diandria class, &c. &c. By* W. H. Weekes, Esq., Surgeon, Lecturer on Philosophical and Operative Chemistry, &c. &c. &c.

Since that period when the justly revered names of Priestly and Ingenhouse shed a halo of refulgence around experimental philosophy, and the former made known the result of his celebrated enquiries on the *respiration* of plants, not only botanists and vegetable physiologists, but chemical philosophers also, appear to have concurred in the general opinion, that plants absorb carbonic acid from the air under certain circumstances, and emit oxygen in return; and Dr. Ingenhouse concludes that this change occurs only during exposure to the direct rays of the sun. It is further presumed that in the *dark* an opposite effect obtains, and that carbonic acid gas is neither absorbed nor oxygen gas evolved; but on the contrary, oxygen disappears, and carbonic acid is disengaged.

I am neither prepared nor disposed to deny, that, "under certain circumstances," these conclusions do appear to be borne out and established, generally, by attentive observation and experiment; but there are likewise facts and circumstances, which I shall submit, warranting the conclusion that these results do not invariably obtain from the functional exercise of every description of plants, and which, I think, also render it worth while to enquire whether, as is generally as-

1232. I have had occasion in describing the precautions necessary in the use of the inductive apparatus, to refer to one founded on induction in curved lines (1203.); and after the experiments already described, it will easily be seen how great an influence the shell-lac stem may exert upon the charge of the carrier ball when applied to the apparatus (1218.), unless that precaution be attended to.

1233. I think it expedient, next in the course of these experimental researches, to describe some effects due to *conduction*, obtained with such bodies as glass, lac, sulphur, &c., which had not been anticipated. Being understood, they will make us acquainted with certain precautions necessary in investigating the great question of specific inductive capacity.

(To be continued.)

II. *On the decomposition of water by the agency of growing plants, more particularly the Aquatic Confervæ, the Lemna, a genus of the Monœcia Diandria class, &c. &c. By* W. H. Weekes, Esq., Surgeon, Lecturer on Philosophical and Operative Chemistry, &c. &c. &c.

Since that period when the justly revered names of Priestly and Ingenhouse shed a halo of refulgence around experimental philosophy, and the former made known the result of his celebrated enquiries on the *respiration* of plants, not only botanists and vegetable physiologists, but chemical philosophers also, appear to have concurred in the general opinion, that plants absorb carbonic acid from the air under certain circumstances, and emit oxygen in return; and Dr. Ingenhouse concludes that this change occurs only during exposure to the direct rays of the sun. It is further presumed that in the *dark* an opposite effect obtains, and that carbonic acid gas is neither absorbed nor oxygen gas evolved; but on the contrary, oxygen disappears, and carbonic acid is disengaged.

I am neither prepared nor disposed to deny, that, "under certain circumstances," these conclusions do appear to be borne out and established, generally, by attentive observation and experiment; but there are likewise facts and circumstances, which I shall submit, warranting the conclusion that these results do not invariably obtain from the functional exercise of every description of plants, and which, I think, also render it worth while to enquire whether, as is generally assumed, it be a fact that the oxygen evolved by the respiratory action in plants, is uniformly derived from the decomposition of the carbonic acid gas, as absorbed from the atmosphere, soil, &c., or from that of the more abundant source, water, in which oxygen is known to form a large proportional constituent.

A series of cautious and minutely observed experiments occupying my attention at frequent intervals during some eight or ten years past, have, I presume, authorized me to indulge in the above conclusions, and to assert that pure oxygen alone is constantly evolved, by certain plants at least, whether they be exposed to the influence of solar light alone, or subjected to the alternate changes of day and night.

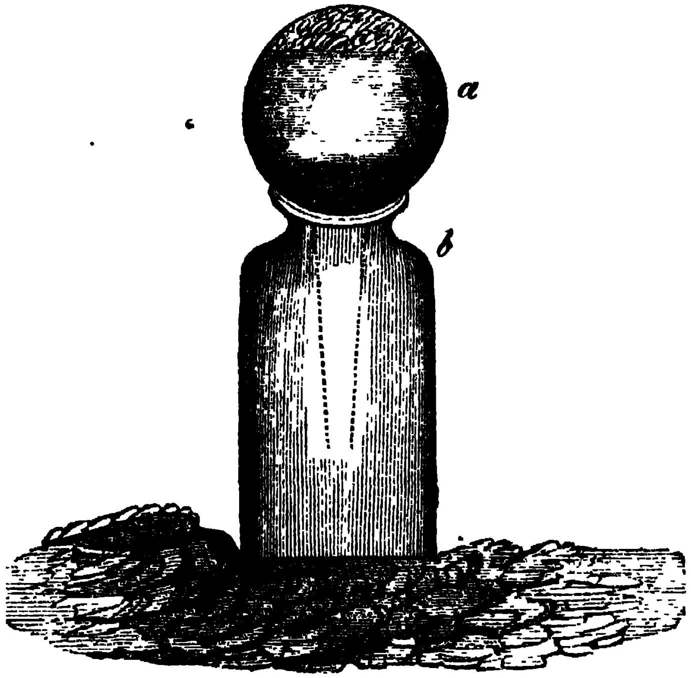
The discovery of this interesting and additional feature in the operative chemistry of nature, owes its remote origin to circumstances which I feel claim from me, at least, the tribute of a brief recital. It is now about twelve years since I had the peculiar satisfaction of acquiring the scientific acquaintance and ultimate friendship of Thomas Pine, Esq., of Maidstone, in Kent, the author of a theory appropriately denominated by him *Electro-Vegetation*, the legitimate offspring of long patient observation and inductive experiment; and which theory I can have no hesitation in believing must eventually take its place among the established truths of philosophy. Immediately upon our acquaintance Mr. Pine suggested to my management a series of experimental researches such as I might conceive best calculated to subject his opinions to the severest tests of chemical and general examination. In further relation to the theory above mentioned, it is only necessary for me in this place to observe, that the conclusions of its author were amply supported by the long series of experiments in question.

During the progress of these enquiries incidental to the Spring and Summer seasons of the years 1833-4 and 5, it became expedient for me to adopt means whereby I might bring the extreme branches of various *growing* plants and shrubs into operation under a pneumatic apparatus; sometimes employing in my manipulations merely a valve of mercury with a common atmosphere in the receiver above the fluid metal, and at others causing the branches to grow for many days and even weeks within an entire atmosphere of water, limited only by the capacity of the receiver, with a view to collect and examine the gaseous results obtained during the progress of a vigorous state of vegetation. While conducting these researches by means of the usual water trough and graduated bell glass, I often became forcibly impressed, after attentive observation, with the idea that the leaves and branches of plants, growing within my hydro-pneumatic apparatus, were materially indebted for the *large quantity* of their gaseous products to decomposition of a portion of the surrounding atmosphere of water, as well as to exhalations from the surface of their leaves, &c., originating in the decomposition of carbonic acid, though, neither then nor subsequently have I found cause to regard the generally received opinion on this subject as being devoid of foundation under ordinary circumstances and in a dry atmosphere.

Pursuing, at the period above mentioned, the train of thought thus suggested, I was led to consider the well known experiment of placing a fresh *detached* sprig of mint or other succulent plant within an inverted glass jar of water, for the purpose of exhibiting the evolution of bubbles of oxygen from the surface of its leaves exposed to the action of solar light; nor could I long hesitate in adopting, as an *opinion* at least, that the oxygen obtained in this experiment owed its origin, in no inconsiderable degree, also to decomposition of a portion of the water employed, and not entirely, as generally believed, to the carbonic acid held in solution by the fluid.

A multiplicity of engagements continued to delay my intention of endeavouring to illustrate this important question by a further appeal to experiment, until the immediate ardour arising out of the subject had somewhat abated; when, early in the Autumn of 1837, my attention thereto was strongly revived by the accidental circumstance of a decanter of river water, in which some small portion of a very minute species of *confervæ* had luxuriantly vegetated, having been left unmolested and exposed at times, during several weeks, to a strong sunlight in the window of my bed-room. I now observed that on the sides and neck of the glass innumerable bubbles of gas were collected and continuously arose from the surface of the confervæ or green vegetable matter before mentioned; and this gaseous product I had every reason, short of actual testing, to consider as oxygen derived from partial decomposition of the water in my decanter.

The season had too far advanced to permit of resuming my former researches, especially with the delicate confervæ, now the more immediate object of my attention; I therefore waited, with no small degree of impatience, the arrival of the spring and summer of 1838, with the design of subjecting my theoretical conclusions to the test of actual experiment. It was not, however, until the commencement of the month of August, that the *Punctalis*, a minute species of confervæ abounding in stagnant waters, as ponds, ditches, water-tanks, &c., appeared sufficiently luxuriant for my purpose; when the simple but completely efficient form of apparatus, represented in the annexed sketch, was immediately put in requisition for the occasion. The bolt-head *a*, holding one gallon, having been taken to a water-tank in which a sufficient quantity of the confervæ in question was discovered to have vegetated, the globular part of the glass vessel was forcibly immersed beneath the surface of the fluid, until the orifice of the neck could be brought into an appropriate position to admit of the globe filling freely, while the current of water, during its downward impetus, carried with it an ample quantity of the plant sought to be operated upon. The position of the bolt-head having been now reversed with the opening of the neck downwards, the stoneware jar *b*, charged also with the water of the tank, was plunged perpendicularly underneath, and when the neck of the glass had been immersed within the water of the jar, until the inferior circumference of the globular part rested upon the substantial rim of the lower vessel, the two were carefully removed and placed immediately in an eligible situation in my garden, subject to frequent observation. I do not think I can convey to those whom it may possibly interest, a better idea of the subsequent progress of my experiment, than by subjoining occasional extracts from my daily Journal of Memoranda, or rough notes, made on the spot at the moment of observation.



August 12th, 1838.—At 8 A. M. placed a quantity of the Confervæ Punctalis, in rain water, under the pneumatic apparatus in garden—evening bright with light breezes from S. W. Since being at rest the whole of the minute plant has resumed its natural tendency to the surface of the fluid, and occupies the zenith portion of the glass globe. Appearance perfectly healthy.

13th, eight o'clock, A. M.—A gas has been abundantly evolved, *during the past night*. The confervæ has been in consequence depressed from its original position in the upper hemisphere of the globe, its former place being now occupied by the gaseous product, fully equal in amount to the bulk of plant; I presume not less than from sixteen to eighteen cubic inches. The water of the jar has overflown in a corresponding degree.

18th.—The formation of gaseous matter from confervæ punctalis has been almost regularly progressive during the last five days, and now occupies nearly one-sixth of the globular hemisphere. Appearances indicate that the evolution of gas is on the decline; the plant also shows symptoms of decreasing vigour.

21st.—Gas has not materially augmented to-day; and I conclude that the confervæ has nearly ceased to vegetate.

Four o'clock, P. M.—Resolved to transfer the gas collected from glass globe to a series of air jars of different capacities, for the purpose of chemical examination.—Six o'clock, P. M. Gas generated within the space of nine days, amounts to fifty-eight cubic inches, and proves to be oxygen more pure than usually obtained artificially. An extinguished wax taper is instantaneously relighted by being plunged therein, and phosphorus, iron wire, and other combustibles, burn with great brilliancy. A cubic inch tube, graduated in hundredths, charged with the gas and placed over pure liquor potassa.

22d. evening.—The volume of gas in cubic inch tube over liquor potassa (temperature considered) has diminished only one and a half per cent in about twenty-four hours, and has remained without decrease during the greater part of that time; consequently, the oxygen thus obtained, holds in admixture a smaller proportion of carbonic acid than that generally procured by exposing to a red heat the peroxide of manganese.

These experiments with the confervæ punctalis were several times repeated during the months of August and September, and invariably with similar results, as witnessed by divers scientific friends who obligingly interested themselves on the occasion. Subsequently the *lemna*, a genus of the monœcia diandria class, (commonly denominated ***duck-weed***) was often subjected to the same process of examination, with the only difference that it did not produce oxygen quite so abundantly as the confervæ punctalis, though the gas obtained from the lemna was found to be in a trifling degree more pure than that evolved by the former description of plant, inasmuch as it was proved by chemical analysis to contain only *one* per cent of carbonic acid.

In the early part of the month of October, I commenced a series of similar trials with several larger species of aquatic plants, but, owing to removal from their native habitudes at a late period of the season, my efforts were not attended with a like degree of success. From these latter experiments, however, I learned that it is only in the *perfectly healthy and vigorous state* that plants possess the power to decompose water and liberate its oxygen. Under certain circumstances, which require further researches to define, I am convinced that some plants evolve a portion of nitrogen.

As connected with and arising out of the subject of this paper, I shall permit myself to subjoin a few desultory observations. It has been ingeniously suggested to me by a highly esteemed scientific friend, that the oxygen obtained during the experiments above detailed, might possibly arise from the decomposition of a considerable portion of carbonic acid, not unfrequently held in solution by certain waters. Now, the water employed in my experiments, as first stated, was taken from a large open tank on the premises, and it is quite fair to say that few specimens could be furnished in greater purity. The substances known generally to be held in solution by rain water, are air, carbonic acid, carbonate of lime, and, according to Bergman, occasionally some traces of nitric acid and a little muriate of lime. The best authorities agree that the quantity of air in good water, of the kind in question, does not exceed one-twenty-eighth of the bulk; and that one hundred cubic inches contain generally about one cubic inch of carbonic acid gas, but I have satisfied myself that the rain water actually employed, yielded rather short of the usual assumption. If we reckon the bolt-head to contain one gallon, or about 278 cubic inches, which will be a sufficient approximation to the fact, we shall perceive that the full amount of carbonic acid in the water of my experiment could not exceed three cubic inches, a quantity quite inadequate to furnish on decomposition even a sixth part of the oxygen evolved during the first night, and the possibility of the water acquiring any addition of carbonic acid by absorption from the surrounding atmosphere, was effectually provided against in the construction of the apparatus. Nor does it appear that the common air contained in the water used, had been expelled and thus augmented the volume of gas ultimately measured, because the only deviation from the pure oxygen found on analysis, was from one to one and a half per cent of carbonic acid. If we suppose the plant capable of decomposing atmospheric air, a considerable quantity of nitrogen must have been manifest on examination.

Sir H. Davy, at page 192, fourth edition of his "Last Days of a Philosopher," says "those fishes that spawn in Spring or the beginning of Summer, and which inhabit deep and still waters, as the carp, bream, pike, tench, &c., deposit their eggs upon aquatic vegetables, which, by the influence of the solar light, constantly preserve the water in a state of aëration." Though the form of expression used by our celebrated philosopher is not definitive, I think I may safely assert that the means employed by nature to effect the important object alluded to by Davy, that of preserving the water in a state of aëration, consists in the power of growing plants to decompose that fluid and supply a vivifying principle to the eggs by the disengagement of oxygen. Upon similar grounds, I presume, we might fairly conclude, that the baneful influence of malaria arising from the stagnant waters of marshy districts, is, during the spring and summer materially modified by the oxygen (emphatically characterized as *vital* air by Dr. Priestly) generated from the action of confervæ and other aquatic vegetables, abundantly inhabiting the still waters of such localities. In the season of autumn, when the vigorous action of vegetation has ceased, and the plants themselves in many instances pass into decomposition, experience shows that the demon malaria begins to diffuse its most pestiferous exhalations.

It being obvious, from the experiments above recorded, that the leaves of plants are furnished with organs suited to the office of decomposing water, and as we find only one of the elements of this fluid set at liberty, it follows logically that the other element, *viz.* the hydrogen, is absorbed by the plant and adapted to the purposes of the vegetable economy; at least, I presume that I have brought sufficient evidence to show, that in addition to the offices of the roots, leaves, "*common*" and "*proper vessels*," hitherto known, nature has provided plants with another important source of action, by the *direct* exercise of which they derive from one of the elements of water, a principal constituent* of their own, while from the disengagement of the other, they silently administer to the purity of the atmosphere and the economy of *animal* life. I have not unfrequently spent many hours, aided by the microscope, in watching, particularly in bright days, the evolution of gas bubbles as they are formed and disengaged from small aquatic vegetables, as well as from the detached leaves of other plants immersed in water; and, as one of the fruits gathered from such observation, I imagine I shall not risk any very serious condemnation, in venturing to conjecture that the spinous or downy points presented by the superfices of leaves (and I find it is to *these points* that the bubbles of gas are invariably attracted) are analogous to so many galvanic poles, rendered more or less potent by the agency of solar light and other circumstances; thus, however minute and trivial in their individual operation, producing by their infinitude an amazing aggregate of electro-chemical action; and though, doubtless, this be most conspicuous where exercised in the stagnant pool, or meandering rivulet, yet, nevertheless, extending its natural magic equally to decompose the beautiful leaflet gem exhibited in the spangling dew-drop.

---

* Some ten or twelve years since, while engaged in the analysis of upwards of forty specimens of indigenous and foreign woods, by

the process of close distillation, I became forcibly struck with the proportionately large volume of hydrogen frequently evolved in combination with carbon, &c.; so abundant, indeed, that I was induced to convey it into a temporary reservoir, and occasionally appropriated it as a means of illumination in my laboratory operations. If, during the growth of plants, this quantity of hydrogen be not materially derived from the decomposition of water, by a direct exercise of their external functions, it will be extremely difficult to account for its origin and presence as a component of ligneous fibre; for, though we are not yet accurately familiarized with the *internal* organization of vegetables, and their consequent capabilities, it seems scarcely probable that to this single and somewhat limited source alone, the whole of the hydrogen is attributable which we find resulting on careful analysis.

III. *Experimental and Theoretical Researches in Electricity. Second Memoir. By* WILLIAM STURGEON, Lecturer on Experimental Philosophy at the Hon. East India Company's Military Seminary, Addiscombe, &c.*

Read March 3d, and December 19th, 1838.

On the Identity or Non-identity of Electricity and Magnetism—Different opinions of Philosophers on this topic—Experimental Examination of those Phenomena which are supposed to favour the hypothesis—Examination of M. Ampere's Hypothesis—The polar forces of hard steel Magnets unvanquishable by Electric Currents—The inefficiency of Electric Currents in magnetizing hard steel to a high degree of power—The distribution of magnetic force exhibited by Steel Magnets and by Loadstone, not imitable by Electric Currents.

88. In the first memoir which I had the honour to present to this Society, I endeavoured to elucidate those fundamental principles of electricity, which appear obviously developed by an extensive series of illustrative phenomena, and well calculated to afford an easy explanation of the nature and peculiarity of electric action. There still, however, remains one very important theoretical point on which I have not yet touched; a point which is yet wavering under the dominion of vacillating opinion, without any party venturing a demonstration of his peculiar ideas: or, indeed, showing much, if any, reason for entertaining them.

89. The discovery of the identity of lightning and ordinary electric discharges, by Franklin, and the well established facts of lightning depolarizing compass needles, reversing the polarity of others, and producing other remarkable magnetic phenomena, were events that have, long ago, led philosophers to imagine that electricity and magnetism are not distinct powers of nature: but that, more probably, they emanate, in different forms, from one and the same physical cause. The apparent similarity of the attractions and repulsions in magnetism and electricity, has also been considered as favourable to the hypothesis.

90. It is now more than half a century ago since the celebrated Father Beccaria ventured an opinion, that the electrical and magnetic powers are identical. "Are not these peculiar effects of the electric fire with respect to magnetism,"

---

* From the Transactions of the London Electrical Society.

said this eminent philosopher, "so many proofs which corroborate my former conjectures, that the peculiar magnetic force observed in *loadstone* is to be attributed to either atmospherical or subterraneous strokes of lightning; and that the *universal systematic* properties of magnetic bodies are produced by an universal systematic circulation of the electric element?"* This hypothesis of the illustrious Italian was not much attended to, till the discovery of electro-magnetism, which happened nearly fifty years afterwards; when it was again broached, as a new idea, by M. Ampere. Since that time the hypothesis has gained many proselytes, though there be still some philosophers who do not entertain that opinion and as electricity has latterly produced many phenomena, whose true cause can only be understood by a proper solution of the problem which this disputed point has created, a strict investigation of the various circumstances connected with it can hardly fail to be interesting to the Electrical Society: I have therefore devoted the whole of this memoir to that particularly important subject, in which, it will be found, I have collected, examined, and arranged the most striking instances of analogy in electricity and magnetism: and have also pointed out many phenomena in which they as obviously disagree. I have contemplated the whole as profoundly as I have been able, and have discussed the various topics as I have proceeded, with freedom and candour, in the manner following:—

91. If one of the poles of each of two magnets be presented to each other, a tendency either to recede from, or approach each other is immediately manifested, accordingly as these poles are similar or dissimilar respectively; and because similar and dissimilar electrized bodies evince corresponding tendencies to move *from* or *towards* each other, the two sets of phenomena have been regarded as marking a strong analogy, and have been held forth as evidence in favour of the identity of the magnetic and electric agents. But, before these, or any other supposed analogies be permitted to enter into any code of physical laws, they ought to be examined with the most rigid scrutiny and exactness. The phenomena ought not only to be compared with each other, but each individual event should be traced, as closely as circumstances will permit, to the nearest cause of its production; and in what manner it would be affected by varying the conditions of the experiment: and, in the question before us, it is only from such close investigations as these, that data are to be obtained which can be esteemed of much intrinsic value.

---

* Treatise on Artificial Electricity. By Father Giambatista Beccaria, p. 310, English edition, London, 1776.

92. In contemplating the phenomena I have been speaking of in the manner proposed, let it be supposed that *ns s'n'*, fig. 1, Plate II, are two magnetic needles, each suspended by a fine thread; and that *p* and *n*, fig. 2, are two dissimilarly electrized balls, suspended in a like manner. Then, because of the magnetic poles *ns' n's*, which are opposite to each other, being of different kinds, they will approach each other until they come into contact: and a parallel phenomenon will be exhibited by the dissimilarly electrized balls, *p*. *n*. Thus far the analogy appears to hold good. Our conclusions, however, are not to be drawn from these facts alone, for the motions already performed are the mere preliminaries to the display of other phenomena which demand still greater attention, and reveal the operation of other attributes than those which brought the bodies together. The electric balls, *p*, *n*, very shortly after the first contact, separate from each other; and if their first electric conditions were of equal degrees *above* and *below* the common standard, or neutral state, they would *neutralize* each other's action, and their fibres of suspension would hang parallel to each other. But if their first electric conditions were not of equal degrees above and below the natural standard, both balls would remain either *positively* or *negatively* electrical, accordingly as *p* or *n* exhibited the greater degree of electric tension prior to the first contact. In either case the balls would display a tendency to recede from each other, and diverge their fibres of support.

93. Now the motions last exhibited by the electric balls find no parallel phenomena in the magnetic poles *ns' ns'*, fig. 1, which still cling together without evincing the least tendency to separate: instead of which, it is a well-known fact, that the longer those poles are permitted to remain unmolested the greater degree of force would be required to separate them. Hence, then, without entering into any theoretical disquisition, these electric and magnetic phenomena are so obviously dissimilar, that instead of being susceptible of inferences in favour of an identity in the operating causes, they have an obvious tendency to bias the mind to the very opposite conclusion.

94. Let the two electric balls, *p*, *n*, fig. 3, be suspended on the opposite sides of a fixed ball B, which is in the natural electric condition. The electric bodies *p* and *n* will immediately approach B; and after contact with that body they will recede from it. When the body B is insulated, and the bodies *p* and *n* differ in degree of electric tension *above* and *below* the natural standard respectively, all the three bodies remain electrized after contact: and *p* and *n* exhibit a ten-

dency to recede from B. If, on the other hand, *p* and *n* are of equal degrees of electric tension *above* and *below* the natural standard, they will neutralize each other through the medium of B; and B also will remain neutral. If the body B were uninsulated, it would be a matter of no consequence in what manner *p* and *n* were electrized, they would both become neutralized by contact with that body. Here then we have three conditions under which the electric balls, *p* and *n*, would approach B by electric action; but in no case would they be retained in contact with that body. In every variation of these experiments the bodies, *p* and *n*, would have their electric energies considerably deteriorated by contact with B; and in some cases those energies would totally vanish by such contact, however powerfully they might previously have been displayed.

95. Let now a parallel experiment be made in magnetics, by suspending two light bar-magnets by threads as represented by fig. 4. When the inferior dissimilar poles *n s'* hang on the opposite sides of a soft iron ball *i*, as in the figure, they immediately approach that ball; and when they have once come into contact with it they remain attached to it; and the longer they are left undisturbed the greater is their tendency to remain there: so that the contact, instead of diminishing the attractive force, absolutely increases it. How very different are these events to those which occur by electric action. In every case of contact by magnetic attraction, the forces which bring the bodies together, become exalted in some proportion to the closeness of contact: and in no case are those forces impaired by *time*. The electric attractive forces, on the contrary, are invariably, and immediately impaired by the bodies touching one another. In some cases they are suddenly and totally neutralized; and in no instance are they of long duration independently of a continuous exciting process.

96. Electro-polarization (52,) has an apparent analogy in magnetism, but the different ways in which the experiments may be varied, lead to results which show an obvious difference in the causes producing them. The nearest responsive fact is the polarization of soft iron by placing it in the vicinity of a permanent magnetic pole. If, for instance, the piece of soft iron *s'*, *n'*, fig. 5, be placed near to the magnetic pole *s*, of the steel bar *s*, *n*, a magnetic polarity will immediately be displayed in the iron bar: and arranged as indicated by the letters, viz. the south pole *s* of the magnet *n*, *s*, will cause a north pole in the vicinal extremity *n'*, and a south pole in the remote extremity *s'* of the iron bar: but if the north pole of the magnet be presented to the soft iron as represented by

fig. 6, the order of polarity in the iron will be the reverse of that in the former instance: though still in accordance with the same law: for in both cases the poles in the permanent magnet occasion poles of the opposite kind to be exhibited in the nearest extremity of the iron: and polarity of the *same* kind in the remote extremities of the iron.

97. The circumstances under which the magnetic polarity thus displayed by pieces of soft iron bears so strong a resemblance to those necessary to the production of electro-polarity (62, figs. 89 and 90, Pl. XII, Vol. 2,) that a superficial observer might easily be led to imagine that the same agency was in operation in both cases: but here, as in the cases already described (92, 93, 94, 95,), a close investigation of these phenomena, and a correct view of those which a variation of the circumstances productive of them exhibit, lead to very different inferences. Let us, for instance, permit the pieces of soft iron, as in figs. 5 and 6, to touch the permanent magnetic poles to which they are presented. The steel and iron would remain as decidedly polar as before: and the remote poles s' and n' of the two pieces of iron, and n and s of the steel bars would display still stronger polar forces than prior to the contact. These facts have no parallel in electricity: for if the electric bodies P and N, figs. 89 and 90, Pl. XII, Vol. 2, be brought into contact with the bodies n, p, and p, n, to which they are respectively presented, the phenomena of polarity cease to be exhibited: each pair of bodies immediately becomes similarly electric throughout; the one pair, fig. 89, being all in an electro-positive condition, and the pair, fig. 90, being in an electro-negative condition, on every part of their surfaces.

98. The electric phenomena displayed by bringing the bodies P, and n, p, fig. 89; and N, and p, n, fig. 90, are easily explained by supposing an introgression of fluid *from* the relatively positive to the relatively negative bodies of each pair: but it would be exceedingly difficult to understand how the magnetic bodies maintained their polarity by any *similar* distribution of a fluid, or of any other physical agent, for whatever may be the nature of the magnetic agent, it is obviously more determinedly fixed or accumulated in the extremities of ferruginous bars by close contact, than when those bodies are at an appeciable distance from one another. Hence we discover that the magnetic and electric forces, which, at certain distances, effect such a similarity of phenomena in bodies situated in their respective localities, are productive of no corresponding facts when the approximation of those bodies is sufficiently close. Neither do the phenomena agree which the newly magnetized and electrized bodies

exhibit after they have quitted those original magnetic and electric bodies whereon the respective disturbing forces reside; for, after the separation of *n*, *p*, and *p*, *n*, figs. 89 and 90, Plate XII, Vol. 2, from P and N respectively, the former would exhibit *positive* and the latter *negative* electric action: but the pieces of iron, figs. 5 and 6, Plate II, would lose all traces of magnetic action, when once they were sufficiently removed from the localities of the magnets to which they had been attached.

99. If it can be imagined that by substituting steel for the pieces of soft iron in figs. 5 and 6, Plate II, an analogy to the phenomena exhibited by the electrized bodies would have been more apparent, by the steel retaining magnetic action after quitting the disturbing magnetic poles, I would observe that, its retaining some trace of magnetic action is a fact which cannot be denied: but in that case the steel would remain polar, as is always the case with magnetic bodies: and as no trace of polarity would be exhibited by the electric bodies, but on the contrary, an uniformity of electric action would be discoverable over every part of their respective surfaces, the *supposed* analogy again loses its support, and as decidedly fails in this instance as in those previously discussed. Moreover, the pieces of steel would retain their polarity unimpaired, even after long continued contact with other bodies; whereas the electric bodies would lose all trace of electric action by the slightest touch with uninsulated conductors.

100. A globe of steel may be made to exhibit *permanent* magnetic polarity when far removed from every disturbing force: but the same globe will not maintain any corresponding electric action. A plate of glass will exhibit electro-polarity, on its opposite surfaces, for some considerable time after it has been removed from the exciting apparatus: but magnetic polarity is not known to be exhibited by glass. If then the magnetic and electric elements be identical, why this capricious selection of bodies for the display of these parallel phenomena? The electric forces will attract all kinds of matter without exception; but the magnetic forces appear to be exceedingly select in this particular; operating on particular kinds only. Coated glass, whatever may be its form, affords no *permanent* electric attractions, which are, in the least, comparable with the attractions exhibited by magnetic bodies: for if a metallic arc connect the two sides of a Leyden jar, the electric forces immediately disappear; but an iron arc connecting the poles of a horse-shoe magnet is permanently held there, unless removed by mechanical violence; and the longer it remains undisturbed by extrinsic force, the more vigorously is it

attracted by the poles; and there is no known substance whatever, by which the poles of a magnet may be connected, that will, in the least, deteriorate their powers.

101. Those few kinds of elementary matter on which magnetic attractions are known to be exerted, display no distinction of respect for the *north* or *south* polar forces, being attracted indiscriminately, and to the same extent, by both. Very different indeed are the nice discriminations of the *positive* and *negative* electric forces manifested in an almost endless variety of phenomena, every one of which teems with interest in the contemplations of the philosopher, and beautifully characterizes the agency of their production. If, for instance, an intimate mixture of sulphur and red lead be indiscriminately projected through the air to a series of *positively* and *negatively* electrized surfaces, the powders will be separated from each other by the dissimilar electric forces, into whose spheres of action they are thrown; and the sulphur and red lead will respectively be found at the positive and negative surfaces, exhibiting a peculiarity of arrangement not known to be accomplished by any other kind of physical agency.* Similar selections are uniformly exhibited by electric forces, whenever the particles of compounds on which they operate are sufficiently voluble to be put into motion by them, or are held together by inferior powers. Every individual electro-chemical decomposition appears to be an instance of this kind of action, and demonstrates the peculiarity of this important fact.

102. It has been said by M. Œrsted, that the only difference in the electric and magnetic forces rests in their different degrees of tension or activity; the electric being the more active or vigorous in its operations: and this hypothesis has been attempted to be supported by M. Ampere and other philosophers, whose opinions on this subject will long command respect. But I must confess that I can discern no satisfactory discrimination of this kind, nor am I acquainted with any facts that are even in the least favourable to it. It is well known that electric attractions are the most powerful when the bodies exhibiting them manifest the greatest degree of tension in the display of all other electric phenomena. The spark, for instance, is shown to the best advantage when the

---

* This fact was first shown by Leightenberg. Cavallo and Bennet, especially the latter philosopher, have extended the original experiments of Leightenberg, and varied them in a variety of pleasing and interesting ways.—*Bennet's New Experiments on Electricity. Derby*, 1789.

electric body, whence it proceed, exhibits the greatest degree of attraction: and the charge of a jar is accomplished in the shortest period of time, and with the greatest degree of facility, under similar circumstances. Moreover, when electric discharges are performed, either from a single jar, or from a battery of jars, the striking distance is greatest, the flash is the most brilliant, the noise is the loudest, the physiological effects are the most powerful, and, in fact, every phenomenon is exhibited under the most advantageous circumstances, and in the most perfect manner, when the jar, or battery, is in the most suitable condition for a display of its attractive energies.

103. But now let us enquire into the *extent* to which electric attractions are usually exhibited. Has any electrician ever seen a prime conductor, (which always shows attraction more powerfully than any other electric apparatus) support, by its electric energies alone, a single *ounce* of any kind of matter? I presume not. If, then, with this insignificant attracting force, electricity be prepared for a display of some of its most splendid and terrific phenomena—the production of vivid light, intense heat, the noise of thunder, and the destruction of animal life: and that magnetism proceeds from the same cause or agency, it seems natural to ask, why it is that similar phenomena are not exhibited to the same, or even a greater extent, by a magnetized body whose attractions are ten thousand times ten thousand greater than any ever witnessed in electricity? These important questions, which stand so prominently and essentially in the path of investigation, demand the most profound contemplation of the philosopher, and must not be passed over in silence by those who are endeavouring to identify the electric and magnetic powers. We have yet to learn the mode of producing a *magnetic spark*, and are totally ignorant of the sensation communicated by a *magnetic shock*. And *magnetic chemistry* is so profoundly obscured from our knowledge, that no one knows even of its existence.

104. If our reasoning be permitted to rest on facts alone, independently of favourite notions and ingenious hypotheses, which are but too apt to captivate the imagination of the superficial observer, and, sometimes, even to sap the understanding of the more studious in science, the obvious contrasts in the phenomena presented by electricity and magnetism enforce themselves upon our notice too powerfully to be misunderstood. Even the attractions, themselves, in which *alone* the appearance of analogy exists, are so exceedingly dissimilar, so truly distinct from one another, that their peculiar characteristics are well defined and easily discernible, and

cannot be mistaken by those who devote to them a proper and sufficient degree of attention.

105. An insulated electrized globular body *radiates* its attracting influence on every side alike, when surrounded by an uniform medium, such as the atmospheric air, as may be understood by fig. 7, Pl. 2, which may represent a great circle of the globe with its radiating electric force. But a magnetized globe, similarly situated in space, exhibits no such radial influence; for being polar on opposite points (*n. s.* fig. 8,) of its surface, the greatest *disposable** attracting forces are exerted about those polar regions, and especially in the line of their axis continued. At right angles to that axis, in the plane of the equator, *e e*, the polar forces, by their mutual attractions, nearly balance one another; neither of them exhibit-

---

* It appears by the distribution of iron-filings, when strewed on paper, above a bar magnet, that a considerable portion of the *north* and *south* forces are engaged in attracting one another, as shown by the curve lines assumed by the filings; and, consequently, are not employed, or, at least, very sparingly so, in any attractions which the magnet exercises on foreign bodies, such as pieces of soft iron, magnetic needles, &c., placed a few inches distant from its extremities and in a line with its axis; or, indeed, opposite to any other part of its surface; and, although much more of the magnetic force is brought into play as the iron is brought nearer, and most of all when it is in contact with the pole of the magnet, there is still a considerable portion of force which cannot be exerted on this foreign body, because of its being engaged with the opposite force, about the surface of the steel, which lies between its extremities; and especially that which is situated near to its centre. For convenience then, I call that portion of the magnetic force which lies about the equatorial part, the *engaged force;* and that which is brought into play on foreign bodies, the *disposable force.*

The *disposable force* of any magnet may be diverted from its original directions of action by the approximation of ferruginous bodies; and, in some instances, nearly the whole of it may be drawn from a body on which it operates, without moving either the magnet or the body. To illustrate this point, let a bar magnet be placed six or eight inches distant from the pivot of the needle, and at right angles to its direction. The *disposable* force of the magnet will deflect the needle to some considerable number of degrees. Now place on each side of the magnet, parallel to it, and about three inches distant from it, a piece of soft iron, about its own shape and size. The deflection of the needle will lessen considerably, showing that a portion of the *disposable* force has been diverted from its action on the needle. Now, bring the pieces of iron nearer to the magnet, and the deflection again decreases; and when the pieces of iron are brought into close contact with the magnet, one on each ing much *disposable* influence on exterior bodies. Another great characteristic distinction in the display of the electric and magnetic forces by these bodies appears to be this;—the electric force is wholly *disposable* and ready to be exerted upon, and even *transferred* to, other vicinal bodies: whereas the magnetic forces are neither *transferable* nor wholly *disposable*, for no magnet has yet been known to have its power impaired by contact with unmagnetized bodies, and in no case is the whole of its attracting power exerted upon a vicinal body.

106. I have been exceedingly anxious to discover, if possible, some facts which might afford analogies whereon to fix a basis of reasoning on the identity of these physical agents; but, although I have met with some further phenomena, far from being uninteresting in the discussion, a close examination of their true character has shown their evidence in favour of the supposed identity to be of no more value than that afforded by the facts already noticed.

107. If there be one electric apparatus more than another, whose action resembles the action of the magnet, it is the dry *electric column*, whose polar forces are more uniformly and permanently exhibited than those of any other electrical instrument. But the attractive and repulsive powers of this instrument, like those in all other electrical arrangements, are exceedingly feeble when compared with the gigantic powers of a magnet; they are, moreover, directed towards, and operate upon, every kind of matter without distinction, whereas the magnetic attractions and repulsions, notwithstanding their vigorous action on ferruginous bodies, are, with the exception of one or two of the metals, perfectly inert on all other kinds of matter. The attractions and repulsions of the electric column are productive of vibratory motions in pendulous bodies properly situated between the poles; which

side, from end to end, nearly the whole of the *disposable* force will be exerted on the iron, and but very little of it, if any, will reach the needle so as to cause a perceptible deflection. Now, in this case, the extremities of the magnet are still untouched by the iron, and are, consequently, as much exposed to the needle as when the iron was not present; notwithstanding which, it is obvious from the experiment, that the *disposable* force which before deflected the needle has now taken another direction, and is employed in polarizing the pieces of soft iron. The disposable force of the magnet, however, although it cannot now reach the needle with a sufficient degree of formidableness to accomplish deflection, is not entirely engaged by the iron, a residuum still remaining, which is detected by bringing the needle nearer to the magnet.

show that the vibrating body changes its electric condition at every contact with either pole of the instrument, and accommodates itself to the attractive influence of the opposite pole. When the pendulous body has come into contact with the positive pole, it acquires an electro-positive condition, and is repelled to the negative pole, where it deposits its charge and becomes electro-negative. It is now again under the attractive influence of the positive pole, to which it is compelled to make another journey, and *from* which it receives a new charge and an immediate succeeding repellent impulse, which again directs it to the negative pole; and in this manner the suspended body performs its vibratory motions, being in an electro-positive condition whilst travelling in one direction, and in an electro-negative condition whilst travelling in the other. By these means a *pulsatory current* permeates the pile from the negative to the positive pole, the fluid being transported through the air, from the latter to the former by means of the pendulous body.*

108. Besides the pendulous motions already alluded to, the dry electric column is productive of physiological and chemical phenomena, will emit sparks and charge coated glass and other inferior conductors, as decidedly as charges are produced by the machine: all of which are so perfectly distinct from, so decidedly foreign to, any known capabilities of the magnet, that there is not to be found one solitary trace of analogy in the performance of the two kinds of apparatus. The attractions and repulsions are the only phenomena in which there is a *shadow* of resemblance, whilst in *reality* even this faint analogy has obviously no special existence. The delicate electric forces which alternate the conditions of, and give vibratory motions to, the pendulous body, find no similarity of action in the majestic attractive forces of the magnet, which select those of their own species only; whose coeval polar affinities mutually exalt the action, and constrain the attracted body to assume a determinate polar condition, and prevent its escape from the vigorous influence of the pole to which it is first attached. Hence as no vacillancy in the magnetic condition of the attracted body is produced, the grand essential to vibratory motion has no existence in magnetics: nor can any such locomotions, as those exhibited by

---

* As this discussion requires experimental facts rather than theoretical opinions, I have not, in this place, entered on the doctrine of the dry electric column. It is possible I may have occasion at some other time, to enter fully into the philosophy of this interesting apparatus.

the electric column, be produced by any known self-acting powers of the magnet.

109. If we are to look for the supposed identity of electricity and magnetism amongst electro-magnetic phenomena, we are still as far from arriving at satisfactory conclusions as in any other branch of the science. It is true, we here find some of the most striking and interesting affinities which electricity and magnetism have hitherto developed; affinities which will ever link these sciences together in the firmest bonds of physical union, though by no means identifying the elements by which the phenomena are produced. Each elemental agent plays its own part in the production of electro-magnetic phenomena as decidedly as in those of magnetic electricity, whose display is accomplished by the reciprocal excitement.

110. From the attractions and repulsions exhibited by wires carrying electric currents, M. Ampere was led to imagine that all magnets owe their influence to an unremitting circulation of the electric fluid; an hypothesis so exceedingly ingenious, and so eminently calculated to favour the expectations of some philosophers, that there can be no astonishment excited by its gaining proselytes amongst those whose minds were already predisposed for its reception. But, notwithstanding the respect which is due to the talents of those philosophers who have favoured Ampere's views on this topic, I must candidly confess that the hypothesis has always appeared to me to be much easier to acknowledge than to understand. In the present investigation I have considered experimental facts as the only data on which I can proceed with any chance of success of arriving at a close approximation to true theoretical inferences. I have, therefore, neither ventured an opinion of my own, nor permitted the views of others to influence the inquiry.

111. The imaginary electric currents to which Ampere refers all magnetic action, lead us to enquire into the character and situation of their source, and by what means they can be supposed to be *perpetually* and equably maintained, either on the surface, or within the body, of a steel bar. Here it is that we are led to enumerate and examine all the known artificial sources of electric excitement, and endeavour to trace their influence to the operations of permanent steel magnets. Independently of *magnetic* excitation, we know of only three sources of electric currents, viz. frictional, voltaic, and thermal: for besides these four, there are no other sources known:* hence if a bar of steel which exhibits *permanent*

---

* The dry electric column is here omitted.

magnetism has that power conferred upon it by the influence of electric currents, which must necessarily be as durable as the magnetic action itself, to which of these sources are we to look for the *supposed* actuating currents? Or are there other sources of electric currents of which we are yet entirely ignorant? But, from whatever source those imaginary currents may be supposed to proceed, that source must necessarily be situated either on the surface, or within the body, of the steel. The idea of electric currents being excited by *friction* amongst the particles of the solid metal, is too absurd to be entertained for a moment: and the conditions necessarily required for the production of *voltaic* currents, are no where to be found in the steel: hence our enquiries are necessarily limited to *thermal* excitation alone.

112. That thermo-electric currents are producible in every piece of metal, whether pure or compound, is a fact which I have proved by very extensive experiments, some years ago.* But it must be understood that to produce an electric current by any means whatever, requires a co-existent motion in some of the elements employed during the whole time the current is flowing: unless it be of a momentary duration only, and the effect of an impulse, in which case the current may continue to flow for a short time subsequently to the terminal exciting impulse. When a current is produced by an electric machine, the glass cylinder, or plate, as the case may be, is necessarily kept in motion. When a voltaic combination is the electric source, the *liberated* elements of the liquid in the battery are put into motion and become vehicles for the transportation of the electric fluid to and from the solid parts of the arrangement: and a thermo-electric current depends upon the motion of the calorific matter: for when that element is perfectly at rest in the combination, the electric current ceases to flow.

113. From the above considerations it appears, that a perpetual propagation of thermo-electric currents on the surface, or within the body, of a steel magnet would require a perpetual motion of caloric within its mass: which motion, unless the production of some hidden, mysterious, and unsuspected agent within the steel, would require as continual an influx and efflux of the calorific element from and to the surrounding medium. Moreover, the laws of electro-magnetism require that the direction of the electric currents should be at right angles to the axis of the steel bar; and the ingenious author of the hypothesis has ventured to assert that their route is in that direction, in a series of parallel spirals round

---

* Philosophical Magazine and Annals of Philosophy, vol. x. p. 1.

its surface.* Such, then, are the necessary conditions upon which Ampere's hypothesis essentially depends; and being now, probably for the first time, disrobed of their mysterious habiliments, I must necessarily resign the glory of their *discovery* to those philosophers who still entertain the idea of their existence in the steel, and who may possibly be enabled to penetrate the subject still deeper than I have investigated it. But before I quit this important topic, I will mention a few more facts, which to me, have appeared of some consequence, and can hardly fail to be interesting to others who may be induced to pursue the enquiry.

171. If the temperature of one extremity of a steel bar be elevated, and, by that process, electric currents become excited, those currents would necessarily be more powerful than any which can be supposed to exist in the metal at its natural temperature: and if the other extremity of the steel were to be heated, and again thermo-electric currents be produced in it, these latter currents would be propagated in the opposite direction to the former, and consequently the magnetic forces which they brought into play would be exerted in the reverse order to those which the first currents excited: and these artificially excited electro-magnetic forces being more powerful than any which the *supposed* natural electric currents could produce, they would predominate over these latter, and give new energies to the bar, reversing its poles in accordance with the directions of the currents. But on making the experiments, and carefully examining the phenomena, I find that no such corresponding changes have taken place in the polar forces of the magnet: and, although the poles themselves are considerably molested during the unequal temperature of the extremities and other parts of the magnet, and are removed from their original positions by the heating process, they do not assume those positions and variations of force which the thermo-electric current would necessarily give to them, were they governed by no other influence:† hence I infer, that

---

* Annales de Chimie et de Physique, t. xv.: and Ampere's Recueil des Observations Electrodynamiques.

† At the time this memoir was first drawn up, only a few experiments had been made on this part of the enquiry, the general results being such as are described in the text. But, whilst writing a fair copy for the press, I was led to reconsider this part of the subject, and it occurred to me, that by pursuing the experiments, some results might probably appear which would be interesting in the theory of terrestrial magnetism. I, therefore, resumed the enquiry and have been led to some novel facts which, to me, have appeared exceedingly important, by throwing a new light on the action of caloric on magnetism. They will be explained in the Third Memoir.

thermo-electric currents do not constitute the sustaining power of the magnet.

115. I next subjected a steel bar magnet to the influence of electric currents proceeding from a voltaic pair of copper and zinc. The voltaic combination was of the cylindrical shape and size, which, as is well known, I have long employed for electro-magnetic purposes, the zinc being surrounded with brown paper or calico, to prevent contact with the inside of the copper; and the whole placed in a pint porcelain jar, the exciting liquid being a solution of nitrous acid in water. The magnet which I employed was of hard-cast steel;—cylindrical, and about 6 inches long, and $\frac{3}{4}$ of an inch in diameter. It was well polished on an emery wheel, and of considerable power. It would lift, by one of its poles, a piece of soft iron of its own weight. A piece of soft iron of precisely the same figure and dimensions as the magnet, was also provided. A single helix of copper wire, No. 13, of the same length as the magnet, was formed on a hollow pasteboard cylinder, of sufficient width for the easy introduction of the magnet or iron. With these preparations, and a compass-needle furnished with an agate cap, and supported by a fine steel point, the experiments were carried on in the following manner.

116. When the meridian line of the compass-box had been adjusted parallel to the needle at rest, the helix was placed on the eastern side of its pivot, with its axis in the same horizontal plane as, and at right angles to, the axis of the needle; the nearest extremity being 12 inches from the needle's pivot. Fig. 9, Plate II, is a representation of the arrangement, where C is the compass-box, H the helix, and B the battery. Before the battery connexions were made with the helix, the magnet was introduced to the interior of the latter with its marked end nearest to the needle, consequently at 12 inches distant from its pivot. The south end of the needle was drawn towards the magnet a certain number of degrees, and this deflection being noted, the magnet was taken out of the helix, and replaced again with its poles in the reverse order, by which means the north end of the needle was drawn towards the magnet, which deflection was also noted. The magnet's action on the needle being thus ascertained, the electrical force of the battery was laid on, whilst the magnet was in the helix; and when the deflection arising from this combined force had been ascertained, the battery connexions were reversed, and consequently the direction of the current in the helix was reversed also. This last direction of the current gave a new deflection of the needle, which, after being ascertained, was also noted down. This done, the

magnet was reversed in the helix; and when the deflections of the needle arising from the current traversing the helix in each direction respectively had been ascertained, the electric current was finally cut off, and the deflecting power of the magnet alone again ascertained in the same manner as at first.

117. The bar of soft iron was next placed in the helix, and the electric current again laid on; and when the deflection arising from the polar force of the iron, by the first direction of the current, had been ascertained, the battery connexions were reversed, and with them, of course, the polarity of the iron was reversed also. The new deflection was noted down, and the iron finally removed from the helix. The deflecting power of the current alone, when no iron nor magnet was in the helix, was also ascertained at different times during these experiments; two sets of which were made with two different batteries—the former by an old battery, and the latter by a new one. The results, with all the necessary particulars, are arranged in the following tables:—

First Series of Experiments.

Deflections with the magnet in the helix, with and without the electric current from the old battery: and magnet retouched.

With or without the current.	Marked or unmarked end of the magnet nearest to the needle	North or South end of the needle drawn towards the magnet	Deflections	
Without . . .	Marked	South	15°	1
Ditto.	Unmarked	North	16°	2
With	Marked	South	17°	3
Current reversed	ditto	ditto	7°	4
With	Unmarked	North	18°	5
Current reversed	ditto	ditto	9°	6
Magnet alone . .	Unmarked	North	13°	7
Ditto.	ditto	ditto	12°	8

118. The electro-magnetic force in the helix alone, by this battery, produced no perceptible deflection of the needle: but when the soft iron was placed in the helix, the mean of several deflections, with the currents in different directions, was 17°.

119. By taking the mean of the deflections 3 and 5 in the table, which are those obtained whilst the electro-magnetic action of the current conspired with that of the magnet, and comparing that mean (17·5°) with the mean of the deflections

with the soft iron (17°), we find that they are nearly to the same extent. And by comparing these again with deflections 1 and 2, which are due to the magnet alone, we discover that a current which is incapable of exalting the original deflecting power of the magnet 2°, is yet capable of raising a deflecting power in soft iron, equal to the whole of that exhibited by the magnet. even when aided by the influence of the current. We discover also, by deflections 4 and 6, that the same current, when exerted in *opposition* to the energies of the magnet, is incapable of counteracting more than one-half the deflecting power of the latter. And we learn, by comparing deflections 7 and 8, which are those due to the magnet after being subjected to the *reverse* electro-magnetic action of the current, with deflections 1 and 2, that the *same* electric current, which excited so great a power in soft iron, was incapable of reducing the *permanent* action of the magnet more than one-fifth of that which it originally exhibited.

SECOND SERIES OF EXPERIMENTS.

Deflections with the magnet in the helix, with and without the electric current, with the new battery; and magnet retouched.

With or without the current	Marked or unmarked end of the magnet nearest the needle.	North or South end of the needle drawn towards the magnet	Deflections	
Without . . .	Marked	South	20°	1
Ditto	Unmarked	North	19°	2
With current . .	Marked	South	25°	3
Ditto reversed. .	ditto	North	1°	4
Magnet alone . .	Ditto	South	11°	5
Ditto	Unmarked	North	9°	6

Magnet re-magnetized.

Without . . .	Marked	South	21°	7
Ditto	Unmarked	North	21°	8
With current . .	Unmarked	Ditto	27°	9
Ditto reversed. .	Ditto	South	2°	10
Magnet alone . .	Ditto	North	8°	11
Ditto	Marked	South	10°	12

With this battery the soft iron gave a deflection of 18°; and the current alone, without either magnet or iron in the helix, about 1°.

120. In this second series of experiments there is displayed a manifest superiority of electro-magnetic action over that

shown by the old battery; but although deflections 4 and 10, show that the electro-magnetic action completely counterbalanced the deflecting force of the steel magnet, deflections 5, 6, 11, and 12, as obviously demonstrate that the original magnetic power was very far from being annihilated, and that, notwithstanding the vigorous electric current to which the bar had been subjected, the latter retained about one half of its original power, which that current was unable to subdue. Indeed it appears from both series of experiments that a great portion of the electro-magnetism of the helix operates merely on the *disposable* part of the magnet's force, and diverts it from its original direction, in the same manner as soft iron, or other magnets would do; and the electro-magnetic force thus engaged, is prevented from assisting the other portion in conferring permanent effects on the steel. When the constraining electro-magnetic force is removed, the liberated disposable force of the magnet with which the former had been engaged, again resumes its original direction, and gives the needle a new deflection, in the *same direction*, though not to the same extent as at first (Deflections 5, 6, 11, 12.).

121. I am not aware that any one would venture to assert that electric currents, more powerful than those employed in these experiments, still existed in the steel: and if not, to what cause are we to allude the retained magnetic force? There must be some agent in operation which still sustains the polar action, and resists the energies of the assailing electric current. That agent cannot be electricity, or it would have been subdued by the counteraction of a superior electric force; it must, therefore, be admitted, that some other physical agent, perfectly distinct from the electric, presides over the polar forces of the steel magnet.

122. I am well aware that, had the electro-magnetic force of the current been more powerful, the magnetic forces of the steel would have suffered to a greater extent; and it is possible that an electro-magnetic force might be employed of sufficient extent to completely annihilate the original polarity of the steel, or even reverse its polar action; but I should wish it to be understood, that to accomplish such an effect, the electric current employed must be very powerful indeed: and whatever extent of polarity might be exhibited by the steel after the removal of the exciting electro-magnetic force, the *retention* of that polarity could not be supposed to depend upon that *absent* exciter, any more than the polarity of this, or any other piece of steel, could be supposed to be sustained by the absent magnet which first excited it: and our present know-

ledge of electro-dynamics does not permit us to indulge in the idea that any sustaining electric currents remain in the steel.

123. We have seen by the preceding experiments, that the power of the magnet was considerably lessened by the action of the electro-magnetic force in the helix; but it must be observed that the latter force had no *sustaining* power to contend with, excepting that exercised by the retention of the steel: but if the magnet be placed under the influence of a *sustaining* magnetic force during the time it is assailed by the electro-magnetism of the helix, it will be found that the latter is too impotent to make any other than a very slight permanent impression on the original power of the steel magnet; and, under some circumstances, not the slightest impression is accomplished. To prove this fact, I place the *marked* end of a magnetic bar, seventeen inches long, in contact with the *unmarked* end of the six inch cylindrical magnet whilst placed in the helix, the marked end of the latter being nearest to the needle, as represented by fig. 10, Plate II. I now transmit the electric current through the helix, in a direction which tends to neutralize the magnetism of the inclosed bar. The current is continued for more than a minute, after which it is removed, and as speedily as possible, the long sustaining magnet is removed also. This done, the deflecting power of the cylindric magnet is again ascertained. The following table shows the results.

THIRD SERIES OF EXPERIMENTS.

Deflections with the magnet in the helix, with or without the electric current, from a new battery.

With or without the electric current.	Marked or unmarked pole of the magnet nearest to the needle	N or S. end of the needle attracted	Deflections
Without the current	Unmarked	North	29°
Ditto	Marked	South	31°
Ditto	Ditto sustaining magnet attached,	Ditto	65°
With the current tending to neutralize the magnet . . .	Ditto	Ditto	59°
Current and sustaining magnet removed	Marked	Ditto	26°
	Unmarked	North	24°

124. I next place the cylindrical magnet under the influence of two sustaining magnetic bars, each 17 inches long; submitting it, at the same time, to the action of an electric cur-

rent, tending to neutralize it. The arrangement is represented by fig. 11, and the results were as follows:—

FOURTH SERIES OF EXPERIMENTS.

	Mean deflection of both poles of the Needle
Before the magnet was subjected to the action of the current	30°
After the magnet had been subjected to a current tending to neutralize it	31°

125. When under the sustaining force of two magnets, we find that the electric current makes no impression on the small magnet on which it operated. The trifling power which the magnet gained during the experiment, was obviously due to the influence of the bars between which it was placed. The additional power given to the intervening magnet, by this means is, however, but very small, never amounting to more than 2° of deflection, as I have ascertained by several experiments, by permitting the cylindrical magnet to remain between the poles of the two large ones, as in fig. 11, for two minutes in each experiment; which is a much longer time than it remained under the same influence after the removal of the electric current in the preceding experiments. Hence, since a sustaining magnetic force may be employed to any required extent, the obvious inference is this. *No electric current, however powerful, is capable of impairing the powers of a hard steel magnet, whilst the latter is under the protecting influence of a proper purely magnetic force.*

126. Having ascertained that the sustaining magnetic force does not operate as an exciting power (125), I was led to suppose that the power of the *protected* magnet is sustained by the mutual attractions of its own *disposable* forces (105, note) and those of the sustaining magnets: the north and south polar forces engaging with each other too intimately to be disunited by the assailing electro-magnetism in the helix. This view of the nature of the action led me to try soft iron as a means of sustaining the power of the magnet, whilst the latter was subjected to the action of an electric current, considering that a portion of the disposable force of the magnet would be employed by the iron, and thus be protected from the assailing electro-magnetic force; but it was found by the experiments about to be described, that soft iron affords no protection whatever to the magnet when assailed by a converse electro-magnetic force: but on the contrary, the iron facilitates the subduction of the original powers of the steel magnet.

127. The experiments were made by placing the cylindrical magnet in the helix, and ascertaining its deflecting power on the needle at the original distance of 12 inches. Then placing in contact with its remote pole a cylindrical bar of soft iron, 6 inches long and about an inch in diameter. An additional deflecting force is thus given to the magnet, which deflection is also noted down. Another bar of soft iron, 3½ inches long, and about the same thickness as the former, was next placed in contact with that pole of the magnet nearest to the needle, and the new deflection thus given to the needle also noted down. This done, the electric current from a new battery was transmitted through the helix, whose magnetic powers were opposed to the powers of the enclosed magnet. The following table shows the results:—

FIFTH SERIES OF EXPERIMENTS.

Deflections with the magnet in the helix, with and without the soft iron and electric current from a new battery.

With or without the soft iron and electric current	Marked or unmarked end nearest to the needle	N or S end of needle attracted	Deflections	
Without current or iron	Marked	South	30°	1
With the larger piece of iron . .	ditto	ditto	42°	2
With both pieces of iron	ditto	ditto	65°	3
Do. with a converse electric current	ditto	North	40° then 19°	4
Current cut off, but iron remaining	ditto	South	25°	5
Magnet alone	ditto	ditto	10°	6

128. The principal circumstances to be noticed, in these experiments, are the singular changes of polarity by the soft iron, and the final subduction of a great portion of the force of the magnet. By deflection 4 we see a transposition of polarity by the action of the current. The new deflection thus given to the needle at first rose to 40°, but gradually sank down to 19°, where it remained permanent for some time. This reduction of the deflection was, of course, dependent on a reduction of polar energy in the nearest piece of iron: and as the polarity of the iron depended on the polar condition of the magnet, we learn that the transient transposition of its polarity is accomplished to the greatest extent, immediately after the current has got into full play, and that it gradually subsides for about one minute afterwards, at which time it has arrived at its minimum. These versatilities in the polar action

of the magnet are observable in all cases when it is subjected to a converse electro-magnetic action, whether there be any iron attached to its poles or not, though without iron they are not so great as when that metal is present. They are exceedingly curious, and are involved in a theoretical principle, which it is not necessary to enter into at present. By comparing deflections 1 and 6 we find that the magnet has lost a considerable portion of its power, which portion is greater by 6° or 8° than that usually lost when no iron is present, all other circumstances being the same; which shows that the attachment of the iron to its poles facilitates the subduction of the original powers of the magnet. See also the first and second series of experiments.

129. I had next recourse to the reverse process of that which was pursued in the last experiment. I placed the soft iron cylinder in the helix, and attached one pole of the cylindrical steel magnet to that extremity of it which was nearest to the needle; and whilst thus arranged, an electric current was transmitted through the helix. The distance between the pivot of the needle and nearest pole of the magnet was 12 inches. The following results were obtained:—

SIXTH SERIES OF EXPERIMENTS.

	Deflections.
Magnet alone, prior to being placed in the arrangement	38°
Magnet attached to the iron bar, the latter being under the influence of the current .	45°
Magnet alone, after the iron and current were removed	38°

The magnetism of the soft iron left no additional permanent power on the steel magnet.

130. Having ascertained that an electric current is capable of subduing a considerable portion of the original power of an unprotected steel magnet (119, 120), it became an enquiry of some interest to ascertain whether or not the same current, with the magnet reversed in the helix, was capable of restoring the power which it had previously subdued. For this purpose, the cylindrical steel magnet was retouched; and after its deflecting power, at the distance of eight inches, had been ascertained, it was subjected to the action of an electric current from a perfectly new battery, whose copper exposed about a square foot of surface, with a proportionate rolled zinc cylinder inside. The battery was made exceedingly active by a solution of nitro-sulphuric acid. The following table shows the results.

SEVENTH SERIES OF EXPERIMENTS.

Magnet alone,	previous to its exposure to the current	39°
Ditto	after being exposed to a *converse* current	21°
Ditto	after being exposed to a *direct* current	25°
Ditto	after a second exposure to ditto	26°
Ditto	after several other exposures to ditto	26°

131. From this series of experiments, we learn that the active electric current here employed was incapable of restoring $\frac{1}{3}$ of that portion of the deflecting force, of a newly magnetized hard steel bar, which it was previously enabled to subdue, although as powerful during the one process as during the other. This exceedingly curious fact I have found in the results of several other experiments, and with batteries of different powers. But the same law does not hold good, unless the magnet has been magnetized to a high degree previously to its being subjected to the electric currents; nor, perhaps, will it be found *generally* exact, even under these circumstances, although I have not met with any results in direct contradiction to it. And although the ratio of the *subdued* and *restored* force may vary, I have cause to believe that in no case will the restored force be more than one-half of that which had been subdued by the same current, when the magnet employed is hard cast steel, and not below the dimensions of that which I have described (115): and the voltaic plates of proportional magnitude.

132. Another interesting fact presented itself by neutralizing the cylindrical steel bar, and afterwards magnetizing it by the electro-magnetic action in the helix, whilst the latter was transmitting a copious and active current from the battery last described (129), furnished with a new zinc. The deflecting power which the steel acquires, by this process, is about one-half of that which it exhibits by means of ordinary magnetic excitation. I have doubled and trebled the coil in the helix, but in no case has the magnetic power of the steel increased above that I have just mentioned. The facts developed by these experiments, are partly attributable to the magnetic force receiving different forms of distribution by the magnetic and electric processes of excitement; though principally from an absolute incapacity in the latter of bringing forth those intense magnetic forces which hard steel is susceptible of displaying. There seems, indeed, to be a vigorous tension in the magnetism of hard steel, which that of electric currents cannot compete with in vanquishing those formidable resisting forces presented by hard ferruginous bodies, whilst undergoing the magnetizing process. Even the magnetism of soft

iron, when brought into play by electric currents though much more abundant in quantity, is of far lower tension than that of hard steel. This curious fact may be shown by experiments with two horse-shoe magnets; one of which shall be soft iron, brought into play by electric currents, and the other a permanent one of hard steel. When the cross pieces of both magnets are of soft iron, the iron magnet will have the greatest lifting power; but when both cross pieces are of hard steel, the steel magnet will have the greatest: and this is the case even when the power of the iron magnet (with soft iron cross pieces) exceeds the other to a considerable extent.

133. There is a remarkable phenomenon observed whilst magnetizing hard steel by electric currents. The deflecting power of the steel is much greater whilst under the dominion of the current than after the latter is cut off. Now, as the helix alone exhibits no action on the needle (118, 119) the experiment shows that there is a temporary disposable force excited even in hard steel, which that metal does not exhibit when the exciting cause is removed. This fact probably arises from a new distribution, rather than from an absolute loss of the magnetism first excited by the current.

134. Having ascertained that the existence of electric currents is nowhere to be found in permanent steel magnets, (114) and also demonstrated the inadequacy of electric excitement to the production of that extent of magnetic energy in hard steel, which is susceptible of development by the ordinary process of magnetization (131), it may now be interesting to inquire how far the doctrine of *systems* of electric currents is susceptible of application in explaining the phenomena exhibited by permanent steel magnets.

135. Let N and N′, fig. 12. represent transverse sections of two cylindrical systems of electric currents, both of which are flowing in the same direction, as represented by the arrows: and let these cylinders be prolonged parallel to each other to any required distance behind the paper. Now, because of the electric currents on the adjacent sides of these cylinders running in opposite directions, in every pair of parallel sections, similar to those represented on the paper, those cylinders will exhibit a repulsion for each other throughout their whole length, or from end to end, according to the principles of electro-magnetism. Let, now, the remote extremity of the cylinder N′ be turned towards the spectator, permitting the cylinder N to remain unmolested. Under these circumstances, the *same* extremities N, and N′, of the two cylinders whose adjacent currents, in the former case, flowed in *opposite directions*, will now flow in the *same direction*, as may be

understood by looking at fig. 13: and consequently those extremities will attract each other. Again, let the arrows in fig. 14 represent the directions of two cylindrical systems of electric currents placed at right angles to each other, as C and C′. The adjacent portions of these currents flow in the same direction, and consequently will *attract* each other. Now place the electro-magnetic system C′ in either of the positions represented by fig. 15, and it is seen that the adjacent currents in C and C′ now flow in opposite directions, and will consequently *repel* each other.

136. From the above illustrations we learn that the extremities of two systems of electric currents will either attract or repel each other, according to the positions in which they are placed, and that they do not exhibit any specific polarity in the manner of ferruginous magnets, whose attractions and repulsions have no dependence whatever upon the positions in which their extremities are placed with respect to each other, but are invariably referrible to their specific polar character. There is, indeed, a striking distinction in the distribution of the magnetic force of steel bars, and that exhibited by electric conducting wires, whether the latter be in a simple strand, or coiled into any particular fashion. A conducting wire formed into a hollow helix displays but very little polarity exteriorly, in the direction of its axis (118, 119,), because of the inner and outer sides of the coil exerting their magnetic forces in opposite directions: but with hollow steel magnets, the polar forces of each individual extremity conspire with each other, and operate in concert upon vicinal ferruginous matter, whether previously polarized or otherwise; and in precisely the same manner as such matter is operated on by *solid*-magnets. Hence it is, that a polarized needle, or small bar, freely suspended, with its centre in the equatorial plane of a hollow steel magnet, whether *inside* or *outside* of the tube, will invariably assume one and the *same* direction: whereas a similarly suspended needle, with reference to, and under the influence of, a hollow system of electric currents, would assume *one* direction when *within*, and the opposite direction when *without*, the system: and as this peculiarity of magnetic arrangement would attend every system of electric currents that can possibly be formed, it is just to infer that the distribution of force displayed by steel magnets, or by loadstone, cannot be imitated by any system of electric currents whatever: and *vice versâ*, the exquisitely uniform arrangements of enveloping magnetic action, so beautifully displayed around electric currents, appear to be totally inimitable by any known forms of ferruginous magnetic bodies.

137. It would be an almost endless task to examine every fact that might be brought to bear, directly, or indirectly, on the subject of this investigation. I have not dwelt on electro-magnetism to the extent I would have done, had my theoretical views on that department of electricity not been already before the public, although I have cited those electro-magnetic phenomena which appear to be the most important in the present discussion. In other departments of electricity I have enumerated such facts as have appeared necessary to collate with purely magnetic phenomena; and having discussed them individually as I have proceeded, a retrospection would be needless in this place. The inference to be drawn from the investigation of the *facts* alone, appears to me to admit neither of doubt nor equivocation; and may be thus briefly stated: *There are no facts on record which demonstrate an identity in electricity and magnetism; but, on the contrary, there are many phenomena which justify the idea of their being perfectly distinct powers of nature.*

IV. *On the use of Electro-magnets made of iron wire for the Electro-magnetic engine. By* J. P. JOULE, ESQ. *Communicated in a letter to the Editor.*

Salford, March 27, 1839.

Dear Sir,

In my last letter I gave you an account of some experiments which were intended to prove that electro-magnets made of iron wire are the most suitable for the electro-magnetic engine. In those experiments round wire was used,—and it was my opinion, that the wire magnets were put in a disadvantageous position, in consequence of the interstices between the wires. I have since confirmed my views on this subject by the following experiment:

I constructed two magnets. The first consisted of 16 pieces of square iron wire, each $\frac{1}{11}$ inch thick, and 7 inches long, bound very tightly together so as to form a solid mass, whose transverse section was $\frac{4}{11}$ inch square; it was then enveloped by a ribbon of cotton and wound with 16 feet of covered copper wire, of $\frac{1}{16}$ inch diameter. The second was made of solid iron, and was in every other respect precisely like the first. These magnets were fitted to the apparatus used in my former experiments, and care was taken to make the friction of the pivots equal in each. The mean of several experiments gave 162 revolutions per minute for the first, and 130 for the second magnet.

In the further prosecution of my enquiries, I took 6 pieces of round iron of different diameters and lengths, and 1 piece of hollow round iron, $\frac{1}{16}$ of an inch thick; these were bent into the U form, so that the shortest distance between the poles of each, was half an inch; each was then wound (with the usual precautions to ensure insulation), with 10 feet of covered copper wire, $\frac{1}{40}$ inch in diameter. The lengths and diameters are given in the table. No. 1 is the hollow magnet. The attraction was ascertained by suspending a straight steel magnet, 1½ inch in length, horizontally to the beam of a balance, and bringing the several magnets directly underneath at the distance of half an inch, which was preserved by the interposition of a piece of wood. Care was taken that the battery remained constant during the experiment.

	No. 1.	No. 2.	No. 3.	No. 4.	No. 5.	No. 6.	No. 7.
Length in inches.	6	5½	2¾	5¼	2½	5¼	2½
Diameter in inches.	½	¼	½	⅜	⅜	¼	¼
Weight lifted in ounces.	36	52	92	36	52	20	28
Attraction in grains.	7·5	6·3	5·1	5·0	4·1	4·8	3·6

A steel magnet of such dimensions as enabled me to compare it fairly with the rest, excited in the same circumstances an attractive power equal to 23 grains, while, at the same time its lifting power was only 60 oz.

These results will not appear surprising if we consider, first, the resistance which iron presents to the induction of magnetism: and, secondly, how very much the power of iron to conduct magnetism is exalted solely by the completion of the ferruginous circuit. In order, however, to explain why the long electro-magnets have a *greater* attracting power, and lift *less* weight, than the short magnets of the same diameter, it will be necessary to observe that it was impossible to wrap the whole 10 feet of wire on the smaller magnets, without disposing it in two or even three layers (according to the size of the magnets): this is a great disadvantage, and one might anticipate in consequence that the power of the long magnets should be greater than that of the short for lifting as well as for attraction, contrary to the results in the table; this, however, may be explained, if we admit that the comparative resistance of the iron of the electro-magnet increases to a very great amount, when its magnetism is so greatly excited by the contact of the armature.

Nothing can be more striking than the difference of the ratios of lifting to attractive power, in different magnets; whilst the steel magnet attracts with the force of 23 grains and lifts 60 oz., No. 3 attracts 5·1 grains and lifts 92 oz.

Here are some very general directions for making electro-magnets for lifting. 1st. The magnet, if of considerable bulk, should be compound; and the iron used, of good quality and well annealed. 2d. The bulk of the iron should bear a much greater ratio to its length than is generally the case. 3d. The poles should be ground quite true, and fit flatly and accurately to the armature. 4th. The armature should be equal in thickness to the iron in the magnet.

I shall now proceed to consider with greater care, what form of electro-magnet is best for distant attraction, as that is the only force of any use in the electro-magnetic engine. Here two things must be considered—the length of the iron, and its sectional area.

Now with regard to the length of the iron, I have found that its increase is always accompanied with disadvantage, unless the wire is (by using a shorter length) forced to too great a distance from the iron. In making magnets for the engine it will be proper to use a length less than that which gives the maximum of attraction. on several accounts.

The next thing to be considered is the sectional area. You have shown,* that on placing a hollow and solid cylinder of iron successively within the same electro-magnetic coil, the hollow piece exerted the greatest influence on the needle. I wished to ascertain whether a hollow magnet might be represented by a solid one whose sectional area and circumference is the same, and whose thickness is twice as great as that of the hollow magnet. Fig. 12 and 13 Plate I. will show more clearly what I mean: they represent sections of a hollow and a rectangular magnet, and it will be seen that if either of them is divided at the dotted lines, the separate pieces when put properly together, will make up the other. Two electro-magnets were constructed, each 7 inches long, and covered with 22 feet of covered copper wire $\frac{1}{16}$ inch in diameter; the sections were precisely similar, but double the size of those in the figures. Here is their actual attraction, at half an inch distance. for the proper pole of a straight steel magnet.

	Hollow magnets.	Solid magnets.
Attraction in grains	1·9	1·7
With a more powerful battery	4·5	4·0

---

* See the very interesting researches at page 470, Vol. I.

It is evident from this that the hollow magnet has the greatest attractive force, but I do not think that the difference is so great as to counterbalance the many advantages which the solid magnet would give, if used in the engine. I shall therefore first relate a rather important experiment; and secondly make an attempt to determine the sectional area of solid iron most proper for different powers of battery.

I made five straight magnets of square iron wire $\frac{1}{11}$ inch thick: each was 7 inches long and wound with 22 feet of covered copper wire $\frac{1}{16}$ inch in diameter. No. 1 consisted of 9; No. 2 of 16; No. 3 of 25; No. 4 of 36; and No 5 of 49 wires; arranged in the form of a prism with square base and sections. Five other magnets were made of solid iron, but in every other respect exactly similar to the first. Here are the attracting powers (at half an inch) for a straight steel magnet, with three different galvanic forces.

		No. 1.	No. 2.	No 3.	No. 4.	No. 5.
1st Ex.	Attraction of iron magnet in grains.	1·5	1·9	1·6	2·1	2·0
	Ditto of wire magnet	2·1	2·1	1·7	2·0	1·9
2d Ex.	Iron magnet. . . .	2·0	2·5	2·35	2·45	2.2
	Wire ditto.	2·6	2·8	2·1	2.2	2.05
3d Ex.	Iron magnet. . . .	2·7	3·6	3·4	3·2	3·1
	Wire ditto.	3·3	3·8	3·0	2·9	2·65

The wire used in these magnets was taken at the same degree of temper, as that in which it came from the makers: it was in consequence not so well annealed as the iron with which it was compared. On this account the numbers opposite to the wire magnets are less than they would otherwise be; still, however, the results in the table seem anomalous. First, it will be remarked that while the wire magnets are more powerful in the first numbers, they are less powerful in the last numbers, than the iron magnets. I cannot account for this unless by supposing, according to the hypothesis of Dr. Page, that the wires of which the magnets are composed repel one another's magnetism in such a manner as to tend to neutralize the general force of the electro magnet, and that this neutralizing effect increases with the number of wires used. But the deficiency of No. 3 magnets in ex. 1. is most remarkable, and particularly as by increasing the power of the battery the deficiency is reduced, and that at the same time the wire magnet becomes less, though at first it was more, powerful than the iron magnet, compared with it.

In my next, I shall attempt to determine what sectional area is best for different electric forces. In the mean time,

I remain, dear sir,
Yours most respectfully,
J. P. JOULE.

V. *Note on illuminating Gas, and particularly on the formation of Gas from water by means of* M. Selliques's *apparatus.* *By* M. GROUVELLE. (Extract.)*

M. Selliques never said that water by passing over coke heated to redness, was transformed into *carbonated hydrogen.* It is well known that it produces a mixture of oxide, carbon, and hydrogen, almost entirely pure.

But M. Selliques charges this *hydrogen* with carbon by causing it to traverse a *red-hot* cylinder, when it meets with highly carbureted oils. It is a chemical combination and not a mixture which then takes place, as is proved by the analysis of the gas formed from water, by M. Peligot, Repetiteur à l'Ecole Polytechnique; viz.—

Carbonated Hydrogen	57	100
Oxide of Carbon	28	
Free Hydrogen	15	

Hence the theoretical question of lighting is this: What process gives the most light with one kilog. of oil or any resinous substance, resin, schist, or coal tar, &c.? One kilog. of oil of schist or resin furnished, by Selliques's apparatus, 70 *cubic feet* English of burning gas, of which it requires 3 feet to supply a burner equal to 10 wax candles for one hour; which gives 23 *hours' light.*

But at Belleville, Antwerp, Frankfort, and wherever gas is made on a large scale from *oil of resin* and a portion of pure resin, the mean product is from 15 to 17 *feet* per kilog. of oil, but in three or four days the product falls to 12 and 15 *feet.* The insulated attemps, with new retorts, may give as much as 24 or 25 feet: and M. Tailleberg has pronounced this production of 25 feet as a great discovery. Let us take this number: this gas burns 2½ feet per hour to give the light of 10 wax candles; this is (although nearly double the mean) the report furnished by the lighting of the City of Antwerp, in October, 1837, with gas from resin at 12 feet to the kilog., and in October, 1838, with gas from water. We will only reckon upon 2½ feet; hence, 1 kilog. of oil gives at the maximum 11 hours' light, and admitting even 34 feet to the kilog., as has been stated in a journal (and this quantity cannot perhaps be obtained without the addition of water), it would then be only 15 hours while we have obtained 23 from the gas made from water.

---

* From the Comptes Rendus, &c. No. 23, 1838. Translated by Mr. J. H. Lang.

But the production of gas from water does not stop at 70 feet per kilog. By increasing the proportion of water to oil in the apparatus, we weaken the density of the gas, which approaches to, and even descends lower than, the density of the coal gas. In some experiments made on more than 1500 feet, observed for several consecutive hours and proved by a tedious process, I carried the production to 222 feet of burning gas with 1 kilog. of fish oil (the oil of schist, which I had not at this time, gives in Selliques's apparatus the same results as fish oil).

This gas at 222 feet only burnt 6½ feet to give the light of 10 wax candles; it was scarcely 1/0 weaker than coal gas. Gas produced at 110 feet to the kilog. of oil of schist gave me a consumption of 4 feet 20 for the same burner. Thus at about 160 feet to the kilog. of oil, the gas from water is equal in power to coal gas, and burns 5 feet an hour. Hence, 1 kilog. of oil gives 40 *hours' light*. It is easy to calculate what this light costs with oil of schist, which, in the places of its production, is not more than 5 francs per 100 kilog., with a combustible expenditure which decreases as the proportion of the gas produced and the size of the apparatus increase. With the gas from resin, on the contrary, the decomposition of the oils operating at the melted surface, small retorts are the most advantageous, and at the same time, the volume of the gas produced (not its mass) is increased only in decarbureting, by a higher temperature, a part of the richest carburets of hydrogen.

The indefinite increase of light obtained with the gas from water, in proportion as it is produced more feeble, tends to prove that the presence of the oxide of carbon increases the illuminating power of this gas, doubtless increasing the quantity of heat developed during the combustion.

We learn from two reports made to the Antwerp Society for lighting with gas, that from the 1st of June, the City of Antwerp was lighted with the greatest success by the gas from water; that three furnaces are in active employ and produce from 24 to 25 thousand cubic feet of gas per day; notwithstanding the useless expense with which the Society is charged, the gas at 70 feet to the kilog., costs, workmanship and keeping up the furnaces included, less than 5 francs per 100 feet with oil of schist at 15 francs per 100 kilog.

The superior quality of the gas from water to that from coal, on account of its total absence from sulphur and ammonia, need not be discussed; and on the other hand, the cost of this gas is no longer in doubt: at Antwerp, at Belleville even, it has been proved in every way.

At Antwerp, the burner, equivalent to 10 wax candles, consumes at the rate of 3 feet, 1^{c}·35 per hour, and costs 4 francs 50 cents per 100 feet.

At Paris, the coal gas (which is not obtained for nothing as has been pretended) costs, as the companies know, from 4 to 5 francs per 100 feet, including workmanship, washing, and keeping up the apparatus; at Mons, even the director allows 3 francs. At London, it costs 2s. 6d. or 3 francs 12 cents.

But it would not be difficult for us, with oil of schist at 6 or 8 francs, to produce in London, or even in Belgium, gas from water, at the rate of 160 feet to the kilog., equal in power and superior in quality to the coal gas, at less than 3 francs per 100 cubit feet English.

I shall add a few words on the employment of asphaltum pipes.

The question is not what pressure asphaltum pipes will sustain, that of the gasometers being always infinitely small, *but how they will support the chemical and slow action of the gas itself, on the asphaltum.* After some experiments, unfortunately too short, several thousand stone pipes were placed in Louvain, with well baked *asphaltum* joints, and at the end of four or five months most of these joints were eaten and pierced by the gas, which no doubt attacked them by means of the small quantity of essential oil, which it carries in its vapours. It was necessary to replace all the resinous joints by others of clay, covered with Roman cement, and up to the present time the results appear good.

VI. *Description of a Voltaic Battery. By* S. E. Hoskins, M. D. *Communicated in a letter to the Editor.*

Sir,

The plate voltaic battery, although nearly superseded by the more modern cylindrical arrangements, is still of sufficient value to sanction an endeavour towards improving its construction.

If the following description of a method I have devised for its simplification be worthy of a place in your excellent Annals, I shall be obliged by its insertion.

The various inconveniences arising from the usual methods of connecting galvanic plates, and the difficulty of cleansing the zincs when soldered to the coppers, induced me some time ago to seek a more simple mode of effecting junction. The result is a method whereby solder, mercury cups, and binding screws are dispensed with; and perfect connexion between a dozen or eighteen pair of plates effected by the mere adjustment of a couple of thumb screws.

Fig. 14, Plate I., represents a wooden frame with fine transverse saw cuts, half an inch deep, and one fourth or one sixth of an inch apart. This frame is intended to fit over a trough without partitions. Each of its projecting extremities is perforated for the passage of a brass bolt, having a head at one end and a deep thread for the reception of a thumb screw at the other.

The plates which are dropped into the transverse saw cuts of the frame, alternately interlacing, according to Messrs. De la Rue and Young's plan, are cut out of copper and thin sheet zinc.*

The plates being dropped into the frame, the long ears are to be bent over its convex sides, so that a zinc shall be in contact with a copper plate. This being done a strip of wood is placed over the bent ears: over this a strip of brass and the whole bound together by the bolts and thumb screw, until perfect contact is secured.

This arrangement will be better understood by reference to fig. 14, of the accompanying sketch, which gives a transverse view of the apparatus.

a, a, a, a, are the plano-convex sides of the frame. *b, b, b, b,* the alternating plates of zinc and copper. *c, c, c, c,* thin strips of white deal varnished, and barely long enough to cover the series of overlapping ears. *d, d, d, d,* strips of thick sheet brass, as long as the sides of the frame, and perforated at each extremity. E, E, bolts and thumb screws, which are represented as having tightened the binders on one side, and in readiness to do so on the other.

The dotted lines, in fig. 15, are intended for pieces of varnished cord permanently fixed on the copper plates: a simple but effectual method of keeping the plates asunder.

The battery may be used with dilute acid or with the acidulated solution of sulphate of copper recommended by M. De la Rue. With the latter, good decomposing action will continue for upwards of an hour; at the end of which time three inches of fine platinum wire can be kept in a state of incandescence for half an hour or more. In short, the power of the battery is quite equal to that of other plate arrangements possessing advantages peculiar to itself. These advantages are convenience in an extended sense of the word, and economy both of time and money. Eighteen or twenty pair of plates may be sundered and put together again in a few minutes; the zincs therefore may be easily washed, amalgamated, or replaced, with great facility and without the use of the soldering tool, the waste of mercury, or the annoyance arising from a series of binding screws. A stock fashioned by the manipulator may always be kept at hand with no more expense than that of the material; and he may extend his series with ease to any extent.

---

* The thinner the better as it can be readily cut and bent, does not require wide saw cuts, which would weaken the frame;—lasts quite long enough for one durable operation, and admits of fresh plates being used each time.

The only parts which cannot in general be made by the amateur are the trough, the screws, and the frame. The latter requires some degree of nicety in its construction,—none however which a common carpenter, properly directed, may not attain.

A small battery, such as I have described, has been deposited at the Polytechnic Institution, ever since the month of October. Mr. Bachhoffner, to whose kindness on all occasions I am much indebted, has performed a series of carefully conducted experiments with it, and allows me to state in his name that my battery is much more powerful than others of the same order, owing to the approximation of the plates, and that it is much more convenient and manageable than any.

I remain, Sir,
Guernsey, May, 1839. Your obedient servant,
S. E. HOSKINS, M. D.

VII. *On a New Magnetic Electrical Machine. (Magnet-electromotor.) By* Dr. Neeff, *of Frankfort.**

Exhibited at the Friburg Meeting of Philosophers, in Sept., 1838.

Since the time I made known the peculiarities of my electrical wheel, or mill, to the scientific meeting at Bonn, (and afterwards in Poggendorff's Annals for November, 1835,) the remarkable effects of electrical discharges, repeated in rapid succession, have become studied with much attention: and it was soon discovered that, for the production of such a quick succession of electrical light. magnetic electricity is peculiarly and excellently adapted. For this purpose magnetic electricity has been employed as it is excited by the machine first invented by Pixii, then by Saxton's and Clarke's improvements; each of which has an armature of soft iron, surrounded by spirals of copper wire, which rotate in front of the poles of a steel magnet, by which arrangement a machine is brought into good operation; and the progressive improvements which the sagacious and ingenious Ettingshausen has given to this machine, are so excellently contrived, that there appears to be left but little more to be done to accomplish its perfection. In the meantime, however, I have been inclined to believe that some other way must yet be pursued to arrive at the principal object in view: which would be to replace the steel magnet by an electro-magnet of soft iron. The first effect which I obtained by this substitution, was far short of that which I had been led to expect, a circumstance that may be imputed to the deficiency which attended the first construction of the instrument. The essential corrections being, however, attained, the magnetic electrical operation can now be brought to the wished for vigour; the apparatus is easy and convenient to manage, durable in its action, of small dimensions, its price trifling, and is well adapted for a variety of purposes which are met with by the Surgeon, the Physiologist, and the Physician. These results have appeared to me so highly gratifying that I am led to believe the instrument is still susceptible of much farther improvement.

---

* Translated from the German by I.

With respect to the voltaic battery, I have relied upon those liquids which I have hitherto been in the custom of using for the excitation of voltaic troughs, but have returned to the oldest construction of voltaic apparatus, viz, piles of zinc and copper, with intervening discs of moistened paper. The zinc, however, I amalgamate. When the paper discs have become saturated in a solution of sulphuric acid, (consisting of about one acid and ten water) I form the pile, and place it in a screw-press. The performance, of this construction of a battery, is extremely uniform and durable. Experiments may be carried on daily for a considerable time and still the battery will continue active. Even for 12 or 16 days, experiments may be carried on before it is necessary to take the pile to pieces and introduce fresh discs of paper. Besides, the metals become so little corroded that they seldom want any other cleaning than merely wiping away the moisture from about their edges, a circumstance always to be attended to, otherwise a perceptible portion of the force is lost. With this pile, it is pleasing to find that we are not annoyed by the troublesome, indeed dangerous. liberation of gas, which always attends the apparatus when unamalgamized zinc is employed. The screw is also of very great advantage, by the slackening or tightening of which, the effects of the pile can quickly be weakened or strengthened respectively, at pleasure. I use plates of greater dimensions and number, as is necessary for the maximum of effects. Though but little is to be gained, in this way, for the present purpose of the pile, yet it gives me a great advantage of diversifying the force in many ways. The action becomes exhausted in, perhaps, about 14 days. In the manner above described, I prepared 8 copper and zinc plates in four pairs, insulating them from each other by dry paper, and keeping them pretty close together by means of the screw-press, and combined, as occasion required, the similar or dissimilar metals, by means of small conducting wires and quicksilver cups. The eight moistened papers which were placed between the copper and zinc were 4 inches broad and 4½ long, the plates being a little larger. The screw-press is, perhaps, 7 inches long and 6 inches broad, and serves as a basis for the support of the other parts of the apparatus.

The second essential part of the apparatus is the spiral. The principle of which is well known, as far as depends upon the length, thickness, and winding of the wire. The iron axle of the spiral, in every revolution, necessarily weakens the electric action, in consequence of a partial neutralization of the magnetic poles, by its close approach to them during its transits. As regards the function of the spiral, it is now known that, when it closes the circuit, the iron axle becomes magnetic; and, in opening the circuit, this magnetism, as well as that of the wire, immediately disappears; by means of which, the electric fluid in the spiral becomes impelled, and exhibited partly in a spark in the reverse order to the battery circuit, and is partly led off as a momentum current. The best method of a spiral is to have two wires wound close together. We can then employ it according to the various purposes for which it is wanted; we can combine the two wires in the *same* direction or in opposite directions; we can even, by the one, close and open the circuit, and by the other, lead away the magnetic electricity.

The third element of the magnet-electromotor is the mechanical, by which the closings and openings of the circuit through the spiral are performed. For this purpose, I first availed myself of the electric wheel, by which contrivance the shocks succeed one another with great velocity, and become well defined. When I had ascertained the powerful action of the apparatus, I was desirous of having it to excite itself, as in the electro-magnetic machine, without the inconvenience of turning the wheel. The ingenious construction which I employ for the purpose is due to Mr. J. P. Wagner.

It combines simplicity with efficacy, and is the result of a variety of contrivances which have now been carried on for two years: and was given to me by that gentleman. There are two parts inserted between the voltaic series and the spiral, which I call hammer and anvil. The hammer is a piece which is attached to one end of the spiral, (the other end being connected with one pole of the series) and the anvil is connected with the other pole of the series. If now, the hammer rests upon the anvil, the circuit will be closed, and the iron axle, becoming magnetic, attracts an iron plate which fastens to the hammer, and draws it away from its contact with the anvil. By these means the circuit will be opened, and the iron axle immediately loses its magnetism, which permits the hammer to fall down again to the anvil and again closes the circuit. This done the same motions begin anew, and are repeated as long as the pile retains its power. The hammer may be brought to any required distance from the anvil at pleasure, and mercury may be employed between them if thought necessary: and there is also a contrivance for elevating or depressing the anvil. These modifications of the apparatus permit the rapidity of the openings and closings to be varied in several ways.

The apparatus operates upon principles already known. The various combinations of the spiral wire serve the purpose of attaining various objects. If, for instance, we require a great *quantity* of the electric force, we must unite the ends of both spiral wires which are of the same name; by which means we shall have sparks and chemical decompositions at a maximum. On the other hand, if we desire a maximum of *intensity*, then we must unite the ends of the wires which are of *different* names, by which means the greatest effect is produced on inferior conducting bodies. The fiery sparks appear between the hammer and anvil. The decompositions and shocks are obtained by bringing the respective bodies into contact with one end of the spiral wire; also to the quicksilver shank to which the hammer is united, and to that pole of the series which touches the other end of the spiral. Of the experiments I shall mention only one; the deflagration of various metals, and particularly of quicksilver under water. The convulsions of this metal are shown by placing a drop of it under acidulated water and touching it with one end of the spiral wire; a rotation immediately commences in the water; and, by introducing a charcoal point, the phenomenon becomes striking and beautiful. The action on the human body is extremely powerful. When the spiral wire is only 400 feet long we find that lively shocks are given even when the poles are touched with dry fingers, which, by a somewhat stronger pressure, become increased to an insufferable degree. By a moderately weak contact, one hears a gentle crackling noise, which appears to arise from a series of small sparks passing through the insulating epidermis. By moistening the finger in water, one gets only a tolerable superficial connexion, which continues but for a few seconds when the action is strong. The intensity is sufficiently great to transmit shocks through a series of many persons who are connected with moistened hands. A very interesting experiment is made by the employment of two polar plates, by means of which we obtain a current through a mass of water, and the human body, or even a hand immersed between these plates, is acted upon by the current. In this electrical bath, independently of any direct contact with the polar plates, the immersed body deprives the water of the greatest part of the electrical current, and, consequently, a lively sensation is experienced at every point of the immersed part. The importance of such electrical baths for medical purposes is very easily perceived, and ought to be strictly attended to by the faculty.

Finally, by augmenting the length and thickness of the spiral wire, the force becomes exalted, and answers for every purpose; and one can predict with certainty that it is capable of decomposing the alkalies. It is better adapted for this purpose when, instead of wire, a copper band, (a strip of sheet copper) in about twelve even spirals, is wound round the axle, having the inner and outer ends prepared with quicksilver cups for the purpose of varying the connexions. On this plan my Reometer (an instrument described in Gehler's Phil. Dictionary, new edition, vol. vi., sec. 3, p. 2494) is constructed and brought into action.

VIII. *Notice from* DR. ROBERT HARE, Professor of Chemistry, &c., *respecting the fusion of platina, also respecting a new Ether, and a series of gaseous compounds formed with the elements of water.*[*]

I have by improvements in my process for fusing platina, succeeded in reducing twenty five ounces[†] of that metal to a state so liquid, that the containing cavity not being sufficiently capacious, about two ounces overflowed it, leaving a mass of twenty three ounces. I repeat that I see no difficulty in extending the power of my apparatus to the fusion of much larger masses.

---

[*] Communicated by the Author.

[†] Troy weight. The actual quantity fused was 12,250 grs.; the lump remaining weighed 10,937 grs.

When nitric acid or sulphuric acid with a nitrate is employed to generate ether, there must be an excess of two atoms of oxygen for each atom of the hyponitrous acid which enters into combination. This excess involves not only the consumption of a large proportion of alcohol, but also gives rise to several acids and to some volatile and acrid liquids.

It occurred to me that for the production of pure hyponitrous ether a hyponitrite should be used. The result has fully realized my expectations.

By subjecting hyponitrite of potassa or soda to alcohol and diluted sulphuric acid, I have obtained a species of ether which differs from that usually known as nitrous or nitric ether in being sweeter to the taste, more bland to the smell, and more volatile. It boils below 65° of F., and produces by its spontaneous evaporation a temperature of 0—15° F. On contact with the finger or tongue it hisses as water does with red hot iron. After being made to boil, if allowed to stand for some time at a temperature below its boiling point, ebullition may be renewed in it apparently at a temperature lower than that at which it had ceased. Possibly this apparent ebullition arises from the partial resolution of the liquid into an aeriform ethereal fluid, which escapes, both during the distillation of the liquid ether and after it has ceased, at a temperature below freezing. This aeriform product has been found partially condensible by pressure, into a yellow liquid, the vapor of which, when allowed to enter the mouth or nose, produced an impression like that of the liquid ether. I conjecture that it consists of nitric oxide, so united to a portion of the ether as to prevent the wonted reaction of this gas with atmospheric oxygen. Hence it does not produce red fumes on being mingled with air.

Towards the end of the ordinary process for the evolution of the sweet spirits of nitre, a volatile acrid liquid is created which affects the eyes and nose like mustard, or horse radish.

When the new ether as it first condenses is distilled from quick-lime, this earth becomes imbued with an essential oil which it yields to hydric ether. This oil may be afterwards isolated by the spontaneous evaporation of its solvent. It has a mixed odour, partly agreeable, partly unpleasant. From the affinity of its odor and that of common nitrous ether, I infer that it is one of the impurities which exist in that compound.

The new ether is obtained in the highest degree of purity, though in less quantity, by introducing the materials into a strong well ground stoppered bottle, refrigerated by snow and salt. After some time the ether will form a supernatant stratum, which may be separated by decomposition. Any acid, having a stronger affinity for the alkaline base than the hyponitrous acid, will answer to generate this ether. Acetic acid not only extricates but appears to combine with it, forming apparently a hyponitro-acetic ether.

I observed some years ago that when olefiant gas is inflamed with an inadequate supply of oxygen, carbon is deposited, while the resulting gas occupies double the space of the mixture before explosion. Of this I conceive I have discovered the explanation. By a great number of experiments, performed with the aid of my barometer gauge Eudiometer, I have ascertained that if during the explosion of the gaseous elements of water any gaseous or volatile inflammable matter be present, instead of condensing there will be a permanent gas formed by the union of the nascent water with the inflammable matter. Thus two volumes of oxygen, with four of hydrogen, and one of olefiant gas, give six volumes of permanent gas, which burns and smells like light carburetted hydrogen. The same quantity of the pure hydrogen and oxygen with half a volume of hydric ether gives on the average the same residue. One volume of the new hyponitrous ether under like circumstances produced five volumes of gas.

An analogous product is obtained when the same aqueous elements are inflamed in the presence of an essential oil. With oil of turpentine a gas was obtained weighing per hundred cubic inches $16\frac{5}{10}$ grs., which is nearly the gravity of light carburetted hydrogen. The gas obtained from olefiant gas, or from ether, weighed on the average, per the same bulk $13\frac{5}{10}$ grs. The olefiant gas which I used weighed per hundred cubic inches only $30\frac{5}{10}$ grs. Of course if per se expanded into six volumes it could have weighed only one sixth of that weight, or little over five grains per hundred cubic inches. There can therefore be no doubt that the gas obtained by the means in question, is chiefly constituted of water, or of its elements in the same proportion H^2O.

With a volume of the new ether, six volumes of the mixture of hydrogen and oxygen give on the average about five residual volumes. The gas created in either of the modes above mentioned does not contain carbonic acid, and when generated from olefiant gas appears by analysis to yield the same quantity of carbon and hydrogen as that gas affords before expansion.

These facts point out a source of error in experiments, for analyzing gaseous mixtures by ignition with oxygen or hydrogen, in which the consequent condensation is appealed to as a basis for an estimate. It appears that the resulting water may form new products with certain volatilizable substances which may be present.

IX. REVIEWS AND NOTICES OF NEW BOOKS.

A Course of Eight Lectures on Electricity, Galvanism, Magnetism, and Electro-magnetism. By HENRY M. NOAD, *Member of the London Electrical Society.* SCOTT, WEBSTER, *and* GEARY, 36, *Charter-House Square.*

Mr. Noad's Lectures, like some other recent publications purporting to treat on these subjects, are, principally, if not totally, compilations from other sources, to no one of which have they made even the slightest contribution. Mr. Noad has obviously read several authors on the subjects of his Lectures, and, of course, has selected those parts and parcels of their works which appeared most suitable to his purpose. It is our duty, however, in justice to its author, to state, that Mr. Noad has drawn, to a rather unusual extent, on Sir David Brewster's "Treatise on Magnetism," many parcels of which, already in a suitable dress for the "Lectures," of course, required only the mere process of transplantation from one work to the other. We do not, however, find fault with Mr. Noad for thus availing himself of the matter in its original elegant form, because it is well chosen and well adapted to the purpose; and, as he has not attempted to conceal his authorities, but, in general, has been very liberal in acknowledging them, his "Lectures" claim our best wishes for their success, sincerely hoping that they may produce a better effect than that of *beguiling an idle hour*, which the preface informs us is the only "object of the author" for offering them to public notice.

"*Magnetical Investigations.*" *By the* Rev. WM. SCORESBY, B. D., *Fellow of the Royal Societies of London and Edinburgh; Corresponding Member of the Institute of France, &c., &c.* LONGMAN, ORME, BROWN, GREEN, and LONGMANS, *Paternoster Row.*

We have, in this work, a series of magnetical observations and facts of exceedingly great interest, whether they be viewed in a theoretical point of view, or of practical applicability. The labours of Mr. Scoresby, as an experimental philosopher, have long been sufficiently known to the scientific world to establish his reputation as an indefatigable and exact investigator; and we have great pleasure in stating that the originality of many facts which we have observed in this neat little volume, do equal credit to the author with any of the preceding results of his valuable investigations.

Mr. Scoresby prefaces his interesting Investigations with the following series of "Introductory Observations," so admirably appropriated to the scientific importance, and theological dignity, of his subject.

"It had long been conjectured that a most intimate connexion, if not identity of nature, might probably exist among some of the more subtle and mysterious agents, or principles of different denominations, and, apparently, of different characteristics, which universally pervade the region in and about this earth.

"Modern discoveries in electro-magnetism, with the cognate relations which these have developed as existing in other principles of natural bodies, have gone far to verify these anticipations; and at the same time to yield so much additional knowledge of the constitution of the physical system of our planet, as to give a new, an interesting, and a prominent importance to, electrical and magnetical science.

"To the time of Dr. Gilbert, of Colchester, magnetism was only known as a mysterious virtue, existing in and peculiar to, the loadstone, or ferruginous substances which had been touched by this extraordinary mineral, from which certain qualities of attraction and direction were derived. But this eminent individual discovered, as he has left on record in his 'Physiologia Nova, sen Tractatus de Magnete et Corporibus Magnetisis;' published in the year 1600, that the phenomenon of the meridional adjustment of the magnetic needle was not owing to any mystical virtue, exercised out of the course of natural principles, but a mere result of the directive action of the earth; which he truly considered as the controlling agent, by reason of its being in its matter and constitution magnetic.

"So long as magnetism was known only as a separate or simple principle, the philosophic ideas of Dr. Gilbert were never materially advanced; but, on the discoveries of Professor Œrsted, whereby the long-suspected connexion betwixt electricity and magnetism was established, an amazing enlargement was at once yielded to magnetical knowledge, and a corresponding impulse given to magnetic research.

"The effect has been to give a science, formerly considered as comparatively of an inferior class, a grandeur of consideration; placing it at once amongst those mighty principles which infinite wisdom has appointed, and infinite power ordained as essential elements or agencies in the physical constitution of the world. For inasmuch as the inseparable connexion has been established betwixt electricity and magnetism, these, under various forms of development, reciprocally developing each other, it necessarily follows, that, to whatever extent in creation electricity operates, to the same extent the magnetic principle must reach. And inasmuch as heat, light, and chemical action, are each, more or less, developers of one form or another of the electro-magnetic principle, the analogies of science would lead us to infer, that magnetism is co-extensive with these other agencies, throughout their range of operation. Hence, there is little doubt but the principle, which, a few years ago, was known only as a director of the compass needle, as to its utility, and as little more than a curiosity in science, is one of the mighty energies by which, instrumentally, the works of the great Creator are regulated; one of those subtle powers which he hath ordained as his servants, 'fulfilling his word;' and whereby, 'the sweet influences' of the whole system of the universe are bound together, controlled, and upheld. Thus the subject of magnetism becomes of the highest consideration; in science, as to its mightiness and extent of operation; and, in natural theology, as calculated to connect the researches of human intelligence, with him who hath created these wonders; to elevate the feelings of reverence and adoration in the devotional mind, and to proclaim more clearly, in proportion as the invisible things are understood, 'His eternal power and Godhead.'

"It is not my object, however, in this publication, to carry out those views to which the more enlarged consideration of magnetism, as a science, might be advantageously applied; but that, in thus showing something of the importance of the subject as a science, I may solicit, for the contributions which are here offered for it, such reasonable consideration as, in this connexion, they may fairly claim.

"To the subject of *magnetism*, my attention has, for a series of years, been more or less directed; latterly with the view, particularly, of producing more powerful instruments for the determination of delicate variations in, and the actual condition of, the earth's magnetism; a subject which, from its greatly increased importance, is now engaging attention in some of the principal observatories in Europe.

"In contemplating such improvement in instruments dependent for their adjustment on the earth's magnetism, the grand desideratum would obviously be, the attainment of increased energy, or directive power, in magnetic needles, or bars of any given length or mass. And, that the attainment of such increased energy was a promising field of enquiry, I was satisfied, from the mere consideration of the surprising superiority in power of electro-magnets over permanent artificial magnets; strongly indicating the existence of a far greater capacity for magnetism, than we have hitherto been able to develop, or, if developed, to retain. Whilst this consideration yielded every encouragement to the enquiry, an experimental fact, in regard to the proportional power of magnets of *unequal thickness*, suggested that guidance, in pursuing the enquiry, which not only led (as will, I trust, subsequently appear) to a successful result in regard to the object particularly specified, but gave rise to investigations extending beyond my original design, and eminently calculated, I conceive, for the improvement of sea compasses—hitherto so very defective—as well as artificial magnets, and magnetic apparatus generally. The fact referred to was this. When examining, many years ago, the directive power of various artificial bar-magnets, for the purpose of determining the practicability of ascertaining the thickness of rocks, &c. in tunneling and mining, by the method of deviations communicated to the Royal Society in 1831,—the idea occurred to me, that, if the bars ordinarily employed for compass needles, &c. were divided into laminæ; or if, in other words, they were made up of thin plates to the extent of the masses of the bars commonly in use, a greater degree of energy would be obtained. Experiment fully justified this opinion. But previous to the application of the principle to instruments directed by the earth's magnetism, I had made trial of a combination of laminæ of *thoroughly tempered steel*, for the construction of a small compound magnet. The substance made use of was watch-spring, of which fourteen pieces, of two inches in length, were combined, after being magnetized, and formed a small magnet, weighing, in amount of steel, about one hundred grains. From a want of knowledge, at that time, of the best mode of magnetizing thin plates, the power obtained was much less than was expected; but when the same little instrument was subsequently magnetized, in a mass, by the process hereafter described, its efficiency became very striking,—the power being found to be such as to lift, by one pole, a polished nail of about 800 grains, or eight times its own weight.

"A trial apparatus, of the nature of a *variation needle*, on the same principle,—improved, however, for this purpose, by the *separation* of the plates—was constructed in the year 1836, which was exhibited to the 'British Association for the Promotion of Science,' the same year. But being without any *precise* knowledge of the laws of combination in magnetized plates, or even of the actual power of this instrument, though obviously great, a mere general idea only of its relative superiority could then be obtained. Since that time I have investigated, in an elaborate series of experiments, and with a somewhat expensive variety of apparatus, the principles of the construction adopted, so as satisfactorily to prove, I conceive, the decided advantage of that construction for sea, and other, compasses, and to apply the principle to various practical purposes in magnetics.

"In the original 'variation compass' just referred to, the plates, as I have intimated, were not placed in immediate contact, but separated by thin slips of wood or card-board; for I had previously found, when combining magnets for other purposes, that a material loss of power, in the individual intensities of the bars, was, in all cases, occasioned by combination: but that, when the combination was not made in contact, an inferior deterioration took place.

"The subjects to which I was primarily guided by these preliminary considerations and results, extended to the following particulars:—The effect of the division in various directions of the mass of steel, on its magnetic capabilities,— the law of combination of magnetized steel plates in contact—the law of combination when the plates are separated by limited spaces—the effect of temper or degree of hardness, and the degree of permanency of the power in combinations of magnetized steel plates. A few of the most important results obtained from these investigations, were forwarded to the Institute of France, in February, 1838. These investigations proved sufficient to show that the idea entertained in the outset, of practicability of producing, by means of combination of magnetized steel plates, more powerful apparatus than had hitherto been in use, for experiment and observation in magnetical science, and for practical purposes in magnetics generally, was not incorrectly founded. But the most important practical applications of these principles, with the results of several new and distinct investigations, yet remain behind. The description of these, in the first instance, is the object of the present publication; and it is hoped that the results will be found to develop some new and some improved principles of construction, applicable both to instruments designed to be directed by the earth's magnetism, and to the improvement of apparatus in which a permanent and concentrate energy are requisite; together with a useful application of some of the laws developed in these or previous investigations, to the *testing* of the quality of steel, and the determining of the degree of its hardness, purposes of the highest importance in the construction of delicate instruments used in the arts, or by professional men.

"The several results and applications of these recent personal researches, may be conveniently classed under separate heads, belonging to the development of principles, or of different practical processes, in magnetism. Some of these will, no doubt, be resolvable, in certain particulars, into principles or methods heretofore known; but, in all, it is presumed, something peculiar, as to decisiveness of the results bearing on principles, or as to convenience of adaptation, or efficacy of manipulation, or improvement in construction, in regard to the practical subjects, will be found."

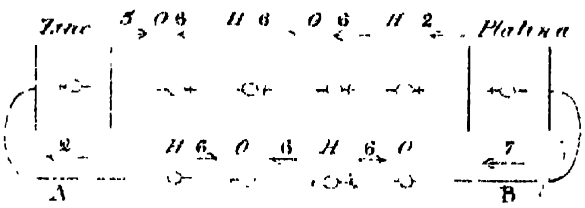
From the above prefatory chapter, our readers will discover that the objects of Mr. Scoresby's investigations are of a high scientific character; and we can assure them that we have not met with so valuable a work on magnetics as that before us, since the appearance of the second edition of Mr. Barlow's "Magnetic Attractions," a work of great merit, and intrinsic importance, in this branch of physics. It is such productions as these that every scientific man delights to peruse; and every scientific journalist ought to take a pleasure in recommending to his readers. Every novel fact that becomes developed by physical enquiries gives new impulses to the pursuit, implants an additional gem in the diadem of science, enhances the lustre of the whole, and, sooner or later, yields new sources of comfort and happiness to man.

X. MISCELLANEOUS ARTICLES.

Galvanic experiments on the body of an executed Murderer.

Coleman, a mulatto, who murdered his wife, was executed at New York on the 15th of Feb., 1839. After the body had hung for about a quarter of an hour it was cut down. Mr. Chilton, and several other scientific men, then operated in the following way on the corpse. The instrument used* in these experiments was a newly invented one, called a Galvanic Multiplier; the whole amount of zinc surface exposed to the acid was about one foot, and yet the shock produced is equal, if not greater, than that of a battery of 100 inch plates. 1*st Experiment.*—The lungs were filled with oxygen gas. The phrenic nerve and eighth pair were dissected in the neck; a metallic piece, having a number of points on it, was placed over the ribs, the points being inserted through the skin. The moment the lungs were filled with the gas, the galvanic current was passed from the nerves at the neck to the diaphragm. The object was to bring about respiration. The effect produced was, violent contraction of all the muscles, the chest heaved but no air appeared to enter the lungs, the head and neck were thrown on one side by the spasm produced. *2d.* The metallic piece was removed from the abdomen, and an incision was made through the cartilage of the seventh rib, one pole of the instrument was placed in the opening, so as to touch the diaphragm; the other was placed on the neck. The effect produced was similar to the first. *3d.* The posterior tibial nerve at the heel was exposed; one pole applied to this the other to the neck. Effect—the muscle of the leg was thrown into action, with convulsive movements of the body. *4th.* One pole was held at the tibian nerve—the mouth was then opened, and the other pole put into it. The moment it touched the tongue the teeth became firmly clenched, and held so hard on to the wire as to require considerable force to extricate it. This was repeated several times. *5th.* The next experiment was to try the effect produced by merely applying the poles of the instrument to the surface of the body, previously wetting its parts with a saline solution, to render the contact more perfect. The effects on the body appeared quite as great as when the large nerves were touched. The poles of the apparatus were placed in the above manner, one to the leg, the other to different parts of the face. The facial muscles were alternately thrown into action as the different nerves of the face were touched. The effect of this was terrific in the extreme. Every muscle of the grim murderer's countenance was thrown into the most horrible contortions: rage, horror, anguish, and despair, the most rapid smiles, the most hideous expressions of contempt and hatred, by turns were depicted on his countenance, and gave a fearful wildness to his face, which far surpassed even the most vivid imagination from Fuseli's brain, or Kean's scenic display that we ever witnessed. Several of the audience were excessively appalled; some left in double quick time, and many confessed, that, if they had staid, they certainly should have fainted. At one part of the operations, when the murderer raised his right arm and passed it in different directions, we saw the cheeks of several stout hearted fellows blanched with fear: and one, whose name we do not wish to mention, actually whispered "sure, he has come to life." Above an hour was spent in the experiments, and then the prison was cleared and the body removed under the directions of the surgeons.—*Jersey Times and Naval and Military Chronicle.*

Fig 68.



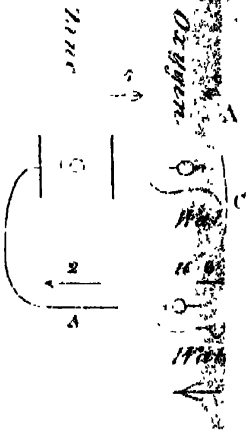
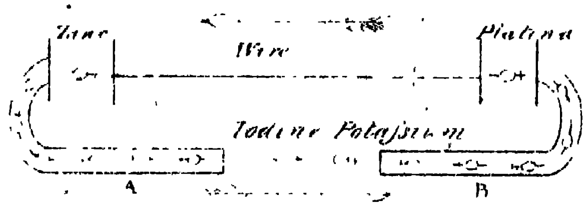


Fig 72.



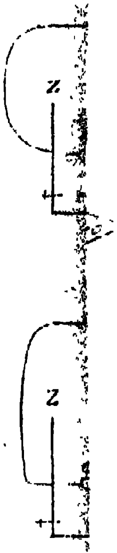


Fig. 76.

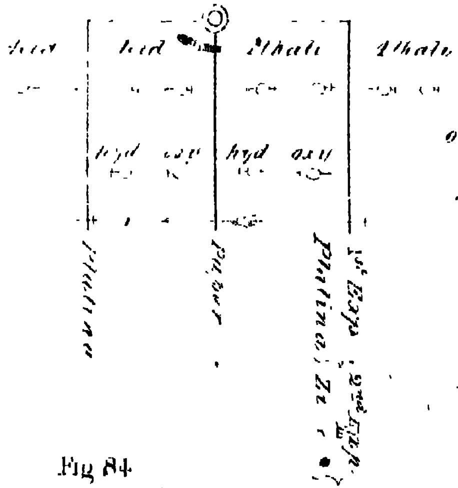


Fig 77.

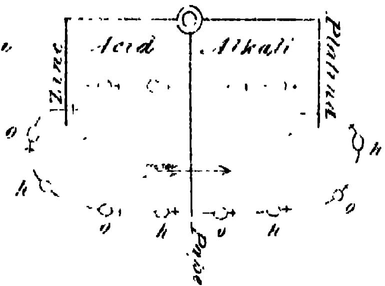


Fig 80.

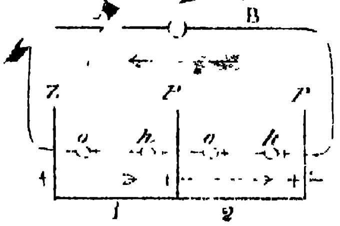


Fig 84.

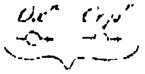


Fig 87.

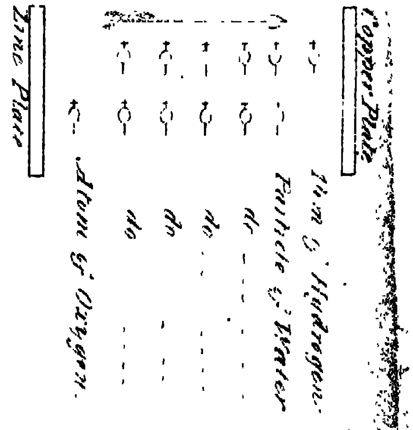


Fig 82.

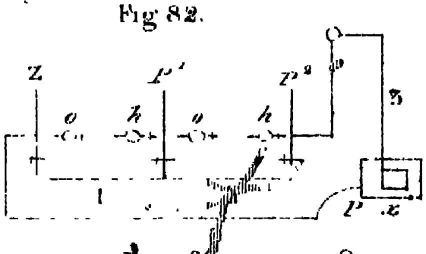


Fig 83.

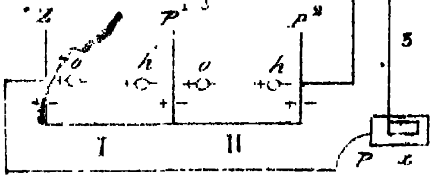
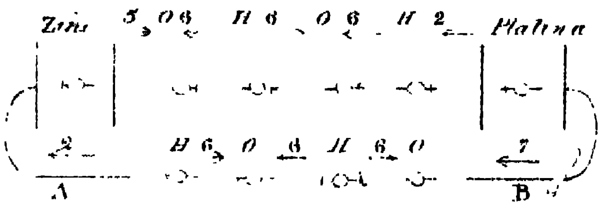


Fig. 68.



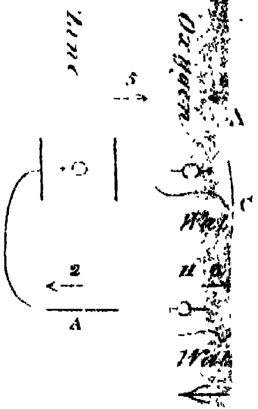
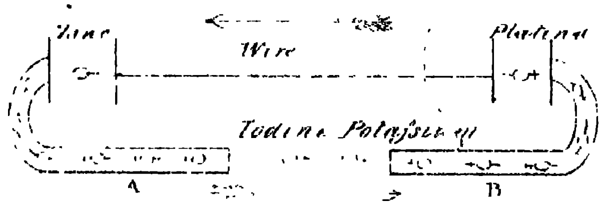


Fig. 72



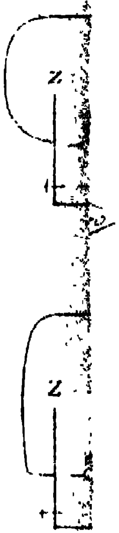


Fig. 76

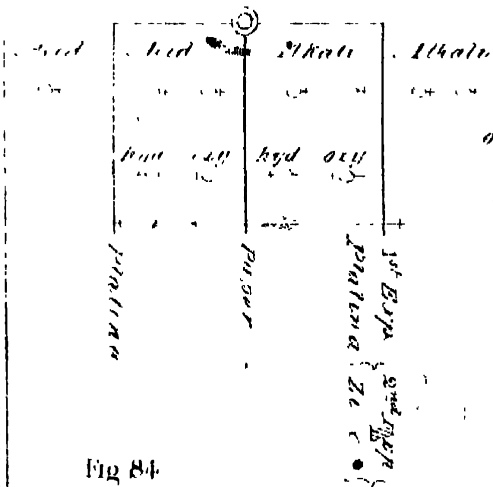


Fig. 77.

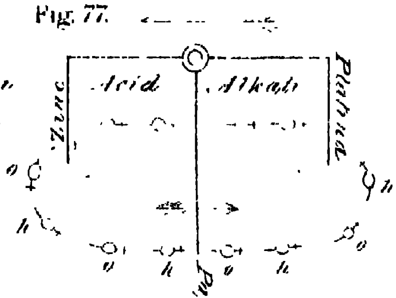


Fig. 80.

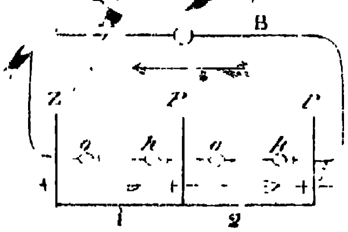


Fig. 84.

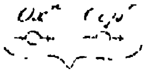


Fig. 87.

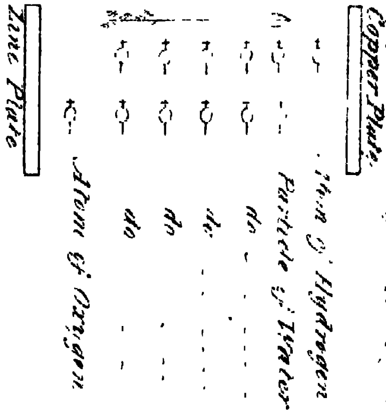


Fig. 82

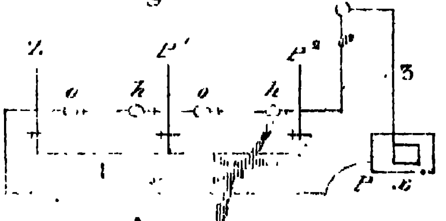
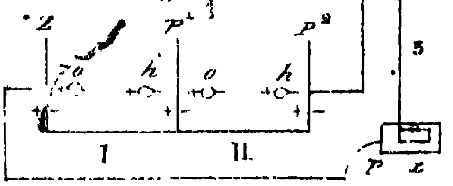
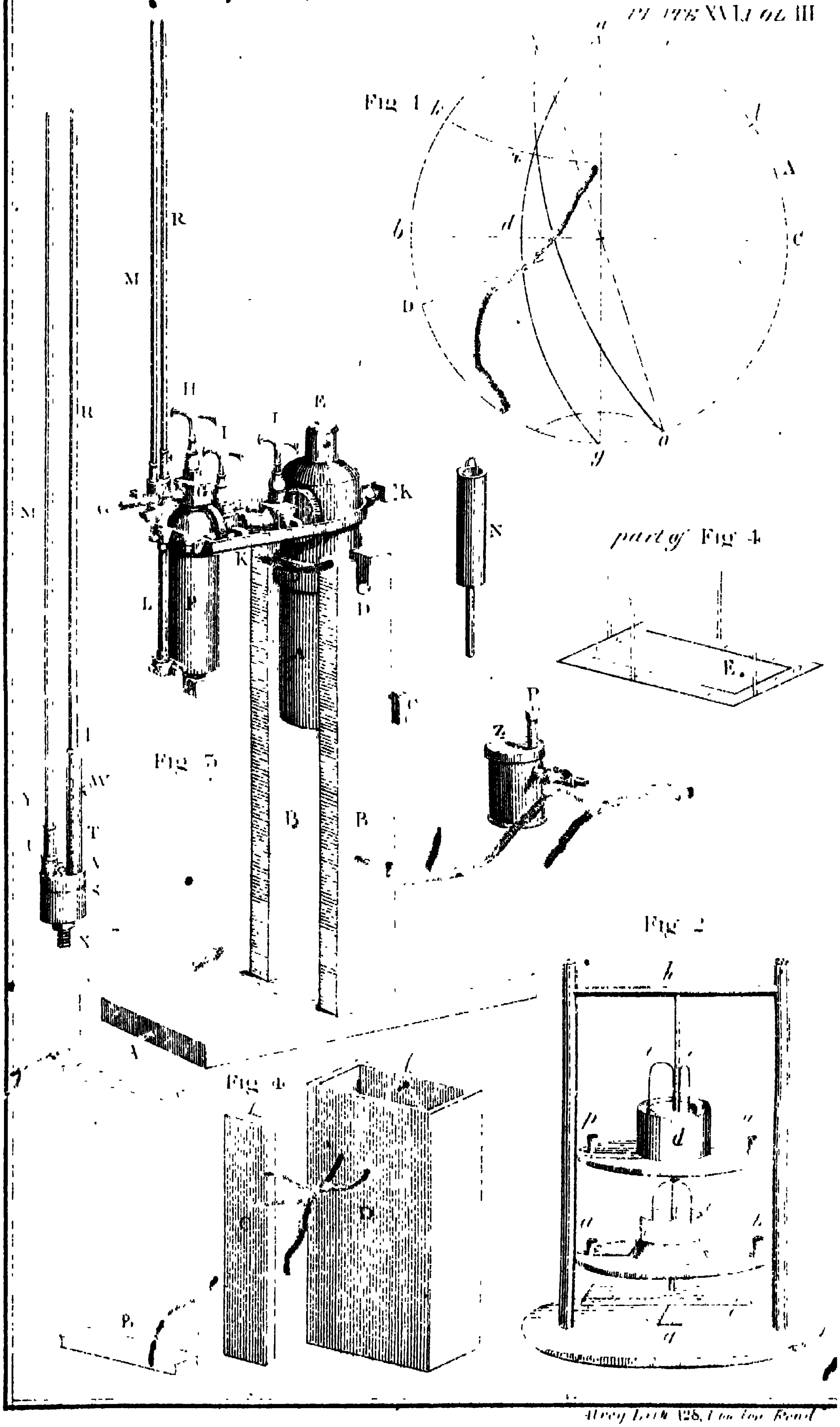


Fig. 83.

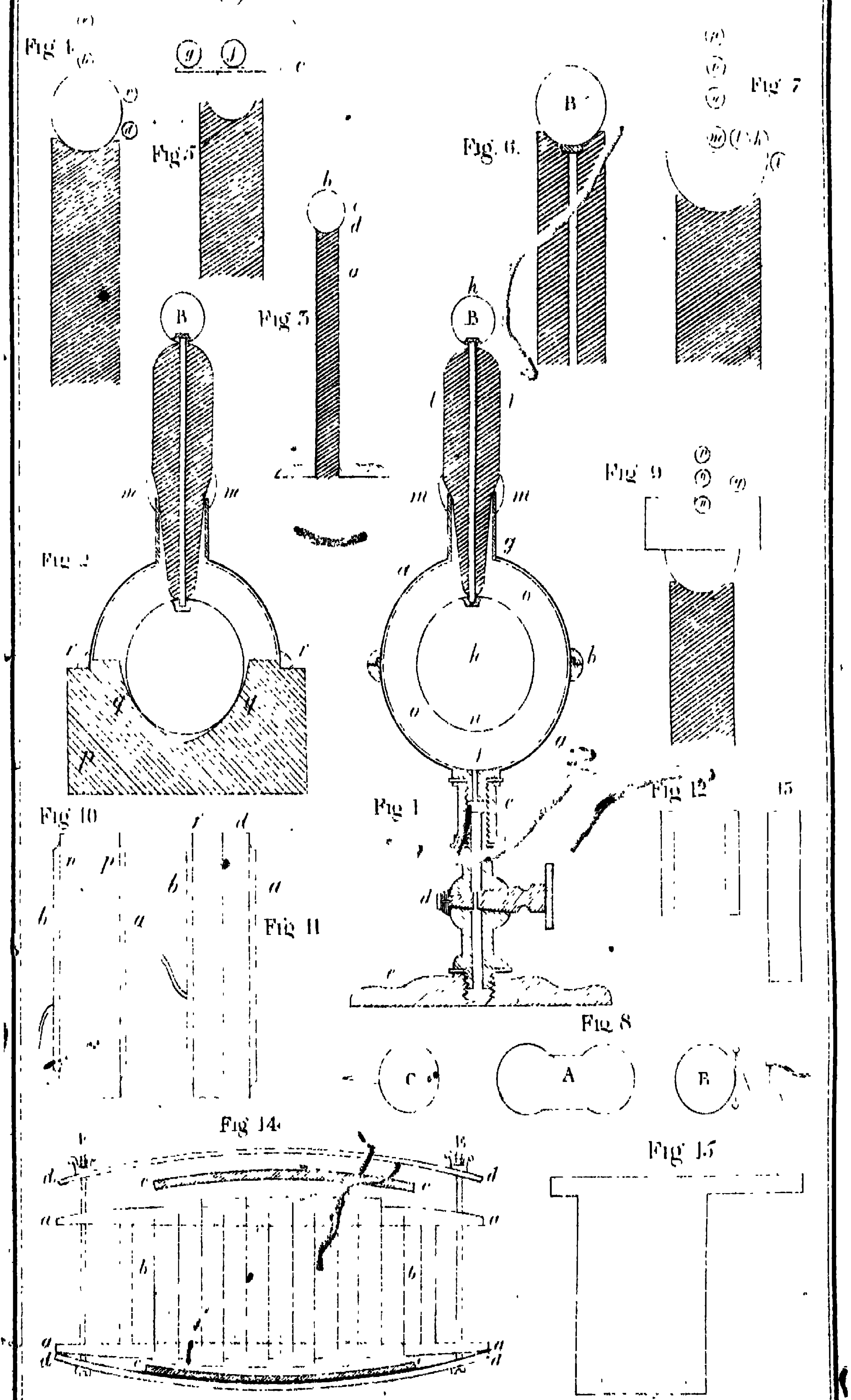




Alex. Lith. 128, London Road

ANNALS OF ELECTRICITY &c.

PLATE I. Vol. IV.



ANNALS OF ELECTRICITY,

Fig. 1

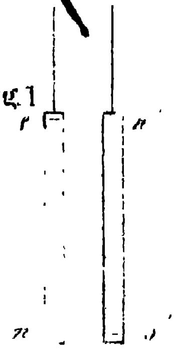


Fig. 2

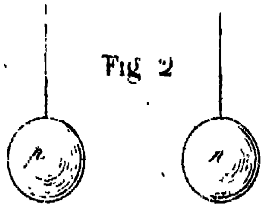


Fig. 3.

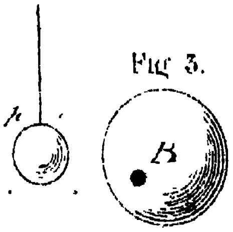


Fig. 5.

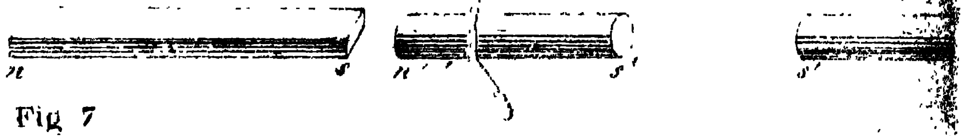


Fig. 7

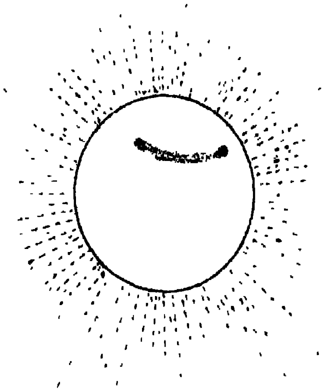


Fig. 8

Fig. 9.

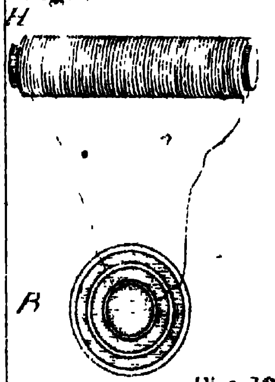


Fig. 10

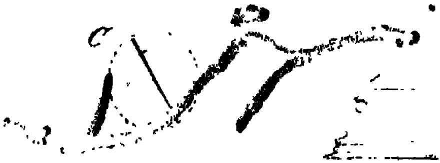
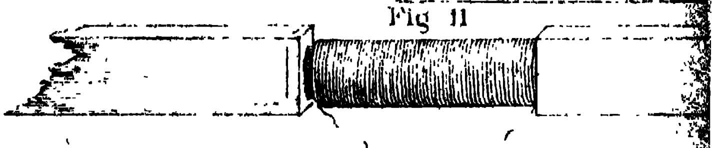


Fig. 11



C

Fig. 12.

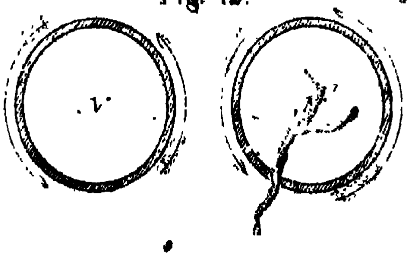
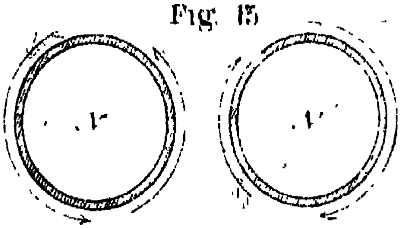


Fig. 13



Alvey Lith. London Road.

Bradshaw & Blacklock Man

RICHARD I. LEAVING CYPRUS.

Bradshaw & Blacklock of Brown St. Manch.

PRINTED FROM AN ELECTROTYPE, MADE AT THE ROYAL VICTORIA GALLERY

THE ANNALS
OF
ELECTRICITY, MAGNETISM,
AND CHEMISTRY;
AND
Guardian of Experimental Science.

DECEMBER, 1840.

Experimental Researches in Electricity, by MICHAEL FARADAY, D.C.L. F.R.S., &c.

FOURTEENTH SERIES.

LII.—§ 20. *Nature of the electric force or forces* § 21. *Relation of the electric and magnetic forces.* § 22. *Note on electrical excitation.*

Received June 21, 1838.—Read June 21, 1838.

§ 20. *Nature of the electric force or forces.*

1667. The theory of induction set forth and illustrated in the three preceding series of experimental researches does not assume anything new as to the nature of the electric force or forces, but only as to their distribution. The effects may depend upon the association of one electric fluid with the particles of matter, as in the theory of Franklin, Epinus, Cavendish, and Mossotti; or they may depend upon the association of two electric fluids, as in the theory of Dufay and Poisson; or they may not depend upon anything which can properly be called the electric fluid, but on vibrations or other affections of the matter in which they appear. The theory is unaffected by such differences in the mode of viewing the nature of the forces; and though it professes to perform the important office of stating *how* the powers are arranged (at least in inductive phenomena), it does not, as far as I can yet perceive, supply a single experiment which can be considered as a distinguishing test of the truth of any one of these various views.

1668. But, to ascertain how the forces are arranged, to trace them in their various relations to the particles of matter, to determine their general laws, and also the specific differences which occur under these laws, is as important as, if not more so than, to know whether the forces reside in a fluid or not; and with the hope of assisting in this research, I shall offer some further developments, theoretical and experimental, of the conditions under which I suppose the particles of matter are placed when exhibiting inductive phenomena.

1669. The theory assumes that all the *particles*, whether of insulating or conducting matter, are as wholes conductors.

1670. That not being polar in their normal state, they can become so by the influence of neighbouring charged particles, the polar state being developed at the instant, exactly as in an insulated conducting *mass*, consisting of many particles.

1671. That the particles when polarized are in a forced state, and tend to return to their normal or natural condition.

1672. That being as wholes conductors, they can readily be charged, either *bodily* or *polarly*.

1673. That particles which being contiguous are also in the line of inductive action can communicate or transfer their polar forces one to another *more or less* readily.

1674. That those doing so less readily require the polar forces to be raised to a higher degree before this transference or communication takes place.

1675. That the *ready* communication of forces between contiguous particles constitutes *conduction*, and the *difficult* communication *insulation*; conductors and insulators being bodies whose particles naturally possess the property of communicating their respective forces easily or with difficulty; having these differences just as they have differences of any other natural property.

1676. That ordinary induction is the effect resulting from the action of matter charged with excited or free electricity upon insulating matter, tending to produce in it an equal amount of the contrary state.

1677. That it can do this only by polarizing the particles contiguous to it, which perform the same office to the next, and these again to those beyond; and that thus the action is propagated from the excited body to the next conducting mass, and there renders the contrary force evident in consequence of the effect of communication which supervenes in the conducting mass upon the polarization of the particles of that body (1675.).

1678. That therefore induction can only take place through or across insulators; that induction is insulation, it being the necessary consequence of the state of the particles and the mode in which the influence of electrical forces is transferred or transmitted through or across such insulating media.

1679. The particles of an insulating dielectric whilst under induction may be compared to a series of small magnetic needles, or more correctly still to a series of small insulated conductors. If the space round a charged globe were filled with a mixture of an insulating dielectric, as oil of turpentine or air, and small globular conductors, as shot, the latter being at a little distance from each other so as to be insulated, then these would in their condition and action exactly resemble what I consider to be the condition and action of the particles of the insulating dielectric itself (1337). If the globe were charged, these little conductors would all be polar; if the globe were discharged, they would all return to the r normal state, to be polarized again upon the recharging of the globe. The state developed by induction through such particles on a mass of conducting matter at a distance would be of the contrary kind, and exactly equal in amount to the force in the inductric globe. There would be a lateral diffusion of force (1224. 1297.), because each polarized sphere would be in an active or tense relation to all those contiguous to it, just as one magnet can affect two or more magnetic needles near it, and these again a still greater number beyond them. Hence would result the production of curved lines of inductive force if the inducteous body in such a mixed dielectric were an uninsulated metallic ball (1219, &c.) or other properly shaped mass. Such curved lines are the consequences of the two electric forces arranged as I have assumed them to be: and, that the inductive force can be directed in such curved lines is the strongest proof of the presence of the two powers and the polar condition of the dielectric particles.

1680. I think it is evident, that in the case stated, action at a distance can only result through an action of the contiguous conducting particles. There is no reason why the inductive body should polarize or affect *distant* conductors and leave those *near* it, namely the particles of the dielectric, unaffected: and everything in the form of fact and experiment with conducting masses or particles of a sensible size contradicts such a supposition.

1681. A striking character of the electric power is that it is limited and exclusive, and that the two forces being always present are exactly equal in amount. The forces are related in one of two ways, either as in the natural normal condition of an uncharged insulated conductor; or as in the charged state, the latter being a case of induction.

1682. Cases of induction are easily arranged so that the two forces being limited in their direction shall present no phenomena or indications external to the apparatus employed. Thus, if a Leyden jar, having its external coating a little higher than the internal, be charged and then its charging ball and rod removed, such jar will present no electrical appearances so long as its outside is uninsulated. The two forces which may be said to be in the coatings, or in the particles of the dielectric contiguous to them, are entirely engaged to each other by induction through the glass; and a carrier ball (1181.) applied either to the inside or outside of the jar will show no signs of electricity. But if the jar be insulated, and the charging ball and rod, in an uncharged state and suspended by an insulating thread of white silk, be restored to their place, then the part projecting above the jar will give electrical indications and charge the carrier, and at the same time the *outside* coating of the jar will be found in the opposite state and inductric towards external surrounding objects.

1683. These are simple consequences of the theory. Whilst the charge of the inner coating could induce only through the glass towards the outer coating, and the latter contained no more of the contrary force than was equivalent to it, no induction external to the jar could be perceived; but when the inner coating was extended by the rod and ball so that it could induce through the air towards external objects, then the tension of the polarized glass molecules would, by their tendency to return to the normal state, fall a little, and a portion of the charge passing to the surface of this new part of the inner conductor, would produce inductive action through the air towards distant objects, whilst at the same time a part of the force in the outer coating previously directed inwards would now be at liberty, and indeed be constrained to inductric outwards through the air, producing in that outer coating what is sometimes called, though I think very improperly, free charge. If a small Leyden jar be converted into that form of apparatus usually known by the name of the electric well, it will illustrate this action very completely.

1684. The terms *free charge* and *dissimulated electricity* convey therefore erroneous notions if they are meant to imply any difference as to the mode or kind of action. The charge upon an insulated conductor in the middle of a room is in the same relation to the walls of that room as the charge upon the inner coating of a Leyden jar is to the outer coating of the same jar. The one is not more *free* or more *dissimulated* than the other; and when sometimes we make electricity appear where it was not evident before, as upon the outside of a charged jar, when, after insulating it, we touch the inner coating, it is only because we divert more or less of the inductive force from one direction into another; for not the slightest change is in such circumstances impressed upon the character or action of the force.

1685. Having given this general theoretical view, I will now notice particular points relating to the nature of the assumed electric polarity of the insulating dielectric particles.

1686. The polar state may be considered in common induction as a forced state, the particles tending to return to their normal condition. It may probably be raised to a very high degree by approximation of the inductric and inducteous bodies or by other circumstances; and the phenomena of electrolyzation (861. 1652. 1706.) seem to imply that the quantity of power which can thus be accumulated on a single particle is enormous. Hereafter we may be able to compare corpuscular forces, as those of gravity, cohesion, electricity, and chemical affinity, and in some way or other from their effects deduce their relative equivalents; at present we are not able to do so, but there seems no reason to doubt that their electrical, which are at the same time their chemical forces (891. 918.), will be by far the most energetic.

1687. I do not consider the powers when developed by the polarization as limited to two distinct points or spots on the surface of each particle to be considered as the poles of an axis, but as resident on large portions of that surface, as they are upon the surface of a conductor of sensible size when it is thrown into a polar state. But it is very probable, notwithstanding, that the particles of different bodies may present specific differences in this respect, the powers not being equally diffused though equal in quantity; other circumstances also, as form and quality, giving to each a peculiar polar relation. It is perhaps to the existence of some such differences as these that we may attribute the specific actions of the different dielectrics in relation to discharge (1394. 1508.) Thus with respect to oxygen and nitrogen singular contrasts were presented when spark and brush discharge were made to take place in these gases, as may be seen by reference to the Table in paragraph 1518 of the Thirteenth Series; for with nitrogen, when the small negative or the large positive ball was rendered inductric, the effects corresponded with those which in oxygen were produced when the small positive or the large negative ball was rendered inductric.

1688. In such solid bodies as glass, lac, sulphur, &c., the particles appear to be able to become polarized in all directions, for a mass when experimented upon so as to ascertain its inductive capacity in three or more directions (1690.), gives no indication of a difference. Now as the particles are fixed in the mass, and as the direction of the induction through them must change with its change relative to the mass, the constant effect indicates that they can be polarized electrically in any direction. This accords with the view already taken of each particle as a whole being a conductor (1669.), and, as an experimental fact, helps to confirm that view.

1689. But though particles may thus be polarized in *any* direction under the influence of powers which are probably of extreme energy (1686.), it does not follow that each particle may not tend to polarize to a greater degree, or with more facility, in one direction than another; or that different kinds may not have specific differences in this respect, as they have differences of conducting and other powers (1296. 1326. 1395). I sought with great anxiety for a relation of this nature; and selecting crystalline bodies as those in which all the particles are symmetrically placed, and therefore best fitted to indicate any result which might depend upon variation of the direction of the forces to the direction of the particles in which they were developed, experimented very carefully with them. I was the more strongly stimulated to this inquiry by the beautiful electrical condition of the crystalline bodies tourmaline and boracite, and hoped also to discover a relation between electric polarity and that of crystallization, or even of cohesion itself (1316.). My experiments have not established any connexion of the kind sought for. But as I think it of equal importance to show either that there is or is not such a relation, I shall briefly describe the results.

1690. The form of experiment was as follows. A brass ball 0·73 of an inch in diameter, fixed at the end of a horizontal brass rod, and that at the end of a brass cylinder, was by means of the latter connected with a large Leyden battery (291.) by perfect metallic communications, the object being to keep that ball, by its connexion with the charged battery in an electrified state, very nearly uniform, for half an hour at a time. This was the inductric ball. The inducteous ball was the carrier of the torsion electrometer (1229. 1314.); and the dielectric between them was a cube cut from a crystal, so that two of its faces should be perpendicular to the optical axis, whilst the other four were parallel to it. A small projecting piece of shell-lac was fixed on the inductric ball at that part opposite to the attachment of the brass rod, for the purpose of preventing actual contact between the ball and the crystal cube. A coat of shell-lac was also attached to that side of the carrier ball which was to be towards the cube, being also that side which was furthest from the repelled ball in the electrometer when placed in its position in that instrument. The cube was covered with a thin coat of shell-lac dissolved in alcohol, to prevent the deposition of damp upon its surface from the air. It was supported upon a small table of shell-lac fixed on the top of a stem of the same substance, the latter being of sufficient strength to sustain the cube, and yet flexible enough from its length to act as a spring, and allow the cube to bear, when in its place, against the shell-lac on the inductric ball.

1691. Thus it was easy to bring the inducteous ball always to the same distance from the inductric ball, and to uninsulate and insulate it again in its place; and then, after measuring the force in the electrometer (1181.), to return it to its place opposite to the inductric ball for a second observation. Or it was easy by revolving the stand which supported the cube to bring four of its faces in succession towards the inductric ball, and so observe the force when the lines of inductive action (1304.) coincided with, or were transverse to, the direction of the optical axis of the crystal. Generally from twenty to twenty-eight observations were made in succession upon the four vertical faces of a cube, and then an average expression of the inductive force was obtained, and compared with similar averages obtained at other times, every precaution being taken to secure accurate results.

1692. The first cube used was of *rock crystal*; it was 0·7 of an inch in the side. It presented a remarkable and constant difference, the average of not less than 197 observations, giving 100 for the specific inductive capacity in the direction coinciding with the optical axis of the cube, whilst 93·59 and 93·31 were the expressions for the two transverse directions.

1693. But with a second cube of rock crystal corresponding results were not obtained. It was 0·77 of an inch in the side. The average of many experiments gave 100 for the specific inductive capacity coinciding with the direction of the optical axis, and 98·6 and 99·92 for the two other directions.

1694. Lord Ashley, whom I have found ever ready to advance the cause of science, obtained for me the loan of three globes of rock crystal belonging to Her Grace the Duchess of Sutherland for the purposes of this investigation. Two had such fissures as to render them unfit for the experiments (1193. 1698.). The third, which was very superior, gave me no indications of any difference in the inductive force for different directions.

1695. I then used cubes of Iceland spar. One 0·5 of an inch in diameter gave 100 for the axial direction, and 98·66 and 95·74 for the two cross directions. The other, 0·8 of an inch in the side, gave 100 for the axial direction, whilst 101·73 and 101·86 were the numbers for the cross direction.

1696. Besides these differences there were others, which I do not think it needful to state, since the main point is not confirmed. For though the experiments with the first cube raised great expectation, they have not been generalized by those which followed. I have no doubt of the results as to that cube, but they cannot as yet be referred to crystallization. There are in the cube some faintly coloured layers parallel to the optical axis, and the matter which colours them may have an influence; but then the layers are also nearly parallel to a cross direction, and if at all influential should show some effect in that direction also, which they did not.

1697. In some of the experiments one half or one part of a cube showed a superiority to another part, and this I could not trace to any charge the different parts had received. It was found that the varnishing of the cubes prevented any communication of charge to them, except (in a few experiments) a small degree of the negative state, or that which was contrary to the state of the inductric ball (1564. 1566.).

1698. I think it right to say that, as far as I could perceive, the insulating character of the cubes used was perfect, or at least so nearly perfect, as to bear a comparison with shell-lac, glass, &c. (1255.). As to the cause of the differences, other than regular crystalline structure, there may be several. Thus minute fissures in the crystal insensible to the eye may be so disposed as to produce a sensible electrical difference (1193.). Or the crystallization may be irregular; or the substance may not be quite pure; and if we consider how minute a quantity of matter will alter greatly the conducting power of water, it will seem not unlikely that a little extraneous matter diffused through the whole or part of a cube, may produce effects sufficient to account for all the irregularities of action that have been observed.

1699. An important inquiry regarding the electrical polarity of the particles of an insulating dielectric, is, whether it be the molecules of the particular substance acted on, or the component or ultimate particles, which thus act the part of insulated conducting polarizing portions (1669.).

1700. The conclusion I have arrived at is, that it is the molecules of the substance which polarize as wholes (1347.); and that however complicated the composition of a body may be, all those particles or atoms which are held together by chemical affinity to form one molecule of the resulting body, act as one conducting mass or particle when inductive phenomena and polarization are produced in the substance of which it is a part.

1701. This conclusion is founded on several considerations. Thus if we observe the insulating and conducting power of elements when they are used as dielectrics, we find some, as sulphur, phosphorous, chlorine, iodine, &c, whose particles insulate, and therefore polarize in a high degree; whereas others, as the metals, give scarcely any indication of possessing a sensible proportion of this power (1328.), their particles freely conducting one to another. Yet when these enter into combination they form substances having no direct relation apparently, in this respect, to their elements; for water, sulphuric acid, and such compounds formed of insulating elements, conduct by comparison freely; whilst oxide of lead, flint glass, borate of lead, and other metallic compounds containing very high proportions of conducting matter, insulate excellently well. Taking oxide of lead therefore as the illustration, I conceive that it is not the particles of oxygen and lead which polarize separately under the act of induction, but the molecules of oxide of lead which exhibit this effect, all the elements of one particle of the resulting body, being held together as parts of one conducting individual by the bonds of chemical affinity; which is but another term for electrical force (918.).

1702. In bodies which are electrolytes we have still further reason for believing in such a state of things. Thus when water, chloride of tin, iodide of lead, &c. in the solid state are between the electrodes of the voltaic battery, their particles polarize as those of any other insulating dielectric do (1164.); but when the liquid state is conferred on these substances, the polarized particles divide, the two halves, each in a highly charged state, travelling onwards until they meet other particles in an opposite and equally charged state, with which they combine, to the neutralization of their chemical, i. e. their electrical forces, and the reproduction of compound particles, which can again polarize as wholes, and again divide to repeat the same series of actions (1347.).

1703. But though electrolytic particles polarize as wholes, it would appear very evident that in them it is not a matter of entire indifference *how* the particle polarizes (1689.), since, when free to move (380, &c.) the polarities are ultimately distributed in reference to the elements; and sums of force equivalent to the polarities, and very definite in kind and amount, separate, as it were, from each other, and travel onwards with the elementary particles. And though I do not pretend to know what an atom is, or how it is associated or endowed with electrical force, or how this force is arranged in the cases of combination and decomposition, yet the strong belief I have in the electrical polarity of particles when under inductive action, and the bearing of such an opinion on the general effects of induction, whether ordinary or electrolytic, will be my excuse, I trust, for a few hypothetical considerations.

1704. In electrolyzation it appears that the polarized particles would (because of the gradual change which has been induced upon the chemical, i. e. the electrical forces of their elements (918.) rather divide than discharge to each other without division (1348.); for if, their division, i. e. their decomposition and recombination, be prevented by giving them the solid state, then they will insulate electricity perhaps a hundredfold more intense than that necessary for their electrolyzation (419, &c.). Hence the tension necessary for direct conduction in such bodies appears to be much higher than that for decomposition (419. 1164. 1344.).

1705. The remarkable stoppage of electrolytic conduction by solidification (380. 1358.), is quite consistent with these views of the dependence of that process on the polarity which is common to all insulating matter when under induction, though attended by such peculiar electro-chemical results in the case of electrolytes. Thus it may be expected that the first effect of induction is so to polarize and arrange the particles of water that the positive or hydrogen pole of each shall be from the positive electrode and towards the negative electrode, whilst the negative or oxygen pole of each shall be in the contrary direction; and thus when the oxygen and hydrogen of a particle of water have separated, passing to and combining with other hydrogen and oxygen particles, unless these new particles of water could turn round they could not take up that position necessary for their successful electrolytic polarization. Now solidification, by fixing the water particles and preventing them from assuming that essential preliminary position, prevents also their electrolysis (413.); and so the transfer of forces an that manner being prevented (1347. 1703.), the substance acts is an ordinary insulating dielectric (for it is evident by former experiments (419. 1704.) that the insulating tension is higher than the electrolytic tension), induction through it rises to a higher degree, and the polar condition of the molecules as wholes, though greatly exalted, is still securely maintained.

1706. When decomposition happens in a fluid electrolyte, I do not suppose that all the molecules in the same sectional plane (1634.) part with and transfer their electrified particles or elements at once. Probably the *discharge force* for that plane is summed up on one or a few particles, which decomposing, travelling and recombining, restore the balance of forces, much as in the case of spark disruptive discharge (1406.); for as those molecules resulting from particles which have just transferred power must by their position (1705.) be less favourably circumstanced than others, so there must be some which are most favourably disposed, and these, by giving way first, will for the time lower the tension and produce discharge.

1707. In former investigations of the action of electricity (821, &c.) it was shown, from many satisfactory cases, that the quantity of electric power transferred onwards was in proportion to and was definite for a given quantity of matter moving as anion or cathion onwards in the electrolytic line of action; and there was strong reason to believe that each of the particles of matter then dealt with, had associated with it a definite amount of electrical force, constituting its force of chemical affinity, the chemical equivalents and the electro-chemical equivalents being the same (836.). It was also found with few, and I may now perhaps say with no exceptions (1341.), that only those compounds containing elements in single proportions could exhibit the characters and phenomena of electrolytes (697.); oxides, chlorides, and other bodies containing more than one proportion of the electro negative element refusing to decompose under the influence of the electric current.

1708. Probable reasons for these conditions and limitations arise out of the molecular theory of induction. Thus when a liquid dielectric, as chloride of tin, consists of molecules, each composed of a single particle of each of the elements, then as these can convey equivalent opposite forces by their separation in opposite directions, both decomposition and transfer can result. But when the molecules, as in the bichloride of tin, consist of one particle or atom of one element, and two of the other, then the simplicity with which the particles may be supposed to be arranged and to act, is destroyed. And, though it may be conceived that when the molecules of bichloride of tin are polarized as wholes by the induction across them, the positive polar force might accumulate on the one particle of tin whilst the negative polar force accumulated on the two particles of chlorine associated with it, and that these might respectively travel right and left to unite with other two of chlorine and one of tin, in analogy with what happens in cases of compounds consisting of single proportions, yet this is not altogether so evident or probable. For when a particle of tin combines with two of chlorine, it is difficult to conceive that there should not be some relation of the three in the resulting molecule analogous to fixed position, the one particle of metal being perhaps symmetrically placed in relation to the two of chlorine: and, it is not difficult to conceive of such particles that they could not assume that position dependent both on their polarity and the relation of their elements, which appears to be the first step in the process of electrolyzation (1345. 1705.).

§. 21. *Relation of the electric and magnetic forces.*

1709. I have already ventured a few speculations respecting the probable relation of magnetism, as the transverse force of the current, to the divergent or transverse force of the lines of inductive action belonging to static electricity (1658, &c.).

1710. In the further consideration of this subject it appeared to me to be of the utmost importance to ascertain, if possible, whether this lateral action which we call magnetism, or sometimes the induction of electrical currents (26. 1048, &c.), is extended to a distance *by the action of the intermediate particles* in analogy with the induction of static electricity, or the various effects, such as conduction, discharge, &c., which are dependent on that induction; or, whether its influence at a distance is altogether independent of such intermediate particles (1662.).

1711. I arranged two magneto-electric helices with iron cores end to end, but with an interval of an inch and three quarters between them, in which interval was placed the end or pole of a bar magnet. It is evident, that on moving the magnetic pole from one core towards the other, a current would tend to form in both helices, in the one because of the lowering, and in the other because of the strengthening of the magnetism induced in the respective soft iron cores. The helices were connected together, and also with a galvanometer, so that these two currents should coincide in direction, and tend by their joint force to deflect the needle of the instrument. The whole arrangement was so effective and delicate, that moving the magnetic pole about the eighth of an inch to and fro two or three times, in periods equal to those required for the vibrations of the galvanometer needle, was sufficient to cause considerable vibration in the latter; thus showing readily the consequence of strengthening the influence of the magnet on the one core and helix, and diminishing it on the other.

1712. Then without disturbing the distances of the magnet and cores, plates of substances were interposed. Thus calling the two cores A and B, a plate of shell-lac was introduced between the magnetic pole and A for the time occupied by the needle in swinging one way; then it was withdrawn for the time occupied in the return swing; introduced again for another equal portion of time; withdrawn for another portion, and so on eight or nine times; but not the least effect was observed on the needle. In other cases the plate was alternated, i. e. it was introduced between the magnet and A for one period of time, withdrawn and introduced between the magnet and B for the second period, withdrawn and restored to its first place for the third period, and so on, but with no effect on the needle.

1713. In these experiments *shell-lac* in plates 0·9 of an inch in thickness, *sulphur* in a plate 0·9 of an inch in thickness, and *copper* in a plate 0·7 of an inch in thickness were used without any effect. And I conclude that bodies, contrasted by the extremes of conducting and insulating power, and opposed to each other as strongly as metals, air, and sulphur, show no difference with respect to magnetic forces when placed in their lines of action, at least under the circumstances described.

1714. With a plate of iron, or even a small piece of that metal, as the head of a nail, a very different effect was produced, for then the galvanometer immediately showed its sensibility, and the perfection of the general arrangement.

1715. I arranged matters so that a plate of *copper* 0·2 of an inch in thickness, and ten inches in diameter, should have the part near the edge interposed between the magnet and the core, in which situation it was first rotated rapidly, and then held quiescent alternately, for periods according with that required for the swinging of the needle; but not the least effect upon the galvanometer was produced

1716. A plate of shell-lac 0·6 of an inch in thickness was applied in the same manner, but whether rotating or not it produced no effect.

1717. Occasionally the plane of rotation was directly across the magnetic curve: at other times it was made as oblique as possible; the direction of the rotation being also changed in different experiments, but not the least effect was produced.

1718. I now removed the helices with their soft iron cores, and replaced them by two *flat helices* wound upon card board, each containing forty-two feet of silked copper wire, and having no associated iron. Otherwise the arrangement was as before, and exceedingly sensible; for a very slight motion of the magnet between the helices produced an abundant vibration of the galvanometer needle.

1719. The introduction of plates of shell-lac, sulphur, or copper into the intervals between the magnet and these helices (1713.), produced not the least effect, whether the former were quiescent or in rapid revolution (1715.). So here no evidence of the influence of the intermediate particles could be obtained (1710.).

1720. The magnet was then removed and replaced by a flat helix, corresponding to the two former, the three being parallel to each other. The middle helix was so arranged that a voltaic current could be sent through it at pleasure. The former galvanometer was removed, and one with a double coil employed, one of the lateral helices being connected with one coil, and the other helix with the other coil, in such manner that when a voltaic current was sent through the middle helix its inductive action (26.) on the lateral helices should cause currents in them, having contrary directions in the coils of the galvanometer. By a little adjustment of the distances these induced currents were rendered exactly equal, and the galvanometer needle remained stationary notwithstanding their frequent production in the instrument. I will call the middle coil C, and the external coils A and B.

1721. A plate of copper 0·7 of an inch thick and six inches square, was placed between coils C and B, their respective distances remaining unchanged; and then a voltaic current from twenty pairs of 4-inch plates was sent through the coil C, and intermitted, in periods fitted to produce an effect on the galvanometer (1712.), if any difference had been produced in the effect of C on A and B. But notwithstanding the presence of air in one interval and copper in the other, the inductive effect was exactly alike on the two coils, and as if air had occupied both intervals. So that notwithstanding the facility with which any induced currents might form in the thick copper plate, the coil outside of it was just as much affected by the central helix C as if no such conductor as the copper had been there (65.).

1722. Then, for the copper plate was substituted one of sulphur 0·9 of an inch thick; still the results were exactly the same, i. e. there was no action at the galvanometer.

1723. Thus it appears that when a voltaic current in one wire is exerting its inductive action to produce a contrary or a similar current in a neighbouring wire, according as the primary current is commencing or ceasing, it makes not the least difference whether the intervening space is occupied by such insulating bodies as air, sulphur and shell-lac, or such conducting bodies as copper, and the other non-magnetic metals.

1724. A correspondent effect was obtained with the like forces when resident in a magnet thus. A single flat helix (1718.) was connected with a galvanometer, and a magnetic pole placed near to it; then by moving the magnet to and from the helix, or the helix to and from the magnet, currents were produced indicated by the galvanometer.

1725. The thick copper plate (1721.) was afterwards interposed between the magnetic pole and the helix; nevertheless on moving these to and fro, effects, exactly the same in direction and amount, were obtained as if the copper had not been there. So also on introducing a plate of sulphur into the interval, not the least influence on the currents produced by motion of the magnet or coils could be obtained.

1726. These results, with many others which I have not thought it needful to describe, would lead to the conclusion that (judging by the *amount* of effect produced at a distance by forces transverse to the electric current, i. e. magnetic forces,) the intervening matter, and therefore the intervening particles, have nothing to do with the phenomena; or in other words, that though the inductive force of static electricity is transmitted to a distance by the action of the intermediate particles (1164. 1666.), the transverse inductive force of currents, which can also act at a distance, is not transmitted by the intermediate particles in a similar way.

1727. It is however very evident that such a conclusion cannot be considered as proved. Thus when the metal copper is between the pole and the helix (1715. 1719. 1725.) or between the two helices (1721.) we know that its particles are affected, and can by proper arrangements make their peculiar state for the time very evident by the production of either electrical or magnetical effects. It seems impossible to consider this effect on the particles of the intervening matter as independent of that produced by the inductric coil or magnet C, on the inducteous coil or core A (1715. 1721.); for since the inducteous body is equally affected by the inductric body whether these intervening and affected particles of copper are present or not (1723. 1725.), such a supposition would imply that the particles so affected had no reaction back on the original inductric forces. The more reasonable conclusion, as it appears to me, is, to consider these affected particles as efficient in continuing the action onwards from the inductric to the inducteous body, and by this very communication producing the effect of *no loss* of induced power at the latter.

1728. But then it may be asked what is the relation of the particles of insulating bodies, such as air, sulphur, or lac, when *they* intervene in the line of magnetic action? The answer to this is at present merely conjectural. I have long thought there must be a particular condition of such bodies corresponding to the state which causes currents in metals and other conductors (26. 53. 191. 201. 213.); and considering that the bodies are insulators one would expect that state to be one of tension. I have by rotating non-conducting bodies near magnetic poles and poles near them, and also by causing powerful electric currents to be suddenly formed and to cease around and about insulators in various directions, endeavoured to make some such state sensible, but have not succeeded. Nevertheless, as any such state must be of exceedingly low intensity, because of the feeble intensity of the currents which are used to induce it, it may well be that the state may exist, and may be discoverable by some more expert experimentalist, though I have not been able to make it sensible.

1729. It appears to me possible, therefore, and even probable that magnetic action may be communicated to a distance by the action of the intervening particles, in a manner having a relation to the way in which the inductive forces of static electricity are transferred to a distance (1677.); the intervening particles assuming for the time more or less of a peculiar condition, which (though with a very imperfect idea) I have several times expressed by the term *electro-tonic state* (60. 242. 1114. 1661.) I hope it will not be understood that I hold the settled opinion that such is the case. I would rather in fact have proved the contrary, namely, that magnetic forces are quite independent of the matter intervening between the inductric and the inducteous bodies; but I cannot get over the difficulty presented by such substances as copper, silver, lead, gold, carbon, and even aqueous solutions (201. 213.), which though they are known to assume a peculiar state whilst intervening between the bodies acting and acted upon (1727.), no more interfere with the final result than those which have as yet had no peculiarity of condition discovered in them.

1730. A remark important to the whole of this investigation ought to be made here. Although I think the galvanometer used as I have described it (1711.- 1720.) is quite sufficient to prove that the final amount of action on each of the two coils or the two cores A and B (1713. 1719.) is equal, yet there is an effect which *may* be consequent on the difference of action of two interposed bodies which it would not show. As time enters as an element into these actions* (125.), it is very possible that the induced actions on the helices or cores A, B, though they rise to the same degree when air and copper, or air and lac are contrasted as intervening substances, do not do so in the same time; and yet, because of the length of time occupied by a vibration of the needle, this difference may not be visible, both effects rising to their maximum in periods so short as to make no sensible portion of that required for a vibration of the needle, and so exert no visible influence upon it.

1731. If the lateral or transverse force of electrical currents, or what appears to be the same thing, magnetic power, could be proved to be influential at a distance independently of the intervening contiguous particles, then, as it appears to me, a real distinction, of a high and important kind, would be established between the natures of these two forces (1654. 1664.). I do not mean that the powers are independent of each other and might be rendered separately active, on the contrary they are probably essentially associated (1654.), but it by no means follows that they are of the same nature. In common statical induction, in conduction, and in electrolyzation, the forces at the opposite extremities of the particles which coincide with the lines of action, and have commonly been distinguished by the term electric, are polar, and in the cases of contiguous particles act only to insensible distances; whilst those which are transverse to the direction of these lines, and are called magnetic, are circumferential, act at a distance, and if not through the mediation of the intervening particles, have their relations to ordinary matter entirely unlike those of the electrical forces with which they are associated.

---

* See Annales de Chimie, 1833, tom. li. pp. 422, 428.

1732. To decide this question of the identity or distinction of the two kinds of power, and establish their true relation, would be exceedingly important. The question seems fully within the reach of experiment, and offers a high reward to him who will attempt its settlement.

1733. I have already expressed a hope of finding an effect or condition which shall be to statical electricity what magnetic force is to current electricity (1658.) If I could have proved to my own satisfaction that magnetic forces extended their influence to a distance by the conjoined action of the intervening particles in a manner analogous to that of electrical forces, then I should have thought that the lateral tension of the lines of inductive action (1659.), or that state so often hinted at as the electro-tonic state (1661. 1662.), was this related condition of statical electricity.

1734. It may be said that the state of *no lateral action* is to static or inductive force the equivalent of *magnetism* to current force; but that can only be upon the view that electric and magnetic action are in their nature essentially different (1664.). If they are the same power, the whole difference in the results being the consequence of the difference of *direction*, then the normal or *undeveloped* state of electric force will correspond with the state of *no lateral action* of the magnetic state of the force; the electric current will correspond with the lateral effects commonly called magnetism: but the state of static induction which is between the normal condition and the current will still require a corresponding lateral condition in the magnetic series, presenting its own peculiar phenomena; for it can hardly be supposed that the normal electric, and the inductive or polarized electric, condition, can both have the same lateral relation. If magnetism be a separate and a higher relation of the powers developed, then perhaps the argument which presses for this third condition of that force would not be so strong.

1735. I cannot conclude these general remarks upon the relation of the electric and magnetic forces without expressing my surprise at the results obtained with the copper plate (1721. 1725.) The experiments with the flat helices represent one of the simplest cases of the induction of electrical currents (1720.); the effect, as is well known, consisting in the production of a momentary current in a wire at the instant when a current in the contrary direction begins to pass through a neighbouring parallel wire, and the production of an equally brief current in the reverse direction when the determining current is stopped (26.). Such being the case, it seems very extraordinary that this induced current which takes place in the helix A when there is only air between A and C (1720.) should be equally strong when that air is replaced by an enormous mass of that excellently conducting metal copper (1721.). It might have been supposed that this mass would have allowed of the formation and discharge of almost any quantity of currents in it, which the helix C was competent to induce, and so in some degree have diminished if not altogether prevented the effect in A: instead of which, though we can hardly doubt that an infinity of currents are formed at the moment in the copper plate, still not the smallest diminution or alteration of the effect in A appears (65.). Almost the only way of reconciling this effect with generally received notions is, as it appears to me, to admit that magnetic action is communicated by the action of the intervening particles (1729. 1733.).

1736. This condition of things, which is very remarkable, accords perfectly with the effects observed in solid helices where wires are coiled over wires to the amount of five or six or more layers in succession, no diminution of effect on the outer ones being occasioned by those within.

§ 22. *Note on electrical excitation.*

1737. That the different modes in which electrical excitement takes place will some day or other be reduced under one common law can hardly be doubted, though for the present we are bound to admit distinctions. It will be a great point gained when these distinctions are, not removed, but understood.

1738. The strict relation of the electrical and chemical powers renders the chemical mode of excitement the most instructive of all, and the case of two isolated combining particles is probably the simplest that we possess. Here however the action is local, and we still want such a test of electricity as shall apply to it, to cases of current electricity, and also to those of static induction. Whenever by virtue of the previously combined condition of some of the acting particles (923.) we are enabled, as in the voltaic pile, to expand or convert the local action into a current, then chemical action can be traced through its variations to the production of *all* the phenomena of tension and the static state, these being in every respect the same as if the electric forces producing them had been developed by friction.

1739. It was Berzelius, I believe, who first spoke of the aptness of certain particles to assume opposite states when in presence of each other (959.). Hypothetically we may suppose these states to increase in intensity by increased approximation, or by heat, &c. until at a certain point combination occurs, accompanied by such an arrangement of the forces of the two particles between themselves as is equivalent to a discharge; producing at the same time a particle which is throughout a conductor (1700.)

1740. This aptness to assume an excited electrical state (which is probably polar in those forming non-conducting matter) appears to be a primary fact, and to partake of the nature of induction (1162.), for the particles do not seem capable of retaining their particular state independently of each other (1177.) or of matter in the opposite state. What appears to be definite about the particles of matter is their assumption of a *particular* state, as the positive or negative, in relation to each other, and not of either one or other indifferently; and also the acquirement of force up to a certain amount.

1741. It is easily conceivable that the same force which causes local action between two free particles shall produce current force if one of the particles is previously in combination, forming part of an electrolyte (923. 1738.). Thus a particle of zinc, and one of oxygen, when in presence of each other, exert their inductive forces (1740.), and these at last rise up to the point of combination. If the oxygen be previously in union with hydrogen, it is held so combined by an analogous exertion and arrangement of the forces; and as the forces of the oxygen and hydrogen are for the time of combination mutually engaged and related, so when the superior relation of the forces between the oxygen and zinc come into play, the induction of the former or oxygen towards the metal cannot be brought on and increased without a corresponding deficiency in its induction towards the hydrogen with which it is in combination (for the amount of force in a particle is considered as definite), and the latter therefore has its force turned towards the oxygen of the next particle of water; thus the effect may be considered as extended to sensible distances, and thrown into the condition of static induction, which being discharged and then removed by the action of other particles produces currents.

1742. In the common voltaic battery, the current is occasioned by the tendency of the zinc to take the oxygen of the water from the hydrogen, the effective action being at the place where the oxygen leaves the previously existing electrolyte. But Schœnbein has arranged a battery in which the effective action is at the other extremity of this essential part of the arrangement, namely, where oxygen goes to the electrolyte.* The first may be considered as a case where the current is put into motion by the abstraction of oxygen from hydrogen, the latter by that of hydrogen from oxygen. The direction of the electric cuarent is in both cases the same. when referred to the direction in which the elementary particles of the electrolyte are moving (923. 962.), and both are equally n accordance with the hypothetical view of the inductive action of the particles just described (1740.)

---

* Philosophical Magazine, 1838, xii. 225, 315. See also De la Rive's results with peroxide of manganese. Annales de Chimie, 1836, lxi. p. 40.—*Dec.* 1838.

1743. In such a view of voltaic excitement, the action of the particles may be divided into two parts, that which occurs whilst the force in a particle of oxygen is rising towards a particle of zinc acting on it, and falling towards the particle of hydrogen with which it is associated (this being the progressive period of the inductive action), and that which occurs when the change of association takes place, and the particle of oxygen leaves the hydrogen and combines with the zinc. The former appears to be that which produces the current, or if there be no current, produces the state of tension at the termination of the battery; whilst the latter, by terminating for the time the influence of the particles which have been active, allows of others coming into play, and so the effect of current is continued.

1744. It seems highly probable, that excitement by friction may very frequently be of the same character. Wollaston endeavoured to refer such excitement to chemical action;* but if by chemical action ultimate union of the acting particles is intended, then there are plenty of cases which are opposed to such a view. Davy mentions some such, and for my own part I feel no difficulty in admitting other means of electrical excitement than chemical action, especially if by chemical action is meant a final combination of the particles.

1745. Davy refers experimentally to the opposite states which two particles having opposite chemical relations can assume when they are brought into the close vicinity of each other, but *not* allowed to combine†. This, I think, is the first part of the action already described (1743.); but in my opinion it cannot give rise to a continuous current unless combination takes place, so as to allow other particles to act successively in the same manner, and not even then unless one set of the particles be present as an element of an electrolyte (923. 963.); i. e. mere quiescent contact alone without chemical action does not in such cases produced a *current*.

1746. Still it seems very possible that such a relation may produce a high charge, and thus give rise to excitement by friction. When two bodies are rubbed together to produce electricity in the usual way, one at least must be an insulator. During the act of rubbing, the particles of opposite kinds must be brought more or less closely together, the few which are most favourably circumstanced being in such close contact as to be short only of that which is consequent upon chemical combination. At such moments they may acquire by their mutual induction (1740.) and partial discharge to each other, very exalted opposite states, and when, the moment after, they are by the progress of the rub removed from each other's vicinity, they will retain this state if both bodies be insulators, and exhibit them upon their complete separation.

---

* Philosoohical Transactions, 1801, p. 427. † Ibid. 1807, p. 34.

1747. All the circumstances attending friction seems to me to favour such a view. The irregularities of form and pressure will cause that the particles of the two rubbing surfaces will be at very variable distances, only a few at once being in that very close relation which is probably necessary for the development of the forces; further, those which are nearest at one time will be further removed at another, and others will become the nearest, and so by continuing the friction many will in succession be excited. Finally, the lateral direction of the separation in rubbing seems to me the best fitted to bring many pairs of particles, first of all into that close vicinity necessary for their assuming the opposite states by relation to each other, and then to remove them from each other's influence whilst they retain that state.

1748. It would be easy, on the same view, to explain hypothetically, how, if one of the rubbing bodies be a conductor, as the amalgam of an electrical machine, the state of the other when it comes from under the friction is (as a mass) exalted; but it would be folly to go far into such speculation before that already advanced has been confirmed or corrected by fit experimental evidence. I do not wish it to be supposed that I think all excitement by friction is of this kind; on the contrary, certain experiments lead me to believe, that in many cases, and perhaps in all, effects of a thermo electric nature conduce to the ultimate effect; and there are very probably other causes of electric disturbance influential at the same time, which we have not as yet distinguished.

Royal Institution,
June, 1838.

In a paper which was read at the Glasgow meeting of the "British Association for the Promotion of Science" I had occasion to trace the experiments of M. Schoenbein and others, on the inactivity of certain metals on acids, to others of a similar nature performed by Mr. Keir some fifty years ago. Since the reading of my paper at Glasgow, I have been requested to insert Mr. Keir's experiments in an early number of the "Annals," which I now do with great pleasure, as I think that many readers will be much interested by becoming acquainted with those original experiments of Keir, which, within the last few years, have commanded so much attention, as novelties emanating from the labours of other experimentors. W. S.

LIII.—*Experiments and Observations on the Dissolution of Metals in Acids, and their Precipitations: with an Account of a New Compound Acid Menstrum useful in some mechanical operations of parting metals.* By JAMES KEIR, Esq., F.R.S. (Abridgement of the Philosophical Transaction of the Royal Society of London for the year 1790.)

In the following paper, says Mr. Keir, I intend to relate two sets of experiments: one, showing the effects of compounding the vitriolic and nitrous acids in dissolving metals: and the other, describing some curious appearances which occur in the precipitation of silver from its solution in nitrous acid by iron, and by some othe[illegible]stances. In a subsequent paper I hope to continue the subj[illegible]f metallic dissolution* and precipitation, first, by adding some experiments on the quantities and kinds of gas produced by dissolving different metals in different acids, under various circumstances: 2ndly, by submitting certain general propositions, which seem deducible from the facts related; and lastly, by concluding with some reflections relative to the theory of metallic dissolution and precipitation.

PART 1.—*On the effects of Compounding the Vitriolic and Nitrous Acids, under various circumstances, on the dissolution of metals.*

§ 1.—*On the Mixture of Oil of Vitriol and Nitre.*—1. The properties of the several acids, in their separate states, have been investigated with considerable industry and success: and those of one compound, aqua regis, are well known, on account of its frequent use in dissolving gold: yet not only various other combinations of different acids remain to be examined; but also the changes of properties to which these mixed acids are subject, from the difference of circumstances, especially those of concentration, temperature, and of that quality which is called, properly or improperly, phlogistication, are subjects still open for inquiry.

2. As I shall have frequent occasion to speak of phlogistication and dephlogistication of acids, I wish to premise, that by these terms I mean only certain states or qualities of those bodies, but without any theoretic inference. Thus vitriolic acid may be said

---

* The English word solution has two significations in chemistry; one expressive of the act of dissolving, as when we say, that, "solution is a chemical operation;" and the other, denoting the substance dissolved in its solvent, as, "a solution of silver in nitrous acid." The French language is equally equivocal, as the word "dissolution" [illegible] used in both the above-mentioned senses. In treating on this subject, in which both meanings were very frequently required, sometimes in the same sentence, I could not but be sensible of confusion in the style, and I have therefore confined the word solution to express the substance dissolved together with its solvent, and the word dissolution to denote the act of dissolving.

to be phlogisticated by addition of sulphur or other inflammable matter, by which it is converted into sulphurous acid, without determining whether this change be caused by the addition of the supposed principle phlogiston, as one set of philosophers believe, or by the action of the added inflammable substance in drawing from the acid a portion of its aërial principle, by which the sulphur, its other element, is made to predominate, as others have lately maintained. It were much to be wished that we had words totally unconnected with theory; that chemists, who differ from each other in some speculative points, may yet speak the same language, and may relate their facts and observations, without having our attention continually drawn aside from these, to the different modes of explanation which have been imagined. But at present we have only the choice of terms between words derived from the ancient theory, and those which have been lately proposed by the opposers of that theory. In this dilemma I have preferred the use of the former, not that I wish to show any predeliction to either theory, but because that system, having long been generally adopted, is understood by all parties: and principally because, by using the words of the old theory, I am at liberty to define them, and to give significations expressive merely of parts, and of the actual state of bodies; whereas the language and theory of the antiphlogistic chemists being interwoven and adapted to each other, the former cannot be divested of its theoretical reference, and therefore seems inapplicable to the mere exposition of facts, but ought to be reserved solely for the explanations of the doctrines from which this language is derived. Thus, by the definition before mentioned of phogistication, this word expresses not the presence or existence of an hypothetical principle of inflammability, but a certain well known quality of acids and of other bodies, communicated to them by the addition of many actually inflammable substances. Thus, nitrous acid acquires a phlogisticated quality by addition of a little spirit of wine, or by distillation with any inflammable substance.

3. No two substances are more frequently in the hands of chemists and artists than vitriolic acid and nitre, yet I have found, that a mere mixture of these when much concentrated, possess properties which neither the vitriolic acid nor the nitrous, of the same degree of concentration, have, singly, and which could not be easily deduced, *à priori,* by reasoning from our present knowledge of the theory of chemistry.

4. Having found by some previous trials that a mixture composed of nitre dissolved in oil of vitriol was capable of dissolving silver easily and copiously, while it did not affect copper, iron, lead, regulus of cobalt, gold, platina, I conceived, that it might be useful in some cases of the parting of silver from copper and the other metals above mentioned; and having also observed, that the dissolving powers of the mixture of vitriolic and nitrous acids varied greatly in different degrees of concentration, and phlogistication, I thought that an in-

vestigation of these effects might be a subject fit for philosophical chemistry, and might tend to illustrate the theory of the dissolution of metals in acids. With these views I made the following experiments:—

5. I put into a long necked retort, the contents of which, including the neck, were 1400 grain measures, 100 grain measures of oil of vitriol of the usual density at which it is prepared in England, that is, whose specific gravity is to that of water as 1·844 to 1, and 100 grains of pure and clear nitre, which was then dissolved in the acid by the heat of a water-bath. To this mixture 100 grains of standard silver were added; the retort was set in a water-bath, in which the water was made to boil, and a pneumatic apparatus was applied to catch any air or gas which might be extricated.—The silver began to dissolve, and the solution became of a purple or violet colour, no air was thrown into the inverted jar, excepting a little of the common air of the retort, by means of the expansion which it suffered from the heat of the water-bath, and from some nitrous fumes which appeared in the retort, and which having afterwards condensed, occasioned the water to rise along the neck of the retort, and mix with the solution; the remaining silver was then separated and weighed, and it was found that 39 grains had been dissolved: but probably more would have been dissolved if the operation had not been interrupted by the water rushing into the retort.

6. In the same apparatus 200 grains of standard silver were added to a mixture of 100 grains of nitre, previously dissolved in 200 grain measures of oil of vitriol; and in this solvent, 92 grains of the silver were dissolved, without any production of air or gas. The solution, which was of a violet colour, having been poured out of the retort whilst warm (for with so a large a portion of nitre, such mixtures, especially after having dissolved silver, are apt to congeal with small degrees of cold), in order to separate the undissolved silver from it, and having been returned into the retort without this silver, I poured 200 grains of water into the retort, on which a strong effervescence took place between the solution and the water, and 3100 grain measures of nitrous gas were thrown into the inverted jar. On pouring 200 grains more of water into the retort, 600 grain measures of the same gas were expelled. Further additions of water yielded no more gas; neither did the silver, when afterwards added to this solution, give any sensible effervescence, or suffer a greater loss of weight than two grains.

7. In the same apparatus 100 grains of standard silver were exposed to a mixture of 30 grains of nitre dissolved in 200 grain measures of oil of vitriol; and in this operation 80 grains of silver were dissolved, while at the same time, 4500 grain measures of nitrous gas were thrown into the inverted jar. When the undissolved silver was removed, 200 grains of water were added to the solution, which was of a violet colour, and on the mixture of the

two fluids an effervescence happened; but only a few bubbles of nitrous gas were then expelled.

8. In the same apparatus 100 grains of standard silver were exposed to a mixture of 200 grain measures of oil of vitriol, 200 grains of nitre, and 200 grains of water: and in this operation 20 grains of the silver were dissolved without any sensible emission of air or gas.

9. In these experiments, the copper contained in the standard silver gave a reddish colour to the saline mass which was formed in the solution, and seemed to be a calx of copper interspersed through the salt of silver. I perceived no other difference between the effects of pure and standard silver dissolved in this acid.

10. I then exposed tin to the same mixture of oil of vitriol and nitre, in the same apparatus, and in the same circumstances, taking care always to add more metal than could be dissolved, that, by weighing the remainder, the quantity capable of being dissolved might be found, as I had done with the experiments on silver; and the results were as follow:—

11. No tin was dissolved nor calcined by the mixtures in the proportion of 200 grain measures of oil of vitriol to 200 grains of nitre: nor by any other mixture in the proportion of 200 grain measures of oil of vitriol to 150 grains of nitre, and consequently no gas was produced in either instance.

12. With a mixture in the proportion of 200 grain measures of oil of vitriol and 100 grains of nitre, the tin began soon to be acted on, and to be diffused through the liquor: but no extrication of gas appeared till the digestion had been continued two hours in boiling water; and then it took place, and gave a frothy appearance to the mixture, which was of an opaque white colour, from the powder of tin being diffused among it. In this experiment, the quantity of tin thus calcined was 73 grains, and the quantity of nitrous gas extricated during this action on the tin, was 8500 grain measures. Then, on pouring 200 grains of water into the retort, a fresh effervescence took place between the water and the white opaque white mass, and 4600 grain measures of nitrous gas were thrown into the inverted receiver.

13. With a mixture in the proportion of 100 grain measures of oil of vitriol to 30 grains of nitre, 30 grains of tin were dissolved or calcined, and the nitrous gas, which began to be extricated much sooner than in the last mentioned experiment with a larger proportion of nitre, amounted to 6300 grain measures. Water, added to this solution of tin, did not produce any effervescence.

14. With a mixture in the proportion of 200 grain measures of oil of vitriol, 200 grains of nitre, and 200 grains of water, 133 grains of tin were acted on with an effervescence, which took place violently, and produced 6500 grain measures of nitrous gas.

15. The several mixtures above mentioned, in different proportions of nitre and oil of vitriol, did, by the help of the heat of the water-bath, calcined mercury into a white or grayish powder. Nickle was also partly calcined and partly dissolved by these mixtures. I did not perceive that any other metal was affected by them, excepting that the surfaces of some of them were tarnished.

16. These mixtures of oil of vitriol and nitre were apt to congeal by cold, those especially which had a large proportion of nitre thus, a mixture of 100 grain measures of oil of vitriol and 480 grains of nitre, after having kept fluid for several days, in a phial not so accurately stopped as to prevent altogether the escape of some white fumes, congealed at the temperature of 55° of Fakrenheit's thermometer: whereas some of the same liquid, having been mixed with equal parts of oil of vitriol, did not congeal with a less cold than 45°. The congelation is promoted by exposure to air, by which white fumes rise, and moisture may be absorbed, or by any other mode of slight dilution with water.

17. Dilution of this compound acid, with more or less water, alters considerably its properties, with regard to its action on metals. Thus it has been observed, that in its concentrated state it does not act on iron: but by adding water, it acquires a power of acting on that metal, and with different effect according to the proportion of the water added. Thus, by adding to two measures of the compound acid one measure of water, the liquor is rendered capable of calcining iron, and forming with it a white powder, but without effervescence. With an equal measure of water effervescence is produced. With a larger proportion of water the iron gave also a brown colour to the liquor, such as phlogisticated nitrous acid acquires from iron, or communicates to a solution of martial vitriol in water.

18. Dilution with water renders this compound acid capable of dissolving copper and zinc, and probably those other metals which are subject to the action of the dilute vitriolic or nitrous acid.

§ 2. *An account of a new process for separating silver from copper.*—19. The properties of this liquor, in dissolving silver easily, without acting on copper, have rendered it capable of a very useful application in the arts. Among the manufacturers at Birmingham, that of making vessels of silver plated on copper is a very considerable one. In cutting out the rolled plated metal into pieces of the required formes and sizes, there are many shreds, or scraps as they are called, unfit for any purpose but the recovery of the metals, by separating them from each other. The easiest and most economical method of parting these two metals, so as not to lose either of them, is an object of some consequence to the manufacturers. For this purpose two modes were practised; 1st, by melting the whole of the mixed metals with lead, and separating them by eliquation and testing; & 2nd, by dissolving both metals in oil of vitriol, with the help of heat, and by separating the vitriol of copper, by dissolving it in water, from the vitriol of silver, which is afterwards to be reduced and purified. In the first of these methods, there is a considerable waste of lead and copper; and in the second, the quantity of vitriolic acid employed is very great, as much more is dissipated in the form of volatile vitriolic, or sulphureous acid, than remains in the composition of the two vitriols.

Some years ago I communicated to an artist the method of affecting the separation of silver from copper by means of the above mentioned compound of vitriolic acid and nitre; and, as I am informed, that it is now commonly practised by the manufacturers in Birmingham, I have no doubt but it is much more economical, and it is certainly much more easily executed, than any of the other methods; for nothing more is required than to put the pieces of plated metal into an earthen-glazed pan, to pour on them some of the acid liquor, which may be in the proportion of 8 or 10 lb. of oil of vitriol to 1 lb. of nitre: to stir them about, that the surfaces may frequently be exposed to fresh liquor, and to assist the action by a gentle heat from 100° to 200° of Faharenheit's scale. When the liquor is nearly saturated, the silver is to be precipitated from it by common salt, which forms a luna cornea, easily reducible by melting it in a crucible with a sufficient quantity of potash; and lastly, by refining the melted silver, if necessary, with a little nitre thrown on it. In this manner the silver will be obtained sufficiently pure, and the copper will remain unchanged. Otherwise, the silver may be precipitated in its metalic state, by adding to the solution of silver a few pieces of copper, and a sufficient quantity of water to enable the liquor to act on the copper. The property which this acid mixture possesses of dissolving silver with great facility, and in considerable quantity, will probably render it a useful menstruum in the separation of silver from other metals; and as the alchemists have distinguished the peculiar solvent of gold under the title of aqua regis, a name sufficiently distinctive, though founded on a fanciful allusion; so, if they had been acquainted with the properties of this compound, they would probably have bestowed upon it the appellation of aqua reginæ.

§ 3.—*The change of properties communicated to the mixture of vitriolic and nitrous acids by phlogistigation* —20. The above described compound acid may be phlogistigated by different methods, of which I shall mention three. First, By digesting the compound acid with sulphur by means of the heat of a water bath, the liquor dissolves the sulphur with effervescence, loses its property of yielding white fumes; and if the quantity of sulphur be sufficient, and if the heat applied be long enough continued, it exhibits red nitrous vapours, and assumes a violet colour.

Secondly—If, instead of dissolving nitre in concentrated vitriolic acid, this acid be impregnated with nitrous gas, or with nitrous vapour by making this gas, or vapour, pass into the acid, this compound will be phlogisticated, as it contains but only its phlogisticated part, not the entire nitrous acid, or element, the nitrous gas, without the proportion of pure air is necessary to constitute an acid. This impregnation of oil of vitriol with nitrous gas, or nitrous vapour, was first described, and some of the properties of the impregnated liquor noticed, by Dr. Priestly. (See Exp. and Obs. on Air, vol. 3, p. 129 and 217.) Thirdly, By substituting nitrous ammoniac instead of nitre in the mixture with oil of vitriol.

21. The compound prepared by any of these methods, but especially by the first and second, differs considerably in its properties with regard to its action on metals from the acid described in the first section. It has been observed, that the latter compound has little action on any metals but silver, tin, mercury, and nickel. On the other hand, the phlogisted compound not only acts on these, but also on several others It forms with iron a beautiful rose-coloured solution, without application of any artificial heat: and in time a rose-coloured saline precipitate is deposited, which is soluble in water with considerrble effervescence. It dissolves copper, and acquires from this metal, and also from regulus of cobalt, zinc, and lead, pretty deep violet tinges. Bismuth and regulus of antimony were also attacked by this phlogisticated acid. To ascertain more exactly the effects of this phlogisticated acid on some metals, I made the following experiments, with a liquor prepared by making nitrous gas pass through oil of vitriol during a considerable time.

22. To 200 grain measures of the oil of vitriol impregnated with nitrous gas, put into a retort with a long neck, the capacity of which, including the neck, was 1150 grain-measures, I added 144 grains of standard silver, and immersed the mouth of the retort in water, under an inverted jar filled with water, to catch the gas which might be extricated. The acid began to dissolve the silver without the application of heat; the solution became of a violet colour, and the quantity of nitrous gas received in the inverted jar was 14,700 grain measures. On weighing the silver remaining, the quantity which had been dissolved was found to be 70 grains. When water was added to the solution, an effervescence appeared, but only a very small quantity of gas was extricated. By means of water, a white saline powder of silver, soluble in a larger quantity of water, was precipitated from the solution. The solution of silver, when saturated and undiluted, congeals readily in cool temperature, and when diluted to a certain degree with water, gives foliated crystals.

23. In the same apparatus, and in the same manner, 100 grai

measures of this impregnated oil of vitriol were applied to iron. An effervescence appeared without the application of heat, the surface of the iron acquired a beautiful rose-colour or redness mixed with purple; and this colour gradually pervaded the whole liquor, but disappeared on keeping the retort some time in hot water. Notwithstanding a considerable apparent effervescence, the quantity of air expelled into the inverted jar was only 400 grain measures, of which one-fourth was nitrous, and the rest phlogisticated. The solution was then poured out of the retort, and the iron was found to have lost 2 grains in weight. The solution was returned into the retort without the iron, and 200 grains of water were added to it; on which a white powder was immediately precipitated, which re-dissolved with great effervescence. When 2000 grain measures of nitrous gas had been expelled into the inverted jar, without application of heat, the retort was placed in the water-bath, the heat of which rendered the effervescence so strong, that the liquor boiled over the neck of the retort, so that the quantity of gas extricated could not be ascertained.

24. In the same manner 11 grains of copper were dissolved in 100 grain measures of impregnated oil of vitriol. The solution was of a deep violet-colour, and at last was turbid. The quantity of nitrous gas expelled into the inverted jar during the operation was 4700 grain measures. When the copper was removed, and 200 grains of water were added to the solution, an effervescence took place, 1700 grain measures of nitrous gas were expelled, and the solution then acquired a blue-colour.

25. In the same apparatus and manner, 100 grain measures of the impregnated oil of vitriol were applied to tin, which was thence diminished in weight 16 grains, while the liquor acquired a violet-colour, became turbid by the suspension of the calx of tin, and a quantity of nitrous gas was thrown into the inverted receiver equal to 4100 grain measures, without application of heat, and another quantity equal to 4900 grain measures, after the retort was put into a water-bath.

26. Mercury added to the impregnated oil of vitriol formed a thick white turbid liquor, which was rendered clear by addition of unimpregnated oil of vitriol. In a little time this mixture continuing to act on the remaining mercury acquired a purple-colour. The mercury acted on, sunk to the bottom of the glass in the form of a white powder, and the purple liquor, when mixed with a solution of common salt in water, gave no appearance of it containing any mercury in a dissolved state.

27. The nitrous gas with which the oil of vitriol is impregnated shows no disposition to quit the acid by exposure to air; but, on adding water to the impregnated acid, the gas is expelled suddenly with great effervescence, and with red fumes, in consequence of its mixture with the atmospherical air. In adding 210 grains of water,

to 60 grain measures of impregnated oil of vitriol, 2300 grains of nitrous gas were thrown into the receiver; but as the action of the two liquors is instantaneous, the quantity of gas expelled from the retort before its neck could be immersed in water, and placed under the receiver, must have been considerable. The whole of the gas, however, was not extricated by means of the water, for the remaining liquor dissolved 5 grains of copper, while 800 measures of nitrous gas were thrown into the retort, (probably the receiver.)

28. The following facts principally are established by the preceding experiments. 1. That a mixture of the vitriolic and nitrous acids in a concentrated state, has a peculiar faculty of dissolving silver copiously. 2. That it acts on, and principally calcines, tin, mercury, and nickel: the latter of which, however, it dissolves in small quantity: and that it has little or no action on other metals. 3. That the quantity of gas produced while the metal is dissolving is greater, relatively, to the quantity of metal dissolved, when the proportion of nitre to the vitriolic acid is small, than when large: and that when the metals are dissolved by mixtures, containing much nitre, and with a small production of gas, the solution itself, or the metallic salt formed in it, yields abundance of gas when mixed with water. 4. That dilution with water renders the concentrated mixture less capable of dissolving silver, but more capable of acting on other metals. 5. That this mixture of highly concentrated vitriolic and nitric acids, acquires a purple or violet colour when phlogisticated, either by addition of inflammable substances, as sulphur, or by its actions on metals, or by very strong impregnation of vitriolic acid with nitrous gas.[*] 6. That this phlogistication was found to communicate to the mixture the power of dissolving, though in small quantities, copper, iron, zinc, and the regulus of cobalt. 7. That water expels from a highly phlogisticated mixture of concentrated vitriolic and nitrous acids, or of oil of vitriol impregnated with nitrous gas, a great part of its contained gas; and that therefore this gas is not capable of being retained in such quantity by dilute as by concentrated acids. Water unites with the mixture of vitriol and nitre, without any considerable effervescence.

29. To these observations I shall subjoin one other fact, namely, that when, to the mixture of oil of vitriol and nitre, a saturated solution of common salt in water is added, a powerful aqua regis is produced, capable of dissolving gold and platina; and this aqua regis, though composed of liquors perfectly colourless and free from all metallic matter, acquires at once a bright and deep yellow colour. The addition of dry common salt to the concentrated mixtures of vitriolic and nitrous acids produces an effervescence but not the yellow colour; for the production of which therefore a certain proportion of water seems to be necessary.

---

[*] Dr. Priestley has noticed this colour communicated to oil of vitriol by impregnation with nitrous gas or vapour, and also the effervescence produced by adding water to this impregnated liquor.

PART 2.—*On the precipitation of Silver from Nitrous Acid by Iron.*

§ 1. Bergman relates, that on adding iron to a solution of silver in nitrous acid, no precipitation ensued; though the affinity of iron to acids generally is known to be much stronger than that of silver; and though, even with regard to the nitrous acid, other experiments evince the superior affinity of iron; for as iron precipitates copper from this acid, and as copper precipitates silver, we must infer the greater affinity of iron than silver. In the course of his experiments, however, some instances of precipitation occurred, which he attributed to the peculiar quality of the irons which he employed.* I was desirous of discovering the circumstances, and of investigating the cause, of this irregularity and exception to the generally received laws of affinity.

2. I digested a piece of fine silver in pure and pale nitrous acid, and while the dissolution was going on, and before the saturation was completed, I poured a portion of the solution on a piece of clean and newly-scraped iron wire into a wine glass, and observed a sudden and copious precipitation of silver. The precipitate was at first black, then it assumed the appearance of silver, and was five or six times larger in diameter than the piece of iron wire which it enveloped. The action of the acid on the iron continued some little time, and then it ceased; the silver redissolved, and the liquor became clear, and the iron remained bright and undisturbed

---

* Bergman tried many different kinds of iron, and he thought he found two that were capable of precipitating silver. But as he did not discover the circumstances according to which the precipitation sometimes does, and at other times does not happen, he may have been mistaken with regard to the peculiar quality of these two kinds of iron. At least the several kinds which I have tried always precipitated silver in certain circumstances, and always failed to precipitate in certain other circumstances. I do not know any other author who has mentioned this subject, excepting Mr. Kirwan, who, in the conclusion of his valuable papers on the attractive powers on mineral acids, says "I have always found silver to be easily precipitated from its solution in the nitrous acid by iron. The sum of the quiescent affinities being 625, and that of the divellent 746. Yet Mr. Bergman observed, that a very saturated solution of silver was very difficultly precipitated, and only by some sorts of iron, even though the solution was diluted and an access of acid added to it. The reason of this curious phenomenon appears to me to be deducible from a circumstance first observed by Scheele. in dissolving mercury, namely, that the nitrous acid when saturated with it will take up more of it in its metallic form. The same thing happens in dissolving silver in the nitrous acid in a strong heat; for, as I before remarked, the last portions of silver thrown in afford no air, and consequently are not dephlogisticated. Now this compound of calx of silver, and silver in its metallic form, may well be unprecipitable by iron, the silver in its metallic form preventing the calx from coming into contact with the iron, and extracting phlogiston from it." In this paper I shall not enter into the explanation of these appearances; but I thought it necessary to premise that what so eminent a chemist as Mr. Kirwan has suggested on the subject, that the reader may see at once the present state of the question. I shall only remark that the above explanation, not being founded on any peculiarity in the nature of iron, seems to suppose that the silver is also incapable of being precipitated from such solutions as iron, cannot act on by any other metal. But this is not the case; copper and zinc readily precipitate silver from these solutions.

in the solution at the bottom of the wine glass, where it continued during several weeks, without suffering any change, or effecting any precipitation of the silver.

3. When the solution of silver was completely saturated, it was no longer affected by iron, according to Bergman's observation.

4. Having found that the solution acted on the iron, and was thus precipitated, before it had been saturated, and not afterwards, I was desirous of knowing, whether the saturation was the circumstance which prevented the action and precipitation. For this purpose I added to a portion of the saturated solution some of the same nitrous acid, of which a part had been employed to dissolve the silver; and into this mixture, abounding with a superfluous acid, I threw a piece of iron, but no precipitation occurred. It was thence evident that the saturation of the acid was not the only circumstance which prevented the precipitation.

5. To another portion of the saturated solution of silver I added some red smoking nitrous acid; and I found, on trial, that iron precipitated the silver from this mixture, and that the same appearances were exhibited as had been observed with the solution before its saturation.

6. The same effects were produced when vitriolic acid was added to the saturated solution of silver, and iron afterwards applied.

7. To some of the same nitrous acid, of which a part had been employed to dissolve the silver, I added a piece of iron; and while the iron was dissolving I poured into the liquor some of the saturated solution of silver, on which a precipitation of silver took place instantly; though when the same acid had been previously mixed with the solution of silver, and the iron was then added to the mixture, no precipitation had ensued.

8. The quantity of vitriolic acid, or of the red fuming nitrous acid, necessary to communicate to the saturated solution of silver the property of being acted on by iron, varies according to the concentration, and to the degree of phlogistication of the acids added; so that a less quantity than is sufficient does not produce any apparent effect. Yet, when the solution is by the addition of these acids brought nearly to a precipitable state, the addition of spirit of wine will, in a little time, render it capable of acting on iron.

9. It appears then, that a solution of silver is not precipitated by iron in the cold, unless it have a superabundance of phlogisticated acid.[*]

---

[*] It was said, at section four, that the addition of dephlogisticated nitrous acid to a saturated solution of silver did not render this solution precipitable by iron. Yet, as this acid dissolves iron, such a quantity may be added, as to overcome the counteracting quality of the solution of silver, so that the acid shall be able to act on the iron; and while this metal is dissolving, it phlogisticates the mixture, which then becomes capable of being precipitated, and is in fact re-

10. Heat affects the action of a solution of silver on iron; for if iron be digested with heat, in a perfectly saturated solution of silver; such as a solution of crystals of nitre of silver in water, the silver will be deposited in its bright metallic state on different parts of the iron, and the iron which has been acted on by the solution appears in the form of a yellow ochre.

11. Bergman relates, that he has sometimes observed beautiful crystallizations or vegetations of metallic silver formed on pieces of iron immersed long in a solution of silver, I have found that no trial is able to effect this deposition, unless the solution be in a state nearly sufficiently phlogisticated to admit of a precipitation by iron, but not completely phlogisticated enough to effect that purpose immediately.

12. Dilution with a great deal of water seemed to dispose the solutions of silver to be precipitated by iron more easily. A solution of silver, which did not act on iron, on being very much diluted, and having a piece of iron immersed in it, during several hours, gave a precipitate of silver in the form of a black powder.

§ 2. *On the alterations which iron or its surface undergoes by the action of a solution of silver in nitrous acid, or of a pure concentrated nitrous acid.*—13. It has been said, that when iron is exposed to the action of a phlogisticated solution of silver, it instantly precipitates the silver, is itself acted on or dissolved by the acid solution during a certain time, longer or shorter, according to the degree of phlogistication, quantity of superabundant acid, and other circumstances, and that at length the solution of the iron ceases; the silver precipitate is redissolved, if there is superfluous acid; the liquor becomes clear again, but only rendered a little browner by having dissolved some iron; while the piece of iron remains bright and undisturbed at the bottom of the liquor, where it is no longer able to affect the solution of silver.

14. I poured a part of the phlogisticated solution of silver which had passed through these changes, and which had ceased to act on the piece of iron, into another glass, and dropped another piece of iron wire into the liquor; on which I observed a precipitation of silver, a solution of part of the iron, a redissolution of the precipitated silver, and a cessation of all these phenomena, with the iron remaining bright, and quiet at the bottom of the liquor, as before. It appeared then, that the liquor had not lost its power of acting

---

duced to the same circumstances as are described at section 7. The limits of the quantities which produce changes cannot be ascertained, because they depend on the degrees of concentration and phlogistication of the substances employed. and therefore, whenever a change is said to be produced by a certain substance, it means that it may be produced by some proportion, but does not imply by every proportion, of that substance. Without attending to these considerations, persons trying to repeat the experiments mentioned in this paper will be liable to be deceived.

on fresh iron, though it ceased to act on that piece which had been exposed to it.

15. To one of the pieces of iron which had been employed in the precipitation of a solution of silver, and from which the solution, no longer capable of acting on it, had been poured off, I added some phlogisticated solution of silver, which had never been exposed to the action of iron, but no precipitation happened. It appeared then, that the iron itself, by having been once employed to precipitate a solution of silver, was rendered incapable of any further action on any solution of silver. And it is to be observed, that this alteration was produced without the least diminution of its metallic splendour, or change of colour. The alteration however, was only superficial, as may be supposed; for by scraping off its altered coat, it was again rendered capable of acting on a solution of silver. To avoid circumlocution, I shall call iron thus affected, *altered iron;* and iron which is clean, and has not been altered, *fresh iron.*

16. To a phlogisticated solution of silver, in which a piece of bright *altered iron* lay, without action, I added a piece of *fresh iron,* which was instantly enveloped with a mass of precipitated silver, and acted on as usual; but, what is very remarkable, in about a quarter of a minute, or less, the *altered iron* was suddenly covered with another coat of precipitated silver, and was now acted on by the acid solution like the *fresh* piece. In a little time the silver precipitate was redissolved, as usual, and the two pieces of iron were reduced to an *altered* state. When a fresh piece was then held in the liquor, so as not to touch the two pieces of *altered* iron, they were also soon acted upon by the acid solution, and suddenly covered with silver precipitate as before; and these phenomena may be repeated with the same solution of silver, till the superfluous acid of the solution becomes saturated by the iron, and then the dissolution of the precipitated silver must cease.

17. I poured some dephlogisticated nitrous acid on a piece of *altered iron,* without any action ensuing, although this acid readily acted on *fresh iron;* and when, to the dephlogisticated nitrous acid, with a piece of *altered iron* lying immersed in it, I added a piece of *fresh iron,* this immediately began to dissolve, and soon afterwards the altered iron was acted on also by the acid.

18. On a piece of *altered iron* I poured a solution of copper in nitrous acid; but the copper was not precipitated by the iron; neither did this iron precipitate copper from a solution of blue vitriol.

19. *Altered iron* was acted on by a dilute phlogisticated nitrous acid; but not by a red concentrated acid, which is known to be highly phlogisticated.

20. I put some pieces of clean fresh iron wire into a concentrated

and red fuming nitrous acid. No apparent action ensued, but the iron was found to be altered in the same manner as it is by a solution of silver: that is, it was rendered incapable of being attacked either by a phlogisticated solution of silver, or by dephlogisticated nitrous acid.

21. Iron was also *altered* by being immersed some little time in a saturated solution of silver, which did not show any visible action on it.

22. The alteration thus produced on the iron is very superficial. The least rubbing exposes some of the *fresh iron* beneath its surface, and thus subjects it to the action of the acid. It is therefore with difficulty that these pieces of *altered iron* can be dried without losing their peculiar property. For this reason, I generally transferred them out of the solution of silver, or concentrated nitrous acid, into any other liquor, the effects of which I wanted to examine. Or they may be transferred first into a glass of water, and then into the liquor to be examined. But it is to be observed, that if they be allowed to remain long in the water, they lose their peculiar property or alteration. They may be preserved in their altered state by being kept in spirit of sal ammoniac.

23. To a saturated solution of copper in nitrous acid, which was capable of being readily precipitated by fresh iron, I added some saturated solution of silver. From this mixture a piece of *fresh iron* neither precipitated silver nor copper: nor did the addition of some dephlogisticated nitrous acid effect this precipitation.

24. A solution of copper, formed by precipitating silver from nitrous acid by means of copper, was very reluctantly and slowly precipitated by a piece of *fresh iron*; and the iron thus acted on by the acid was changed into an ochre.

25. A saturated solution of silver having been partly precipitated by copper, acquired the property of acting on *fresh iron*, and of being precipitated by it.

26. Fresh iron immersed sometime in solutions of nitre of lead, or of nitre of mercury in water, did not occasion any precipitation of the dissolved metals; but acquired an altered quality. These metals then in this respect resemble silver.

27. It is well known, that a solution of martial vitriol, added to a solution of gold in aqua regis, precipitates the gold in its metallic state. I do not recollect, that the precipitation of a solution of silver, by the same vitriol, has been observed. However, on pouring a solution of martial vitriol into a solution of silver in the nitrous acid, a precipitate will be thrown down, which acquires, in a few minutes, more and more of a metallic appearance, and is indeed perfect silvar. When the two solutions are partly concentrated, a bright argentine film swims on the surface of the mixture, or silvers the side of the glass in which the experiment is made. When a

phlogisticated solution of silver is used, the mixture is blackened, as happens generally to a solution of martial vitriol, where phlogisticated nitrous acid is added to it.

I added about equal parts of water to a mixture of a phlogisticated solution of silver and a solutien of martial vitriol, in which all the silver had been precipitated, and digested the diluted mixture with heat, by which means most of the precipitated silver was redissolved. Bergman has observed a similar redissolution of gold precipitated by martial vitriol on boiling the mixture: but he attributes the redissolution to the concentration of the aqua regis by the evaporation. As this explanation did not accord with my notions, I diluted the mixture with water, and found that the same redissolution occurred both with the solution of silver and with that of gold. But with neither of the metals did I find that the redissolution took place, unless there had been a superabundant acid in the solutions of silver and gold employed.

28. Mercury is also precipitated in its metallic state from its solution in nitrous acid, by a solution of martial vitriol. When the liquor is poured off from its precipitate, this may be changed into running mercury by being dried near the fire.

29. I found also that silver may be precipitated in its metallic state, from its solution in vitriolic acid, by addition of a solution of martial vitriol. A vitriol of mercury may also be decompounded by a solution of martial vitriol, and the mercurial precipitate, which is a black powder, forms globules, when dried and warmed.

30. Luna cornea is not decompounded by martial vitriol; consequently there is no operation of a double affinity. Yet this luna cornea may be decompounded by the elements of martial vitriol, while they are in the act of dissolution: that is, the silver may be precipitated in its metallic state, by digesting luna cornea with a dilute vitriolic acid, to which some pieces of iron are added. And it is to be observed, that this reduction of the silver and precipitation take place, while the acid is yet unsaturated. Marine acid and iron applied to luna cornea effect the same reduction of the silver to a metallic state, even when there is more acid than is sufficient for both metals.

The explanation of these phenomena will be attempted in the subsequent papers which I propose to present on this subject to the society.*

---

* We are not aware that Mr. Keir ever favoured the scientific world with the explanation here proposed.—Edit.

LIV.—*Brief Synopsis of the Principles of* MR. JAMES P. ESPY'S *Philosophy of Storms.*[*]

When the air near the surface of the earth becomes more heated or more highly charged with aqueous vapour, which is only five-eighths of the specific gravity of atmospheric air, its equilibrium is unstable, and up-moving columns or streams will be formed.

As these columns rise, their upper parts will come under less pressure, and the air will therefore expand; as it expands, it will grow colder about one degree and a quarter for every hundred yards of its ascent, as is demonstrated by experiments on the Nephelescope.

The ascending columns will carry up with them the aqueous vapour which they contain, and if they rise high enough, the cold produced by expansion from diminished pressure, will condense some of this vapour into cloud; for it is known that cloud is formed in the receiver of an air-pump when the air is suddenly withdrawn.

The distance or height to which the air will have to ascend before it will become cold enough to begin to form cloud, is a variable quantity depending on the number of degrees which the dew point is below the temperature of the air; and this height may be known at any time by observing how many degrees a thin metallic tumbler of water must be cooled down below the temperature of the air, before the vapour begins to condense on the outside. The highest temperature at which it will condense, which is variable according as there is more or less vapour in the air, is called the "dew point," and the difference between the dew point and the temperature of the air in degrees is called the complement of the dew point.

It is manifest that if the air at the surface of the earth should at any time be cooled down a little below the dew point, it would form a fog by condensing a small portion of its transparent vapour into little fine particles of water, and if it should be cooled 20° below the dew point, it would condense about one half its vapour into water, and at 40° below, it would condense about three-fourths of its vapour into water, &c.

This, however, will not be exactly the case from the cold produced by expansion in the up-moving columns; for the vapour itself grows thinner, and the dew point falls about one-quarter of a degree for every hundred yards of ascent.

It follows, then, as the temperature of the air sinks about one degree and a quarter for every hundred yards of ascent, and the dew point sinks about a quarter of a degree, that as soon as the column rises as many hundred yards as the complement of the dew point contains degrees of Fahr., cloud will begin to form. Or in other words, the bases of all clouds forming by the cold of diminished pressure from up-moving columns of air, will be about as many hundred yards high as the dew point is below the temperature of the air at the time.

---

[*] Copious facts going to establish the principles contained in this Synopsis are given in Mr. Espy's Lectures.

If the temperature of the ascending column should be 10° above that of the air through which it passes, and should rise to the height of 4800 feet before it begins to form cloud, the whole column would then be 100 feet of air lighter than surrounding columns; and if the column should be very narrow, its velocity of upward motion would follow the laws of spouting fluids, which would be eight times the square root of 100 feet a second, that is 80 feet a second, and the barometer in the centre of the column at its base, would fall about the ninth of an inch.

As soon as cloud begins to form, the caloric of elasticity of the vapour or steam is given out into the air in contact with the little particles of water formed by the condensation of the vapour. This will prevent the air in its further progress upwards from cooling so fast as it did up to that point, and from experiments on the Nephelescope, it is found to cool only about one half as much above the base of the cloud as below—that is, about five-eighths of a degree for one hundred yards of ascent, when the dew point is about 70°. If the dew point is higher it cools a little less, and if the dew point is lower, it cools a little more than five-eighths of a degree in ascending one hundred yards.

Now it has been ascertained by aëronauts and travellers on mountains, that the atmosphere itself is about one degree colder for every hundred yards in height above the surface of the sea; therefore, as the air in the cloud, above its base, is only five-eighths of a degree colder for every hundred yards in height, it follows, that when the cloud is of great perpendicular height above its base, its top must be much warmer than the atmosphere at that height, and consequently much lighter.

Indeed the specific gravity of a cloud of any height compared to that of the surrounding air at the same elevation, may be calculated when the dew point is given. For its temperature is known by experiments with the Nephelescope, and the quantity of vapour condensed by the cold of diminished pressure at every point in its upward motion, and of course the quantity of caloric of elasticity given out by this condensation is known, and also the effect this caloric has in expanding the air receiving it, beyond the volume it would have, if no caloric of elasticity was evolved in the condensation of the vapour.

For example, according to the experiments of Prof. Walter R. Johnson, of Philadelphia, a pound of steam at the temperature of

212° contains 1030 of caloric of elasticity, and as the sum of the latent and sensible caloric of steam is the same at all temperatures, it follows that a pound of steam being condensed into 1198 pounds of water at 32° would heat it up 1°. And as the specific caloric of air is only 0.267, if a pound of vapour should be condensed into 1198 pounds of air, it would heat that air nearly 4°, or which is the same thing, it would heat 119 pounds of air 4°, or 100 pounds 48°, and in all these cases it would expand the air about 8000 times the bulk of water generated;' that is, 8000 cubic feet for every cubic foot of water formed out of the condensed vapour. And as it requires between 1300 and 1400 cubic feet of vapour, at the ordinary temperature of the atmosphere, to make one cubic foot of water—if this quantity be subtracted from 8000, it will leave upwards of 6600 cubic feet of actual expansion of the air in the cloud for every cubic foot of water generated there by condensed vapour.

This great expansion of the air in the forming cloud will cause the air to spread outwards in all directions above, causing the barometer to rise on the outside of the cloud above the mean, and to fall below the mean under the middle of the cloud as much as it is known to do in the midst of great storms.

For example, if the dew point should be very high, say 78°, then the quantity of vapour in the air would be about one-fiftieth of its whole weight, and if the up-moving column should rise high enough to condense one-half its vapour into cloud, it would heat the air containing it 45°, and the air so heated would be 45-480ths larger than it would be if it was not so heated. And if we assume a case within the bounds of nature, and suppose the cloud and the column under the cloud to occupy three-fourths of the whole weight of the atmosphere, or in other words, if we suppose the top of the cloud to reach a height where the barometer would stand at 7½ inches, and the mean temperature of the whole column 40° warmer than the surrounding air, then would the barometer fall under the cloud at the surface of the earth 40-480ths of 22.5, or a little more than two inches.

Though the air may be driven up by the ascending column much higher than the point assumed in the last article, the cloud will cease to form at greater heights, because the dew point at these great elevations, falls by a further ascent as rapidly as the temperature—and at greater elevations, it will even fall more rapidly. If for instance the air should rise from where the barometer stands at six inches to where it stands at three inches, the dew point would fall about 20°, but the temperature would fall less than 20°, and therefore no vapour would be condensed by such ascent.

When a cloud begins to form from an ascending column of air, it will be seen to swell out at the top while its base continues on the same level, for the air has to rise to the same height before it becomes cold enough by diminished pressure to begin to condense its vapour into water; this will cause the base to be flat, even after the cloud has acquired great perpendicular height, and assumed the form of a sugar loaf. Other clouds also for many miles around, formed by other ascending columns, will assume similar appearances, and will moreover have their bases all on the same or nearly the same horizontal level; and the height of these bases from the surface of the earth will be the greatest about three o'clock, when the dew point and temperature of the air is the greatest distance apart.

The outspreading of the air in the upper parts of an ascending column will form an annulus all round the cloud, under which the barometer will stand above the mean; of course the air will descend in the annulus, and increase the velocity of the wind at the surface of the earth towards the centre of the ascending column, while all round on the outside of the annulus there will be a gentle wind outwards. Any general currents of air which may exist at the time, will of course modify these motions from the oblique forces they would occasion.

The up-moving current of air must of course be entirely supplied by the air within the annulus, and that which descends in the annulus itself.

The rapid disturbance of equilibrium, which is produced by *one* ascending column, will tend to form *others* in its neighbourhood; for the air being pressed outwards from the annulus, or at least retarded on the windward side, will form other ascending columns, and these will form other annuli, and so the process will be continued.

These ascending columns will have a tendency to approach, and finally unite; for the air between them must descend, and in descending the temperature of the whole column will increase, for it is known that the air, at great elevation, contains more caloric to the pound than the air near the surface of the earth, because it is the upper regions that receive the caloric of elasticity given out in the condensation of vapour into clouds. Therefore, when the air has descended some time in the middle, between two ascending columns, the barometer will fall a little, or at least not stand so high above the mean as it does on the outside of the two clouds, and so the columns will be pressed towards each other.

If one of two neighbouring columns should be greatly higher than the other, its annulus may overlap the smaller one, and of course the current under the smaller cloud will be inverted, and the cloud which may have been formed over the column thus forced to descend, will soon disappear; for as it is forced downwards by the overlapping annulus of the more lofty column, it will come under greater pressure, and its temperature will be thus increased, and it is manifest that as soon as its top descends as low as its base, it will have entirely disappeared, and in the mean time the larger cloud will have greatly increased.

As the air above the cloud formed by an ascending column is forced upwards, if it contains much aqueous vapour, a thin film of cloud will be formed in it by the cold of diminished pressure, entirely distinct from the great dense cumulus below; but as the cumulus rises faster than the air above it (for some of the air will roll off) the thin film and the top of the cumulus will come in contact; and sometimes a second film or cap may be formed in the same way, and perhaps a third and fourth. When these caps form, there will probably be rain, as their formation indicates a high degree of saturation in the upper air.

When the complement of the dew point is very great (20° and more,) clouds can scarcely form; for up-moving columns will generally either come to an equilibrium with surrounding air, or be dispersed before they rise twenty hundred yards, which they must do in this case, before they form clouds. Sometimes, however, masses of air will rise high enough to form clouds, but they are generally detached from any up-moving column underneath, and of course cannot then form cumuli with flat bases; such clouds will be seen to dissolve as soon as they form, and even while forming they will generally appear ragged, thin and irregular.

Moreover, if the ground should be colder during the day, than the air in contact with it, as it sometimes happens after a very cold spell of weather, then ascending columns cannot exist, and of course no cumuli can be formed on that day, even though the air may be saturated with vapour to such a degree as to condense a portion of it on cold bodies at the surface of the earth.

Neither can clouds form of any very great size, when there are cross currents of air sufficiently strong to break in two an ascending current, for the ascensional power of the up-moving current will thus be weakened and destroyed. This is one means contrived by nature to prevent up-moving columns from increasing until rain would follow. Without some such contrivance it is probable that every up-moving column which should begin to form cloud when the dew point is favourable, would produce rain, for as soon as cloud forms, the up-moving power is rapidly increased by the evolution of the caloric of elasticity.

If it should be found by observation that an upper current of air is passing from the mountains of Abyssinia over Egypt to the north, while the wind below is blowing from the north towards the mountains of Abyssinia, this would manifestly be *one* reason why it seldom rains in Egypt during the prevalence of this wind, though it comes highly charged with vapour from the Mediterranean. Besides, it is known that during the prevalence of this wind there are great rains in Abyssinia, and of course if the upper current does flow over Egypt from the south, it would bring in it a large portion of the caloric of elasticity, which it received there, in the great condensation of the vapour as it rose up the sides of the mountains at the head of the Nile; of course the columns of air rising over Egypt, when they entered that current would cease to rise, for the temperature of that current would be many degrees hotter than themselves, and therefore they could not swim in it.

Also, on the leeward side of very lofty mountains, there cannot be rain: for as the air on the windward side rises up the sides of the mountain, it will condense all the vapour which can be condensed by the cold of diminished pressure, before it reaches to the top, and even if a cloud passes over the top to the other side, it would soon disappear, because in passing down the slope it will come under greater pressure, and thus be dissolved by the heat produced. These are some of the causes which prevent rains at particular times and in particular localities.

If, however, the air is very hot below with a high dew point, and no cross currents of air above to a great height, then, when an up-moving current is once formed, it will go on and increase in violence, as it acquires perpendicular elevation, especially after the cloud begins to form. At first the base of the cloud will be flat; but after the cloud becomes of great perpendicular diameter, and the barometer begins to fall considerably, as it will do from the specific levity of the air in the cloud, then the air will not have to rise so far as it did at the moment when the cloud began to form, before it reaches high enough to form cloud from the cold of diminished pressure.

The cloud will now be convex below, and its upper parts will be seen spreading outward in all directions, especially on that side towards which the upper current is moving, assuming something of the shape of a mushroom. In the mean time the action of the inmoving current below and upmoving current in the middle will become very violent, and if the barometer falls two inches under the centre of the cloud, the air will cool about 10°, and the base of the cloud will reach the earth if the dew point was only 8° below the temperature of the air at the time the cloud began to form. The shape of the lower part of the cloud will now be that of an inverted cone with its apex on the ground, and it will be what is called a tornado if it is on land, and a water-spout if at sea.

On visiting the path of a tornado, the trees on the extreme borders will all be found prostrated with their tops inwards, either inwards and backwards, or inwards and forwards, or exactly transverse to the path. The trees in the centre of the path will be thrown either backwards or forwards parallel to the path; and invariably if one tree lies across another, the one which is thrown backwards is underneath. Those materials on the sides which are moved from their places and rolled along the ground, leaving a trace of their motion, will move in a curve convex behind; those which were on the right hand of the path, will make a curve from left hand to right, and those on the left hand of the path will make a curve from right hand to left, and many of these materials will be found on the opposite side of the path from that on which they stood on the approach of the tornado. Also, those bodies which are carried up will appear to whirl, unless they arise from the very centre—those that are taken up on the right of the centre will whirl in a spiral from left to right, and those on the left of the centre, will whirl in a spiral upwards from right to left. On examining the trees which stand near the borders of the path, it will be found that many of the limbs are twisted round the trees and broken in such a manner as to remain twisted, those on the right hand side of the path from left to right, and those on the left hand side of the path from right to left. However, it will be found that only those limbs which grew on the side of the tree most distant from the path of the tornado are broken; for these alone were subject to a transverse strain.

The houses which stood near the middle of the path will be very liable to have the roof blown up, and many of the walls will be prostrated all outwards, by the explosive influence of the air within, and those houses covered with zinc or tin, from being air tight will be liable to suffer most. The floors from the cellars will also frequently be thrown up, and the corks of empty bottles exploded.

All round the tornado at a short distance, probably not more than three or four hundred yards, there will be a dead calm, on account of the annulus formed by the rapid efflux of air above, from the centre of the up-moving and expanding column. In this annulus the air will be depressed, and all round on the outside of it, at the surface of the earth, there will be a gentle wind outwards; and of course all the air which feeds the tornado, is supplied from within the annulus. Nor is this difficult to understand, when the depression of the air in the annulus is considered, for any amount may be thus supplied by a great depression.

Light bodies, such as shingles, branches of trees, and drops of rain or water formed in the cloud, which are carried up to a great height before they are permitted to fall to the earth; for though they may frequently be thrown outwards above, and then descend a considerable distance at the side, they will meet with an inblowing current below, which will force them back to the centre of the up-moving current, and so they will be carried aloft again.

The drops of rain, however, will frequently be carried high enough to freeze them, especially if they are thrown out above so far as to fall into clear air, for this air will in some cases be thirty or forty degrees colder than the air in the cloud. In this case if the up-moving column is perpendicular, the hail will be thrown out on both sides, and on examination it will be found that two veins of hail fell simultaneously, at no great distance apart.

It is indeed probable that in all violent thunder storms in which hail falls, the upmoving current is so violent as to carry drops of rain to a great height, when they freeze and become hail. It is difficult if not impossible to conceive any other way in which hail can be formed in the summer, or in the torrid zone.

In those countries in which an upper current of air prevails in a particular direction, the tornadoes and water-spouts will generally move in the same direction; because the upmoving column of air in this meteor rises far into this upper current, and of course its upper part will be passed in this direction, as the great tornado cloud moves on in the direction of the upper current, the air at the surface of the earth will be pressed up into it by the superior weight of the surrounding air. It is for this reason the tornado in Pennsylvania generally moves towards the eastward.

If a tornado should stop its motion for a few seconds, as it might do, on meeting with a mountain, it would be likely to pour down an immense flood of water or ice, in a very small space; for the drops which would be carried up by the ascending current would soon accumulate to such a degree, as to force their way back, and this they could not do, without collecting into one united stream of immense length and weight, and of course on reaching the side of the mountain, this stream, whether it consisted of water or hail, would cut down into the side of the mountain a deep hole, and make a gully all the way to the bottom of the mountain from the place where it first struck.

As the air spreads out more rapidly above than it runs in below, there will be a tendency in storms to increase in diameter, and also to become oblong from the influence of the upper current in carrying the top of the cloud in its own direction.

At the equator, or at least those parts of it where the trade winds are constant from east to west, it is probable tornadoes travel from east to west. For as the air in the torrid zone is about 80° in temperature at a mean, and the air in the frigid zone is about zero, the air in the torrid zone is constantly expanded by heat about 80-448th of its whole bulk in the frigid zone. This will cause the air at the equator to stand more than seven miles higher from the surface of the earth to the top of the atmosphere than at the north pole. The air therefore will roll off from the torrid zone both ways towards the poles, causing the barometer to fall in low latitudes and rise above the mean in high latitudes. This will cause the air to run in below towards the equator, and of course rise there. Now from the principle of the conservation of areas, it will fall more and more to the west as it rises, and of course the upper current of the air, at the equator, probably moves towards the west.

However, as the air rolls off above, towards the north, it will be constantly passing over portions of the earth's surface, which have a less diurnal velocity than the part from which it set out, and as from the nature of inertia it still inclines to retain the diurnal velocity towards the east which it originally possessed, when it reaches the latitude of about 20° or 25°, it will then probably be moving nearly towards the north—and beyond that latitude its motion will be to the northeasterly.

If violent storm clouds, which necessarily rise to a great height into the upper current, are driven forward in the direction of the upper current, it is probable that the barometer will rise higher in that part of the annulus which is in front of the storm, than in the rear, and if so, a sudden rise of the barometer in particular localities, may become, when properly understood, one of the first symptoms of an approaching storm.

In consequence of the high barometer in front of the storm in a semi-annulus, the air will be forced downwards there, and cause in some cases a more violent action of the air or wind backwards, meeting the approaching storm, than will be experienced, in the rear of the storm.

As the air comes downwards in the semi-annulus in front of the storm, it will come under greater pressure, and therefore any clouds which it may contain, will probably be dissolved, by the heat of great pressure, and therefore on the passage of the annulus, it will probably be fair weather.

Also, as the air above always contains more caloric to the pound, than the air below, there will be an increase of temperature on the passage of the annulus, partly from the increased pressure, but chiefly by the descent of the air; in very hot climates this increase of temperature, in front of the storm, will be very sensibly felt.

The increased pressure in the annulus round a volcano, when it suddenly bursts out, will sometimes under favourable circumstances, be very great, and of course the air will be depressed from a great height, so that some portion of the very air which has gone up in the central parts of the ascending column, and formed cloud by the cold of diminished pressure, will be forced down to the surface of the earth, bringing with it the caloric of elasticity which it received from the condensing vapour; if so, the heat experienced at the time of this descent, will be very great.

These hot blasts of air will alternate with cold blasts, for the air which is forced down from great heights in the annulus will not only be very hot, but very dry, having condensed its vapour, in its previous ascent. Now when this hot dry air flows inwards again towards the volcano and ascends, it will not form cloud, because of its want of vapour; and therefore the process of cloud forming will cease, and consequently rain and hail will cease too, until more air from a greater distance that has not been deprived of its vapour flows in and ascends. Then cloud will again begin to form and the violence and rapidity of the outflowing of the air above will be increased by the evolution of the caloric of elasticity, the barometer will rise rapidly in the annulus and fall in the central part of the ascending column, and these alternations may continue while the volcano is in activity, more particularly if the violence of the volcano itself should be increased periodically.

As air cannot move upwards without coming under diminished pressure, and as it must thus expand and grow colder and consequently form cloud—any cause which produces an upmoving column of air, whether that cause be natural or artificial, will produce rain, when the complement of the dew point is small, and the air calm below and above, and the upper part of the atmosphere of its ordinary temperature.

Volcanoes therefore under favorable circumstances will produce rain; sea breezes which blow inwards every day towards the centre of islands, especially if these islands have in them high mountains, which will prevent any upper current of air from bending the upmoving current of air out of the perpendicular before it rises high enough to form cloud, such as Jamaica—will produce rain every day; great cities where very much fuel is burnt, in countries where the complement of the dew point is small, such as Manchester and Liverpool, will frequently produce rain; even battles, and accidental fires, if they occur under favorable circumstances, may sometimes be followed by rain. Let all these favorable circumstances be watched for in time of drought, (and they can only occur then,) and let the experiment be tried. If it should be successful, the result would be highly beneficial to mankind.

Independent of its utility to the farmer, it would be highly useful to the mariner in the following way.

As the very time and place of the commencement of the rain would be known, it would be easy to find out in what direction from the place of beginning it moved along the surface of the earth. and also its velocity of motion, and the shape that it assumed, from time to time in its progress. Now this knowledge is the principal thing wanting to enable the mariner, who has the power of locomotion, to direct his vessel so, when one of these great storms comes near him, as to use as much wind in the borders of the storm as will suit the purposes of navigation—for heaven undoubted makes the wind blow for his use and not for his destruction, provided he becomes acquainted with the laws to which it is subject. From the preceding principles he will be able to know in what direction a great storm is raging when it is yet several hundred miles from him.—*From Silliman's Journal.*

LV.—*On the Electricity of a Jet of Steam issuing from a Boiler. By* H. G. Armstrong, *Esq., in Letters to Professor* Faraday.*

Sir,

A few days ago, I was informed that a very extraordinary electrical phænomenon, connected with the efflux of steam from the safety-valve of a steam-engine boiler, had been observed at Seghill, about six miles from Newcastle. I therefore took an early opportunity of going over to that place, to investigate the truth of what I had heard, and by so doing I have ascertained the precise facts of the case, which appear to me to possess so much novelty and importance, that I deem it right to transmit the particulars to you believing that in your hands they will prove most conducive to the advancement of science. Without further preface, I shall proceed to narrate what I saw and heard on the spot.

There is nothing remarkable in the construction of the boiler, which is supported upon brick-masonry in the usual way. The safety-valve is placed on the top of a small cylinder, having a flange round the lower end, which is fastened by bolts to the summit of the boiler, between which and the flange, a cement, composed of chalk, oil, and tow, is interposed for the purpose of making the joining steam-tight.

About three weeks ago the steam began to escape at this joining, through a fissure in the cement, and has ever since continued to issue from the aperture in a copious horizontal jet. Soon after this took place, the engine-man, having one of his hands accidentally immersed in the issuing steam, presented the other to the lever of the valve, with a view of adjusting the weight, when he was greatly surprised by the appearance of a brilliant spark, which passed between the lever and his hand, and was accompanied by a violent wrench in his arms, wholly unlike what he had ever experienced before. The same effect was repeated when he attempted to touch any part of the boiler, or any iron-work connected with it, provided his other hand was exposed to the steam. He next found that while he held one hand in the jet of steam, he communicated a shock to every person whom he touched with the other, whether such person were in contact with the boiler, or merely standing on the brickwork which supports it; but that a person touching the boiler, received a much stronger shock than one who merely stood on the bricks.

These singular effects were witnessed and experienced by a great many persons, and among others by two gentlemen with whom I am personally acquainted, and who fully corroborate the above account, which I obtained from the engine-man.

The boiler had been cleaned out the day before I saw it, and a thin incrustation of calcareous matter reaching as high as the water level had been removed, and the consequence was, that the indications of electricity, though still existing, were very much diminished. Still, however, what remained was very extraordinary; for when I placed one hand in the jet of steam and advanced the other within a small distance of the boiler, a distinct spark appeared, and was attended with a slight electrical shock.

---

* From the Philosophical Magazine.

From the effect produced by the cleaning of the boiler, it appears pretty obvious that the phenomenon is in a great measure, though not wholly, dependent upon the existence of an incrustation within; and the reason why such effects do not in any degree attend the effluxion of a jet of steam from a boiler in ordinary cases, must, I apprehend, be sought for in the fact, that in the present instance the steam escapes through an aperture in a non-conducting material, while in a vast majority of cases the escape must take place through a metallic orifice. Can the explosion of boilers, respecting the cause of which so much uncertainty at present exists, have any connexion with the rapid production of electricity which thus appears to accompany the generation of steam?

In the present case the incrustation of the boiler is very rapidly formed, and I therefore expect that in a few days the effects will have become as strong as they were at first. Whenever this takes place I shall again go over to witness them, and if you wish for any further information, I shall be glad to obtain it for you. In the mean time you are at liberty to make any use of this letter that you think fit.

I am, Sir, very respectfully yours,

H. G. ARMSTRONG.

Newcastle upon-Tyne, Oct. 14, 1840.

Newcastle-upon-Tyne, Oct. 22, 1840.

Dear Sir,—I yesterday revisited the boiler at Seghill, in company with some friends, and took with me such apparatus as I deemed necessary for experimenting on the electrical steam. The results of this second visit I now hasten to communicate, and you will find in the following account of my proceedings, answers to all the queries you were kind enough to send me, for the purpose of directing my attention to the proper points of inquiry.

I found the boiler, and every thing connected with it, precisely in the state in which I have already described it, and on trying the steam in the same way as I did on the former occasion, the effect was very nearly the same; but when I placed myself on an insulating stool, the intensity of the sparks which passed between my hand and the boiler was greatly increased, as well as the twitching sensation in the knuckles and wrist, which accompanied the operation, and which in my former letter I designated a slight electrical shock. In pursuance of your instructions, I had provided myself with a brass plate, having a copper wire attached to it, which terminated in a round brass knob. When this plate was held in the steam by means of an insulated handle, and the brass knob brought within about a quarter of an inch from the boiler, the number of sparks which passed in a minute was from sixty to seventy, as nearly as we could count; and when the knob was advanced about one-sixteenth of an inch nearer to the boiler, the stream of electricity became quite continuous. The greatest distance between the knob and the boiler, at which a spark would pass from one to the other, was fully an inch. A Florence flask, coated with brass filings on both surfaces, was charged to such a degree with the sparks from the knob, as to cause a spontaneous discharge through the glass; and several robust men received a severe shock from a small Leyden jar charged by the same process. The strength of the sparks was quite as great when the knob was presented to any conductor communicating with the ground, as when it was held to the boiler. It appeared to make very little difference in what part of the jet the plate attached to the conducting wire was held; but when a thick iron wire was substituted for the plate, the effect was greatest when the wire was held very near to the orifice. The valve was loaded at the rate of 35lbs per square inch; but the pressure of the steam fluctuated considerably, which gave me an opportunity of observing that the quantity of electricity derived from the jet increased and diminished with the pressure. The electricity of the steam was *positive,* for when the pith balls of the electrometer diverged upon an instrument connected with the steam, they were attracted by a piece of sealing wax rubbed on woollen cloth; and when a pointed wire was held by the person on the stool, under the shade of a hat, *a pencil,* and not *a star,* of electrical light became visible.

Besides the principal jet of steam which I operated upon, there were several small streams issuing from different parts of the boiler, and in each of these the electrometer indicated the presence of electricity. From the peculiar manner in which the steam blew off from the safety-valve when the weight on the lever was lifted, it was quite impossible to try any satisfactory experiment upon the steam which was allowed to escape by that means. I applied the gold leaf electrometer to various parts of the boiler, which, I ought to observe, is in direct communication with the ground by means of the steam pipes, but could scarcely detect a trace of electricity in any part of it.

The engine has another boiler besides the one in question, and the two boilers lie immediately adjacent to each other. Having been informed that similar phenomena had been discovered in this second boiler, I proceeded to apply the electrometer to some small pencils of steam which were escaping in different parts, and found the same indications which I had observed under similar circumstances in the first boiler. I then raised the safety-valve, and the column of steam which escaped from it proved as highly charged with electricity as the horizontal jet which issued from the other boiler, and in which the phenomenon had first been observed.

Upon inquiry, I found that the water used in the boilers was obtained from a neighbouring colliery, where it was pumped out of the mine, and that the same water was used for the boiler of a small high-pressure engine adjoining the colliery from which the water was procured. In order, therefore, to form an opinion whether or not the phenomena in question was dependent upon the quality of the water from which the steam was generated, I proceeded to examine the steam evolved from the boiler to which I had been referred, and which proved to be a very small one. The valve was loaded with only twenty pounds on the square inch, and I learned from the engine-man that no appearance of electricity had ever been noticed in the steam. Upon trial, however, I succeeded in obtaining very distinct sparks of electricity from the column of steam which issued from the safety-valve. The sparks were certainly weaker than those obtained at the other engine, but this may reasonably be ascribed to the inferior pressure of the steam, and smaller size of the boiler.

I then repaired to another high-pressure engine, which belonged to the same establishment, and the boiler of which was supplied with *rain* water instead of that drawn from the mine. In this case the pressure of the steam was forty pounds on the square inch. The valve was inaccessible, but a powerful jet of steam was obtained from the upper guage-cock; I could not, however, obtain any trace of electricity in the steam from this boiler, not even sufficient sensibly to affect the gold-leaf electromoter. The presumption, then, is exceedingly strong, that the phenomenon is in some way occasioned by the peculiar nature of the water from which the steam is produced. I enclose you a specimen of the incrustation*, of a month's growth, deposited by the water from the mine in the boilers in which it is used.

I shall be glad to receive any further instructions from you as to the proper mode of pursuing the investigation, and should be much gratified to hear your opinion as to the cause of this most curious phenomenon †.

I am, dear sir,
Very respectfully yours,
H. G. ARMSTRONG.

M Faraday, Esq.

---

* The incrustation is grey and hard; it contains traces of a soluble muriate and sulphate, but consists almost entirely of sulphate of lime, with a little oxide of iron and insoluble clayey matter, carried in probably by the water. There is hardly a trace of carbonate of lime in it.—M. F.

† The evolution of electricity by vaporization, described by Mr. Armstrong

LVI.—*Experiments on the Electricity of High-Pressure Steam.* By H. L. Pattinson, Esq., F.G.S.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

A very singular phenomenon, viz., the production of electricity by two steam-boilers, has been observed in this neighbourhood within the last few weeks, the particulars of which I have the pleasure of transmitting to you for publication in your valuable Journal. The boilers in question are situated at Cramlington Colliery, eight miles north-east of Newcastle, where they supply steam to a high-pressure engine of 28-horse power, employed on the waggon-way to haul full and empty waggons to the top of two inclined planes, leading to the colliery on the one hand, and to the river Tyne on the other. The boilers are cylindrical, with circular ends, each twenty-one feet long, and five feet diameter. They are supplied with water from an adjacent pond by iron feed-pipes, four inches diameter, and the steam they produce is conveyed to the working cylinder by other iron pipes, six inches diameter, which pipes form also a direct metallic communication between the tops of the boilers. By means of appropriate valves the steam is supplied to the cylinder from one or other boiler at pleasure. A pipe, two inches diameter, leads from the bottom of one boiler on the outside of the brick-work to the ash-pit, through which the sediment deposited by the water is occasionally blown from one of Scott's patent collecting cones, and a similar pipe is attached to the other boiler. The boilers are set in brick-work in the usual way, the fires below, with flues reaching all round, and passing into the chimney also in the usual manner. The flues are covered with large flat bricks, and in the space between the boilers the two flues are necessarily separated by a brick wall. The safety-valves are attached to the boilers by flange joints; and between the flanges, to render them steam-tight, is placed a ring of plaited hemp covered with a cement of litharge, sand and linseed oil, mixed up together, and when applied of the consistence of glaziers' putty. This cement, as it soon becomes hard, is used about the engine for steam joints which occasionally fail; but all the joints of the pipes are made of iron borings and sal-ammoniac, as ordinarily employed by

---

is most likely the same as that already known to philosophers on a much smaller scale, and about which there are as yet doubts whether it is to be referred to a mere evaporation, as Harris says, or to chemical action, according to others. This point it neither settles nor illustrates; but it gives us the evolution of electricity during the conversion of water into vapour, upon an enormous scale, and therefore brings us much nearer to the electric phenomena of volcanos, water-spouts and thunder-storms, than before.—M. F.

engine-wrights. The steam is worked at a pressure of thirty-five pounds per inch.

The joint between the top of one of the boilers and the seat of its safety-valve had given way, and steam was issuing forcibly through this aperture, when on Tuesday, September 29th last, the engine-man, William Patterson, while standing with this current of steam blowing upon his legs, took hold of the weight attached to the lever of the safety-valve, to try the strength of the steam, when he felt a peculiar pricking sensation in the ends of his fingers, but as the steam prevented him from seeing distinctly, he thought he had merely struck his fingers rather suddenly against the weight. On Friday, October 2nd, on taking hold of the lever, he again felt a sensation in his fingers of the same kind as before; and on Saturday, the 3rd, on touching the weight, this sensation was stronger, and more distinct; so much so, as to arrest his attention and lead him to mention it to some other workmen employed about the engine, who all handled the weight, and convinced themselves there was something about it very unusual. During the time they were thus employed, Patterson applied his finger gently to the lever, and perceived a spark. This was repeated by the whole party, and they soon found that sparks could be obtained from any part of the end of the boiler, as far as the valve upon the steam-pipe connecting the two boilers, and also from the pipe through which the sediment is blown, as already described. They observed further, that while standing in the volume of steam issuing from the joint, and touching the boiler, these sparks were always much stronger than when the boiler was touched by a person not in the current of steam. In one or two cases, according to their account, when the current of steam issuing from the joint was very strong, the person exposed to it being probably partially insulated by standing upon the dry and warm brick-work surrounding the boiler, gave strong sparks to others out of the current on bringing his hands to theirs; and once or twice they felt, under these circumstances, something like a slight electrical shock. It may be observed, that at this time the weather was exceedingly fine and dry. It was not long before the engineer of the colliery, Mr. Marshall, became acquainted with these circumstances, and his first feeling was to apprehend that the boiler was in danger of exploding, for, as he said, "when there was fire on the outside of the boiler, he did not know what there might be within." He accordingly sent to Messrs. Hawks's, of Gateshead, who built the boiler, for a person to examine it, and Mr. Golightly, their manager in that department, went out on Wednesday, the 7th inst., for that purpose. He gave his opinion as to the safety of the boiler, and returned much surprised at the phenomena it presented. The singular circumstance of a steam-boiler yielding electrical sparks, and giving shocks, now began to be noised abroad; and my friend, Mr. Henry Smith, of Newcastle, who had heard the account both from Mr. Golightly and Mr. Marshall, wrote me a note acquainting me with the matter, and desiring me to go with him to see it, which I did on the 11th inst., and again on the following day, having with us the second time proper electrical apparatus. On our first visit, the boilers being unplugged and empty, we merely satisfied ourselves as to all the particulars of their setting, etc., already detailed. Next day, on our arrival, we found the engine at work, the steam up to a pressure of thirty-five pounds an inch, and blowing off strongly at the joint in the boiler. The day was a little damp, but yet not unfavourabe, and we were informed on alighting that the indications of electricity were very faint and weak; however, we proceeded to our examination, of which the following is the result.—

1. On touching the boiler with the blunt point of a penknife anywhere about the circular end, the weight or the safety-valve itself, with the steam strongly blowing out of the joint, but with no part of the person exposed to the volume of steam, no spark could be perceived whatever.

2. On immersing one hand in the current of steam, and touching the parts of the boiler already named with the point of a penknife held in the other, a very minute but distinct spark was perceived, and this occurred equally on all parts of the boiler, or safety-valve, within reach.

3. By standing in the current of steam, so as to allow it to blow forcibly upon the person, the spark became larger; it was then one-eighth of an inch long.

4. On holding a large shovel in the current of steam with one hand, and touching the boiler with a penknife held in the other, a spark was obtained three-eighths of an inch long.

5. The cap of a gold-leaf electrometer, the bottom of which was held in the hand, was applied to the weight, the body of the operator being entirely out of the current of steam; and no divergence was produced whatever.

6. The electrometer held in the hand had its cap applied to the weight, the other hand of the operator being immersed in the current of steam: strong divergence was immediately produced.

From this it was evident that the electricity proceeded from the steam; but as the boiler-house was damp, so that insulation by glass could not well be preserved, a copper wire was attached to the shovel already mentioned, the end of which wire terminated in the engine-house, some yards distant from the boiler-house, where was placed a table. The shovel was held by Mr. Smith in the current of steam, with its edge about an inch and a half from the aperture through which the steam issued, and the wire leading away from the shovel was insulated by being attached to sticks of sealing-wax held by assistants. Mr. Smith stood on an insulating stool.

7. On touching a pith-ball electrometer, the threads of which were five inches long, with the insulated wire leading from the shovel held as mentioned, the balls diverged four inches with positive electricity.

8. The wire was attached to an insulated tin conductor, when it yielded sparks half an inch in length.

9. A pointed wire attached to this conductor exhibited the brush of light a quarter of an inch long, which always attends the escape of positive electricity from a point into the air.

10. A small jar was now charged so strongly as to give a rather disagreeable shock. By this time a large crowd of men, women and boys from the " Pit Raw," or pitmen's residences near the colliery, attracted by the novelty and singularity of the circumstances, had gathered about us, filling the engine-house and looking on with great curiosity and interest. A circle of sixteen of these men and women was formed, and they received together, much to their surprise and merriment, a powerful shock from the charged jar. This was several times repeated, the numbers receiving the shock varying each time from twelve to twenty.

11. A stout card was perforated by a discharge of the jar; and cotton wrapped round the end of a copper wire and dipped in pounded resin, readily set on fire.

12. When the edge of the shovel was made to approach the aperture through which the steam issued as near as three-quarters of an inch, very vivid and bright sparks of that length passed continually between it and the boiler.

13. The second boiler did not discharge steam through any fissure, but on lifting its valve by the hand it blew off in a strong current. When the shovel was held in one hand in this current of steam issuing from the safety-valve, and the boiler was touched with a penknife held in the other, a spark passed exactly, as under the same circumstances in the boiler subjected to the above experiments.

From this it would appear that the steam of both boilers was in the same electrical condition.

During the whole of these experiments the engine was doing its work as usual, occasionally going and occasionally standing; but no difference was observed in the electricity given off by the steam.

I have been most careful to supply an exact account of the facts of this extraordinary, and, as far as I know, unprecedented case, but I do not offer any theory to account for the phenomena. It is hardly possible to suppose that there is any local peculiarity about these boilers, or the place where they are situated, to occasion the highly electrical condition of the steam produced in them; and yet it is as difficult to suppose the fact of high-pressure steam being electrical, a general one; for if it were so, it could hardly, up to this time, have escaped observation. The conditions, therefore, under which steam becomes electrical require to be investigated, and it is not unlikely that the investigation may lead to important results. I am, Gentlemen,

Your obedient Servant,

H. L. Pattinson.

Bentham Grove, Gateshead,
October 19, 1840.

Ibid.

LVII.—*Specification of a patent for an Improvement in Manufacturing White Lead.* Granted to Smith Gardner, city of New York, August 28, 1840.

To all whom it may concern: Be it known, that I, Smith Gardner, of the city of New York, in the state of New York, have invented an improvement in the process of manufacturing white lead, known to the chemist under the name of carbonate of lead; and I do hereby declare that the following is a full and exact description thereof.

The first part of my procedure consists in the treating of metallic lead by the well known process by which a pulpy substance is produced, which is known to manufacturers under the name of suboxide of lead. This process consists in the placing of granulated lead, or lead in fragments, in vessels lined with sheet lead, and containing water. These vessels may be in a cylindrical form, and made to revolve on their axes, like barrel churns, or they may have a reciprocating instead of a revolving motion; and they may be, and have been, varied in form in different ways, the only essential point in their construction being that the lead contained within them may be subject to continued attrition. Thus far, the process is indentical with that which has been adopted and followed in many manufactories, in which it has been attempted to manufacture white lead from the suboxide of lead so produced.

In these attempts it has been proposed to carbonate the suboxide of lead, by putting portions of carbonate of potash, carbonate of soda, or other carbonates, into the water with the lead undergoing attrition, it having been supposed that the alkaline carbonate would give up its carbonic acid to the oxide of lead, as said oxide was formed. Independently of the known affinities of the respective articles named, I have proved, by repeated experiments, on a large scale, that carbonate of lead cannot be produced in that way. Another attempt to convert the suboxide of lead, obtained by trituration, into white lead, has been by taking the said pulpy oxide, agitating it in a vessel containing water, and forcing a stream of carbonic acid, or of carbonic acid mixed with atmospheric air, through it. By this process a carbonate of lead has been produced, but in so imperfect a manner, as to leave it destitute of all the essential properties of that article; wanting the density, body, and freedom from colour, found in good white lead. In consequence of these defects, the attempts hitherto made to manufacture white lead from the suboxide produced by triturating fragments of lead in leaden vessels, under water, have proved abortive; but, by a very simple variation of the process, I have succeeded in producing good white lead, which has been pronounced by judges to be equal to the best that is imported.

As it was fairly proved that the suboxide would not combine with the carbonic acid, after said suboxide had been fully formed, I determined to vary the process so as to present the carbonic acid, in conjunction with a portion of atmospheric air, to the suboxide of lead in its nascent state; and this I have found perfectly effectual. In order to effect it, I triturate my lead with water in leaden cylinders, or other vessels, as above described, but, instead of leaving the vessels open, or perforating them, for the admission of atmospheric air, I make them close, by means of suitable shutters, or stoppers, which may be removed whenever it is necessary so to do; and during the whole time that the trituration is continued, I introduce carbonic acid, accompanied by atmospheric air, into the triturating vessels. When these vessels are in the form of horizontal cylinders, I pass the gases into them through hollow gudgeons; a mode of construction and procedure well known to machinists; under other forms or modes of constructing my triturating vessels, I adopt whatever means I may consider the best for introducing the gases within them. The result of this process is, that the nascent suboxide of lead presented to the oxygen of the atmospheric air, and to the carbonic acid, combines with them, and at once produces a perfect carbonate of lead, possessing all the essential properties of that article. I in general open each triturating vessel once in about twelve hours, to remove the carbonate of lead which has been formed within it. This may be done more or less frequently, according to circumstances.

When the carbonate of lead thus manufactured, is first obtained, it generally has a light tinge of blue, but this disappears in the process of drying, and it is not important, therefore, to adopt means to prevent it; I have found, however, that by introducing a very small portion of the vapour of vinegar in conjunction with the atmospheric air and carbonic acid, the white lead is at once obtained perfectly free from colour.

The carbonic acid may be generated by the combustion of coal, or by the decomposition of carbonate of lime, or of other carbonates.

Having thus fully shown the manner in which I conduct the process of manufacturing white lead, or carbonate of lead, and pointed out the difference in the process as adopted by me from those heretofore followed, what I claim therein as of my invention, and desire to secure by letters patent, is, simply, the introduction of carbonic acid and of atmospheric air into closed *vessels,* in which fragments of granulated lead is subjected to long continued attrition in water; the introduction of these gases being intended to supply the portion of oxygen and of carbonic acid necessary to convert the nascent suboxide of lead into white lead; by which means a perfect combination is effected, and the desired result attained, as herein set forth. SMITH GARDNER.

Specification of a patent for Manufacturing Carbonate and other Salts of Lead. Granted to HOMER HOLLAND, *Westfield, Massachusetts, November 3d,* 1838.

To all to whom these presents shall come: Be it known, that I, Homer Holland, of the town of Westfield, in the county of Hampden, and state of Massachusetts, have invented several new improvements in processes for compounding, making, and producing pulpy compounds from Metallic lead, and of converting said pulpy lead into sulphate and carbonate of lead for white pigments; and also for making of said pulpy lead into chromate of lead, known as chromic yellow; which special improvements in compounding have not heretofore been known or used: and that the following is a full discriminating, and exact description of said methods, sufficient in detail to distinguish the same from all other processes, and to enable any one skilled in chemistry to apply and use said improvements understandingly. The special improvements which I would describe and claim, consist, 1st. In using any alkaline salt, or substitute, in the moistening solution for the charge and chamber, or open headed cylinders, described and mentioned in my patent dated the 18th day of March, 1836, whose elements consist essentially of oxygen, carbon, and hydrogen, in any proportions, instead of alkaline carbonates, before recommended and employed, as they augment the electro-chemical action, increase the product, and modify and facilitate the combination of the elements with nascent pulpy lead, by their presence, or catalytically.

Acetates of lead, whether neutral or basic, also sugar, and even alcohol, may be advantageously used in the solution, to moisten charge, chamber, and pulp.

2d. In adjusting the pulpy plumbic compound, produced as described in my said patent, for acetate and nitrate of lead, or with the catalytic additions with neutral chromate of potash or soda, or by dissolving the alkaline chromates in water, and using this chromic solution as the moistening of charge and chamber.

The chronic pulp, after subsiding, may have most of all the alkali withdrawn by decantation, and the remainder neutralized by washing with water, made acid by sulphuric or other acid.

The commercial bichromates of potash and soda are to be made neutral by the addition of suitable proportions of their respective bases.

The economy of the above process, in making chromate of lead, is in substituting the plumbic compounds in their nascent state, for the expensive plumbic salts, acetate and nitrate, now usually employed in the manufacture of chromic yellow.

3d. In my said patent for oxidizing and producing lead pulp, although, in the incipient stage of the operation, the lead may be an under oxide, the subsequent exposure, in the open-headed chambers, to the continuous and conjoint action of the elements which constitutes the atmosphere, water, and catalytic additions, together with the friction, and the known and established propeıty, or capacity, which, all metals, in a minute state of division, have of absorbing, "dissolving," or combining with, all elements with which they are in contact, constrains me to disclaim the opinion, that plumbic pulp, under any circumstances, can be considered a definite compound, and much less an oxide; but that it is a compound of lead, into which the elements, hydrogen, carbon, and nitrogen, and their compounds, enter, as well as oxygen.

By the foregoing explication of the pulpy plumbic compound, the following rationale of the modifications of the pulp, in converting it into a perfect carbonate, or sulphate, will be apparent. After carbonating the pulp with certain catalytic additions, artificially, should there be any basic salt, it is to be removed by washing in an alkaline solution, boiling, particularly in making the sulphate of lead, the pulp must be boiled to modify the plumbic hydrate by more highly oxydizing the pulp.

The sulphate of lead is made directly from the pulpy lead, modified and oxydized by heat, while in its moistened state, by digesting it, in any quantity, with sulphuric acid of commerce, previously diluted with twice its measure of water, (more or less,) and suffering the acid thus diluted to become perfectly cold, previous to adding the pulpy lead.

It is necessary to boil the dilute sulphuric acid and pulp thoroughly together in a shallow leaden vessel, with rather an excess of acid, that the product may become a perfect sulphate; in this, great caution is requisite, otherwise the product will be, more or less, a mixture of sulphate, hypo-sulphate, or sulphanide of lead, and its colour changed by mixing and painting in oil. Besides, it will not be as dense, fine, and fusible.

All the pigments should be thoroughly washed in several waters, and may be dried by the well known methods.

The cylinders mentioned in said patent, I now make about four feet in length, and thirty inches in diameter, wholly of lead, either sheet or cast, about one-fifth of an inch in thickness. The ends are entirely open, except an inner rim to retain charge and moistening fluids, or solution, with forming pulp, and allow a free circulation of the atmosphere for its elements.

They are mounted on an axis, passing through their centres, and the centres are of iron, with arms which are attached to the rims of each end of the cylinders. The rotations may vary from six to nine times a minute, and are moved by a drum and belt, or other gearing. The pulpy lead may be withdrawn every six, eight, or twelve hours. The medium charge is fifty pounds, and the moistening fluid, or solution, from three pints to three quarts, or more.

I claim, 1st. The process and method of using the alkaline salt, carbonates, and other catalytic substitutes, as hereinbefore mentioned, in moistening charge, and chambers, described and mentioned in said patent, in producing pulpy plumbic compounds; and I do not intend to restrict their application and use to pulpy leads produced by revolving chambers alone, but to extend their application to the compounds of lead produced by other methods of friction, whether substituted, or adopted, to evade my chambers.

2d. I claim making chromate of lead, as above specified and described.

3d. I claim modifying the pulpy plumbic compounds above described for carbonate of lead, and particularly the processes described for making a definite sulphate of lead, by digesting, boiling, and washing, as above discriminated, and made plain and distinct.

HOMER HOLLAND.

Remarks by the Editor.—We have inserted the three foregoing specifications on the manufacturing of white lead, and of other compounds of lead, because the particular process upon which they are dependent, that of producing these compounds from lead comminuted by trituration, has, of late, excited much interest, and been a subject of frequent inquiry. The first of these specifications leads to the conclusion, that Mr. Holland supposed this process to be new in the year 1836, whilst the fact is that it was the subject of a patent obtained by Joseph Richards, of Philadelphia, in the year 1818. A manufactory was also established at Norristown, Pennsylvania, in which the triturating process was employed, and after essaying the thing for a considerable length of time, the plan was given up. The white lead produced was deficient in body, and its colour was said not to be good.

That Mr. Holland found the process of 1836 defective, is to be inferred from his patent of 1838, for improvements in it. We should be glad, however, to obtain his own account of this matter, as we might err greatly by detailing the information received from others.

Mr. Holland's second specification we think much more elaborate than clear; had language more simple been used, it would have rendered his meaning more obvious to the great body, even of those "skilled in the art." We have ventured to insert, and to change a few words, where we thought that it might be safely done, but further than this we have not gone.

On the 7th of June, 1838, Mr. William Cumberland, of New York, obtained a patent for a process of manufacturing a white pigment, the specification of which we published in vol. xxiii. p. 402. The patent obtained by Mr. Gardner is, it will be seen, for a particular variation of the process of oxidizing and carbonating the pulpy lead, and by which, he states, a very superior white lead is obtained; and his statement has been corroborated by others. We shall have something further to say on this subject.

Journal of the Franklyn Institute.

LVIII.—*On the course or path of the Electric Fluid.* By Henry Dircks, Esq.

Read before the Literary and Philosophical Society of Liverpool, Nov. 16.

Although we have two principal theories by either of which we may account for electrical phenomena, yet as is well known there is no theory that is universally adopted. We prefer that of Du Fay of two fluids, the resinous and vitreous, whereas in America the Franklinian theory of a single fluid continues to be received. It is certainly a curious and remarkable fact, that this important point which appears to be at the very head of our inquiry, in investigating the nature of this exceedingly subtle agent, should have so long withstood every effort that has been made to develope its operation; and that with our expended means of pursuing this interesting investigation, philosophers should still remain divided in opinion. We agree that it is the same agent which is at work in atmospheric, frictional, magnetic, voltaic, organic and thermo-electricity. The same data are taken up by the favourers of either theory to prove their several positions; that influence of *points* is alike advanced to prove the existence of one and of two fluids. Franklin and all electricians after him, speak of the star and the brush, the former negative, the latter positive; whereas Dr. Faraday contends that under favourable circumstances, and especially in some gases, the negative and positive points both offered the electric brush of light. We are all familiar with the experiments proposed as evidence of the existence of a single fluid, as the action of a flame between two balls, one positively, the other negatively electrified, by which the latter becomes very much heated; the stream of air produced when a point proceeds from a conductor; the manner of charging the Leyden phial; and especially that given by Mr. Lullin, when the discharge of a jar is made to perforate a varnished card, between two points on either side, but half an inch asunder, by which the point proceeding from the negative side invariably perforates the card, although a hole may have previously been made opposite the positive point, where a perforation does not otherwise occur; also the common discharge through a card placed against a charged jar, where a burr is produced on both sides, but more markedly if the card is set vertically between the points of the universal discharger, when the burr will be found larger on the negative side where the positive electricity may be supposed to make its exit, and smaller on the positive side, the outlet for the negative or resinous electricity. The appearance by perforating bodies, might at first seem conclusive that there are two fluids, but it has occurred to the writer, and may be worthy of notice here, in explanation of the double burr, though he has never met with any notice of a similar view of this subject, being taken by others, that, as the electric fluid is so rapidly excited by friction, pressure, and slighter causes; the electric discharge itself, by its amazing rapidity, may become the exciter of a quantity of the fluid previously latent, which brought into activity, a reaction may be thereby produced, and this whether there is one or two fluids. This seems to be both a reasonable and highly probable consequence.

We here have instances of the effects of the electric fluid, but can neither arrive at any conclusion respecting its nature, nor ascertain the direction of its course. As we might hope to arrive at something more conclusive by considering this latter point, which indeed is the main object of the present paper, we shall proceed to this more important inquiry.

One known means of tracing the passage of the electric discharge is that made when the points of the universal discharger are placed an inch apart on a card, having a broad line painted on it with vermillion, when the discharge leaves a well defined irregular black line. Observation in this way, however, is very limited. We wish to arrive, for instance, at something definite whether there is one or two fluids—and we wish to see in the path it takes whether it passes right over, meets half way, or passes side by side. In short, what are the peculiarities exhibited by the discharge of the Leyden jar?

Dr. Faraday, in his most excellent and alaborate "Researches," states that an ever present question on his mind has been, "Whether electricity has an actual and independent existence as a fluid or fluids, or was a mere power of matter, like that we conceive of the attraction of gravitation. If determined *either way*," he adds, "it would be an enormous advance in our knowledge." Not only every experiment which has for its object the elucidation of electrical phenomena, but likewise the opinions of electricians may truly be said to be of extreme value. It is well, therefore, that Dr. Faraday has put on record as well in what he succeeded as in what he failed. The ill success of one may suggest another course of experimental inquiry to some other worker in this prolific field of scientific research, and thus we may hope gradually to develop many important results in connexion with electrical science, from which, with good cause, we expect to reap many discoveries of great practical benefits.

It early appeared to me quite within the range of possibility to render this active fluid a tell-tale, as it were, of its own progress, especially in conducting the discharge of the Leyden battery. I felt convinced of this from what has already been noticed of the piercing of cards, the black line left on a vermillion coloured card, and also from the markings left on the uncoated glass by the spontaneous discharge of an overcharged jar. But my object was to obtain evidence on a larger scale, and of a more conspicuous character.

My first experiments were made with a piece of window glass four inches square, smeared on one side with a mixture of flowers of sulphur and white lead ground together with gum water, laid evenly on the glass and dried. When placed against the side of the Leyden jar, the charge may be passed over it by using the discharging rod, in which way a dark brownish line two or three inches long, having a circuitous course, is easily produced.

Not satisfied with this result I at length adopted a plan which successfully affords an interesting illustration of the path of the electric fluid through a considerable space, varying with the quantity of charged coated surface. From 18 inches to 2 feet is easily obtained with a gallon jar, or battery of equal capacity, provided the electrical machine is in good working order. The means of effecting this will appear very simple, though the conditions requisite for its success are not so obvious as might at first appear. Take a broad oblong plate of glass, place under it a sheet of white paper, then by striking a fine hair-sieve containing iron filings, let fall on the glass an equal distribution of the filings until they communicate a dark-grey shade over the paper. The glass so prepared is to be placed in the line of communication for making the discharge. When this is done with the white paper under the glass, the result is most conspicuous, beautiful, and interesting. The appearance that instantly follows is something like a map of a serpentine river, with often small branches issuing out in many streams at some of its principal windings and again running into the main branch. Throughout the tortuous course of this passage the iron filings are swept away to the breadth of one-eighth to a quarter of an inch and upwards by the rapid transit of the fluid, with as much neatness and precision as if carefully removed by some process requiring extreme care and delicacy of manipulation. Often a few grains form an irregular central line. If a short piece of crooked wire in the form of a ring, arch, or helix, be placed in or a little out of the direction of the fluid, it is made part of the circuit, and the filings are not disturbed if any arched form or immediate connection offers a more perfect conductor. On shaking the filings off the glass no trace appears to remain, until breathed upon, when a clear thread like line, having a slight dark colour, becomes distinctly observable.*

The success of this experiment seems to depend on a peculiar arrangement, and the best I have found, is to have the Leyden jar placed on the edge, and touching the filings at one end of the glass plate; a perpendicular rod of thick wire being at the other, from the top of which, a connexion may be made (by a discharging rod,) with the ball of a Leyden phial. A full charge is requisite to make a good marking of the path, and the filings should not be too thickly spread, otherwise the electricity passes over in flashes ; a communication, too, should be made between the outside of the jar, and some good conductor. The vertical pillar at the further end of the plate, has been formed to answer when long thin bent wires proved quite ineffectual.

It is only to be regretted that this beautiful experiment leaves the subject still open to enquiry ; but this may be one step, which, in other hands, may be made serviceable in obtaining greater results. I cannot pass over, in this place, mentioning a very easy means of tracing, and so registering, the several experiments made at each discharge. This is done by taking the glass, strewed with filings, and having a marking which is to be copied ; on each end or down each side place a thin lath, on this lay another, but of plain glass of equal size, over all place a slip of paper long enough for a tracing. Now, rest the glasses between two tables set apart, or between two flat bars of wood, resting on a table, and in such a situation, that a small lighted candle placed on the floor, will throw the shadow of the filings up through the glass on the back of the paper. There being no other light in the room, this is easily done. Or by giving a coating of thick glue to cartridge paper ; this, if carefully managed, would take up the filings off the glass, and show a reversed specimen of the electric path. In this way I have taken the filings and preserved the figure made by the magnet.

---

* A description of the apparatus will be seen at the end of the article.

Another experiment, too interesting to be omitted, was performed with a few sheets of strong printing paper, stitched like a pamphlet. In the first experiment made with this, it was laid on the table of the universal discharger, and the balls being removed, the blunt pointed wires were placed on the paper, an inch and a quarter asunder; the discharge of a very large jar, slit the paper, giving it the form of two small folding doors. With a mixture of equal parts of flowers of sulphur, and red lead, the face of the upper and three lower leaves were strewed over. The result on making the discharge was not always the same—thus

Ex. 1. In a passage of one and a quarter inch, the positive end was harmlessly passed over for more than one-third, leaving only a dark line on the top leaf; from hence to the negative end, the paper was ripped open, the cut being in shape like the letter H. On examining the lower or second leaf, the remaining two-thirds of the passage, that is, the horizontal line of the H., presented a broad black marking, which had struck also to the under side of the upper leaf. The third leaf was untouched.

Ex. 2. This was precisely the same as the foregoing, with the exception of being a shorter path, and more violently torn, so that the rent, formed a very oblong H., and the positive side was uninjured for near half way. The remaining half, which we call the negative side, showed a broad black band on the face of the second leaf.

Ex. 3. This passage was remarkable from the paper being pierced on the positive side, clear of the rent beyond it, which was of a very imperfect H form. The paper was unmarked and uninjured for a quarter of the path on the positive side, at the end of this the paper was pierced with a small hole. On the second leaf, a round black spot occurred, corresponding with this terminus of the positive side, and at the negative end where the rent begins, there was another black spot or star, both connected by a straight cut in the paper not discoloured, and branching off right and left at the negative end, in form like a T. The third leaf not marked.

Ex. 4. Here the passage from the positive was marked one-third with a faint line, at the end of which a small hole appears, and another hole at the commencement of the negative passage, without tearing the paper. On the second leaf these holes have corresponding black perforated spots, and on the third leaf there is a broad black mark, with a corresponding one on the upper side of the leaf above it. These black marks are all very like the representation of mountains in a map, and have a white band running through their centre.

Here, as in Mr. Lullin's experiment, there is a tendency on the negative side to enter the paper, although its surface is covered with a conducting substance. There is more violence, too, on this side, where indeed we have a disruptive discharge. These experiments are on many accounts exceedingly interesting. It would appear as if the positive or vitrious electricity had greater velocity than the negative, that the two electricities meet at this point, and uniting cause an explosion, followed in this instance by a chemical effect—the production of a sulphuret of lead, which marks *only* the remaining two-thirds of the path. This, if correct, would seem to offer some modification in the remarks Dr. Faraday makes on the current—he says, "It is a most important part of the character of the current, and essentially connected with its very nature, that it is always the same. The two forces are *every where* in it. There is *never* one current of force, or one fluid only. Any one part of the current may, as respects the presence of the two forces there, be considered as precisely the same with any part; and the numerous experiments which imply their possible separation, as well as the theoretical expressions which, being used daily, assume it, are, I think, in contradiction with facts." What he next adds is too remarkable in connexion with our experiments not to call for special notice. "It appears to me to be as impossible to assume a current of positive or a current of negative force alone, *or of the two at once with any predominance of the one over the other*, as it is to give an absolute charge to matter." (1627.) The establishment of this as a fact or its disproof he justly considers of the utmost importance.

We might almost be inclined to inquire in reference to the electrical experiment from the consideration of which we have digressed. Has the resinous electricity a tendency downwards, and the vitreous a tendency upwards? Or, has the latter greater velocity than the former? Or do these experiments at all prove "that the centres of the two forces (or electricities), or elements of force, can be separated to any sensible distance?"

November, 1840.

Fig. 2, plate vi., Shows the arrangement of apparatus for making a long path through iron filings. A, the Leyden jar; B, a glass pillar mounted with a wire supporting the metal bar C, D. A, D, B, a plate of glass strewed over with iron filings. C, E, F, the discharging rod by which to complete the connexion in making the discharge.

Fig. 3. A, B, D, the glass plate and filings displaying the path of the electric fluid. A, the negative end, B, the positive.

LIX.—*On Electro-Magnetic Forces.* By J. P. Joule, Esq.

67. I have in my last paper described a method of constructing the electro-magnet which is attended by great results. The few additional experiments which I have now the pleasure of submitting to the readers of the "Annals," are, I think, confirmatory of the principles before advanced.

68. A piece of *stub* iron was (as in the manufacture of gun bar-

rels) formed into a spiral and welded on a mandril into the shape of a thick tube, by which process the iron was rendered very compact and sound throughout. This, and another piece of iron which was intended for an armature, were planed, turned, and fitted with eye hole screws in the manner that I have previously described (39)*. In fig. 1, pl. vii. C. represents the electro-magnet, D. the armature, and A. B. a conductor of copper rod or wire passing along one side, returning by the axis, and then away by the other side, so as to go about the whole once only, and in a shape somewhat similar to that of the letter S. The length of the electro-magnetic cylinder is two feet; its external diameter is 1·42 in., and its internal, 0·5 in.; the weight of the iron of the magnet, with the screws, is 6 lbs. 11 oz.; that of the armature 3 lbs. 7 oz.; and the least sectional area of the magnetic circuit (49), 10¼ square inches. This electro-magnet, in order to distinguish it from the rest, I call No. 5.

69. A copper rod, ⅜ of an inch thick, was covered with a ribbon of cotton, and bent about the cylinder as I have just described. The electro-magnet and its armature were then secured, by means of cords passing through the eyeholes, to strong pieces of iron affixed to the *levers*, (43). A battery consisting of eight of the cast iron cells (66), each of which presented an effective surface of two square feet, was arranged as a single pair, and, in connection with the electro-magnet, induced a lifting power of about 1350 lbs.

70. Being aware that a bundle of thin wire is a much better conductor than a rod of the same weight and length, I removed the copper rod and substituted for it a bundle, consisting of 60 copper wires, each 1-25th of an inch thick. With this arrangement it was found that 16 cast iron batteries, in a series of 2, produced a lifting power of 1856 lbs., or 183 times the weight of iron employed in both the magnet and its armature.

71. Now, by dividing the power thus obtained by the least sectional area of the magnetic circuit upon which it is induced, we have a specific power of 181, which is only two-thirds of that which a comparison with other electro-magnets would lead us to expect.† This deficiency is, I think, mainly owing to the very small relative quantity of conducting metal about this (No. 5) electro-magnet, a deficiency which demands a proportionate increase of battery power, in order to produce the same effects. This, with the difficulty of making the weight bear evenly on every part of so long a cylinder, may, I think, satisfy us that, if every circumstance were strictly attended to, its maximum lifting power would obey the general rule.

72. Having suspected that the extreme power of the large electro-magnet, No. 1‡, had not been attained in my last experiments, on

---

* Annals for September, Vol. 5, p. 190.

† See Table 4, Annals, Vol. 5, p. 193.

‡ For a description of this electro-magnet, see (39), Vol. 5, p. 190. It has

account of the imperfect insulation of its coils (45), I was determined to try it again, and to use every precaution which was calculated to develope its magnetism to the full extent. The old wire was removed, and 21 copper wires 1-25th of an inch thick, and 23 feet long, were bound together by cotton tape. This was wrapped on the iron, which had been previously insulated by a piece of calico.

73. Sixteen cast-iron cells, of the same size as those previously described, were then arranged in a series of four, and connected by sufficiently good conductors to the electro-magnet. The power which was then necessary to break it from its armature was 2775lbs., or nearly a *ton and a quarter*. An immense weight, when it is considered that the whole apparatus, magnet, armature, and coils, weighs less than 26lbs.

74. Now by the formula $x = 280\,a'$ (51), we have $280 \times 10 = 2800$ for the greatest lifting power of this electro-magnet, or only 25lbs. more than that actually found, which cannot but be considered as a striking proof of the accuracy of the general principles I have before advanced (49). That the *saturation* of the iron was very nearly effected, appears from the fact that the quantity of electricity used above, was fourtimes as great as that which was competent to make the same electro-magnet carry 19 cwt.

75. Although the battery that I have used for obtaining *maximum* effects is very powerful, a very good lifting power may be attained by means of a very small voltaic arrangement. For instance, No. 1 can carry 8 cwt. when the current generated by a single pair of 4-inch plates of iron and amalgamated zinc, is passed through its coils; and with single plates of platinized silver and amalgamated zinc, exposing only two square inches of surface, the attraction is such as to require the utmost force I can exert, even to *slide* the armature.

Broom Hill, near Manchester, November, 23rd, 1840.

Errata in Mr. Joule's paper on Electro-Magnetic Forces.— Vol. 4, p. 478, table 4, for 16·6 read 1·66. Vol. 4, p. 481, line 25, for considerable read considerably. Vol. 5, p. 196, table vii., for 26 and 11 read 2·6 and 1·1.

---

been presented to the "Royal Victoria Gallery" of Manchester, where it still remains on exhibition.

BRITISH ASSOCIATION PROCEEDINGS
AT GLASGOW, 1840.

Dr. Playfair "On a New Fat Acid."—Dr. Playfair had examined some of the vegetable fats, for the purpose of ascertaining whether the margaric acid contained in them possessed a constant composition. He remarked that the acid in the butter of nutmegs was peculiar, and had not formerly been examined. Pelouze and Bondet have stated in the *Annales de Chimie*, that it is margaric acid. Dr. Playfair considered that the radicals of serecic and œnanthic acid were similar; in the former, however, one equivalent of hydrogen is replaced by one equivalent of oxygen. It is a beautiful white crystalline compound melting at 49° C., and is soluble both in alcohol and ether. The combination of the acid with oxide of glyceril, exists in the butter; it unites with metallic oxides and forms salts: these were described, but the results are not susceptible of analysis, as they were principally numerical. The formula of the acid is $C_{28} H_{54} O_3$.

Dr. Ettling "On the Indentity of Spiroilous and Saliculous Acid."—The oil discovered by M. Pagenstecher, and obtained by the distillation of the *spiræa ulmaria*, has already attracted considerable attention. Dr. Ettling had analyzed it previously to the appearance of M. Piria's valuable paper on Salicyl. The oil decomposes into two oils on keeping, one of which is specifically lighter, the other heavier than water. Dr. Ettling discovered that the latter possessed the same composition as hydrated benzoic acid. The action of ammonia on the oil gives rise to some new interesting compounds. In order to obtain these compounds it is indifferent whether saliculous or spiroilous acid be employed. The final product of the action of ammonia upon these, is the amide of salicyl (salicylamide). This body evidently belongs to the class of amides, for it does not evolve ammonia, on the addition either of potash or of acids. The cause of its formation is as follows: three atoms of saliculous acid unite with three atoms of ammonia, and form saliculite of ammonia, whilst three of hydrogen and oxygen combine together and form water. This salicylamide unites with copper, iron, and lead, forming compounds.

Professor Liebig "On Poisons, Contagions, and Miasms."—Dr. Playfair stated that he had prepared, at the request of the author, a statement of Professor Liebig's new views on the subject of poisons. Poisons might be divided into two classes, those belonging to the inorganic and organic kingdoms. Many substances were called inorganic poisons which had in reality no claim to be considered as such. Sulphuric, nitric, and muriatic acid, when brought into contact with the animal economy, merely destroyed the continuity of the organs, and might be compared, in their *modus operandi*, to the action of a heated iron, or a sharp knife. But there are others—and these are the true inorganic poisons—which entered into combination with the substance of the organs without effecting any visible lesion of them. Thus it is known, that when arsenious acid or corrosive sublimate is added to a solution of muscular fibre, cellular tissue, or fibrin, these enter into combination with them, and become insoluble; when they are introduced into the animal organism the same circumstance must happen. But the bodies formed by the union of such poisons with animal substances are incapable of putrefaction; they are incapable, therefore, of effecting and suffering changes; in other words, organic life is destroyed. The high atomic weight of animal substances explains the cause of such small quantities being requisite for producing deadly effects. After stating several chemical details on this subject, it was shown that to unite with 100 grains of fibrin, as it exists in the human body, (in which it is combined with 30,000 parts of water) only 3¾ grains of arsenious acid are necessary, or 5 grains of corrosive sublimate. The second class of poisons were those belonging to the organic kingdom. For some such substances as brucia and strychnia, no data exist by which it can be determined to what cause their action may be assigned. But the morbid poisons, such as putrid animal and contagious matter, appear to owe their action to a peculiar agent, which exerts a much more general and powerful action than chemists are aware of. Thus, when oxide of silver is thrown into peroxide of hydrogen, the oxide is reduced, and metallic silver remains. Here there can be no affinity, for oxygen can have no affinity for oxygen. It is merely that a body in a state of motion or decomposition is capable of inducing upon or imparting its own state of motion or decomposition to any body with which it may be in contact. There is a disease frequently produced in Germany by using decayed sausages as an article of food. The symptoms attending the disease are remarkable, and distinctly indicate its cause. The patient afflicted with the disease becomes much emaciated, dries to a complete mummy, and finally dies. The muscular fibre and all parts similarly composed disappear. The cause of this evidently is, that the state of decomposition, in which the component parts of the sausages are, is communicated to the constituents of the blood, and this state not being subdued by the vital principle, the disease proceeds until death ensues. It is remarkable that the carcases of the individuals, who have died in consequence of it, are not subject to putrefaction. The cause of the action of contagious matter is similar. It is merely a gaseous matter in the state of transformation, and capable of imparting the state of transposition, in which its atoms are, to the elements of the blood. It is capable of being reproduced in the blood just as yeast causes its own reproduction in fermenting *wort*. The causes of the action of yeast and of contagion were shown to be the same, and examples were produced in which similar reproductions take place in common chemical processes. There are two kinds of yeast used in the brewing of Bavarian beer.

The fermentation caused by one is tumultuous; that produced by the other is tranquil. They, therefore, induce the peculiar state of transposition in which their atoms are upon the elements of the sugar. The same was shown to be the case with the vaccine virus of cow and human small-pox, of which, one produces a violent action upon the constituents of the blood, whilst the other causes a gentle action quite distinct from the former.

Professor Hannay said he could not exactly coincide with the views proposed regarding the action of inorganic poisons, as he was convinced the cause of their virulence was owing to something further than mere combination with the animal membranes; nor could he coincide in the comparison brought forward by Dr. Playfair, that sulphuric and oxalic acids merely acted like a heated iron, by destroying the continuity of particular organs. He thought that through the course of the inquiry chemistry had been too much kept in view, and that medicine had not been sufficiently consulted. It was singular to see us brought back to the time of Hippocrates, who also had affirmed that contagious matter was a kind of yeast acting in the blood. Dr. Playfair explained that Professor Liebig expressly states in his report, that this subject cannot be completed without the co-operation of physiologists; that he had therefore merely brought forward the purely chemical part of the inquiry, and hoped thereby to draw the attention of physiologists to its further investigation. Hippocrates had certainly compared the action of yeast with that of contagious matter, and the comparison was so apt that it could scarcely be avoided; but the merit of Liebig's views is, that he has explained the action of yeast, and shown that it is owing to a peculiar agent which has hitherto escaped attention, but which plays a very important part in the phenomena of combination and decomposition.

PROF. FORBES IN THE CHAIR.

On the Decomposition of Glass, By Sir David Brewster.

There is no subject more curious or more instructive than the disintegration of crystallized and uncrystallized bodies, either by the direct influence of chemical agents, or the slow process of natural decomposition. At the Edinburgh Meeting of the Association, I submitted (said Sir David) to this Section a brief account (which has been since published in an enlarged form in the Edinburgh Transactions) of remarkable optical phenomena produced by the instantaneous action of water and other fluids on crystals, and on their subsequent decomposition when placed in their saturated solutions. Since that time I have had occasion to examine the phenomena of decomposed glass, both of that which is found in Italy, of which I have received the finest specimens from Mrs. Buckland and the Marquis of Northampton, and of other specimens recently found in making excavations among the ruins of the Chapter-house of the Cathedral of St. Andrew's. In decomposed glass, the decomposition commences in points, and extends itself either in planes so as to form thin films, or in concentric coats so as to form concentric films. When the centres of decomposition are near each other, the concentric films or strata which they form interfere with each other, or rather unite, and the effect of this is, that the glass is decomposed in film of considerable irregularity, their surfaces having a finely mammillated appearance, convex on one side and concave on the other. The films thus formed are of extreme beauty, and afford, by transmitted light, colours of infinite beauty and variety, surpassing anything produced in works of art. They have the effect of dissecting, as it were, the compound surface of the solar spectrum, or of sifting and separating the superimposed colours, in a manner analogous to what is produced by coloured and absorbing media. I have succeeded, indeed, in producing one or more bands of white light incapable of decomposition by the prism; and there can be no doubt that they will be found to exercise a similar or an analogous action on the leading rays on the thermometric spectrum. In the decomposed glass from St. Andrews, a change of a very different kind is effected. In some cases the siliceous and metallic elements of the glass are separated in a very singular manner, the particles of silex having released themselves from the state of constraint produced by fusion and subsequent cooling, and arranged themselves circularly round the centre of decomposition; while the metallic particles, which are opaque, have done the same thing in circles alternating with the circles of the siliceous particles. This restoration of the silex to its crystalline state, is proved by its giving the colours of polarized light, and possessing an axis of double refraction.—The notice was illustrated by diagrams and specimens of the different kinds of glass referred to.

Prof. Forbes observed that few persons can form any correct conception of the total amount of the value of glass used in the various optical instruments, on the correct action of which so much depended. Whether the decomposition which Sir David Brewster had now brought under notice, arose from the action of the atmosphere, or from intermolecular action, as Sir David Brewster seemed to think, or from some original defect in making or annealing the glass, it was of the utmost consequence. Dr. Traill had given him a specimen of a piece of plate glass manufactured near Liverpool, which, when polished, proved to be filled with fissures and flaws in the interior; and he informed him that the manufacturers had no means of ascertaining the defect, which frequently occurred, until they had gone to the expence of polishing the plates.—Sir David Brewster said, that the value of the glass employed in philosophical instruments was indeed incalculable, and that the most valuable glass articles manufactured by Frauenhofer,* of Munich, seemed to be peculiarly liable to some superficial decomposition of this kind. A prism of this glass in the Observatory of Paris had become absolutely black. A prism belonging to himself had become quite blue on the surface, although as yet its action on light was not effected. The large object glass of the principal telescope in the Observatory of Edinburgh had began to show decided symptoms of superficial decomposition; and many other instances also could be mentioned. He considered it of the utmost importance that a remedy should be discovered and applied.—Prof. Forbes mentioned some instances of this kind of decomposition taking place in telescopes on the continent, which showed that it was nothing peculiar to our climate.—Sir David Brewster did not think it arose from atmospheric action at all, but from some mutual action of the particles of glass themselves.—Prof. Forbes, Then why is it confined to the surface? why does it not pervade the mass of the glass?—Sir D. Brewster, Because at the surface the particles have more freedom than within; and if the new compounds are larger than the glass itself, then they have power to expand.

---

* M. Lamont, the Professor of Astronomy at Munich, who is in the constant habit of using Frauenhofer's glasses, was not present at this conversation; but he afterwards informed Sir David Brewster that there was an easy and effectual remedy for this tendency of Frauenhofer's glass to deteriorate on the surface, which was, to rub it frequently with the finer parts of whiting prepared by elaborating a mass of whiting in water, the fine powder to be dried and used on old soft linen.

On the Rings of Polarized Light produced in specimens of Decomposed Glass. By Sir David Brewster.

In the course of a series of experiments "On the Connexion between the Absorption of Light and the Colours" of thin plates, published in the Phil. Trans. 1837, I accidentally observed under the polarizing microscope certain phenomena of polarized tints of great beauty and singularity. These tints were sometimes linear and sometimes circular, and in some specimens they formed beautiful circular rings traversed by a black cross, resembling the phenomena of mineral crystals, or those produced by rapidly cooled circular plates or cylinders of glass. Having found in the decomposed glass from St. Andrews that the siliceous particles had resumed their position as regular crystals, and arranged themselves circularly round the centre of decomposition, I was led to suppose that this was the cause of the phenomenon, and that the rings were the effect of the double refraction of the minute crystals. A few experiments, however, overturned this hypothesis, and I was soon satisfied, by a little further investigation, that the phenomena arose wholly from the polarization of the transmitted light by *refraction*, the splendid colours being entirely those of thin plates, which were sometimes arranged so as to have the appearance of concentric rings. The structure by which these effects were produced, was compared by the author to a heap of very deep watch-glasses laid one above another. When the thin films were arranged longitudinally, and were inclined to the general surface of the plate, so as to transmit the rays obliquely, the light was still polarized, but only in one plane—namely, a plane perpendicular to the plane of incidence. When a drop of *water* or *oil* was introduced between the films, the phenomena of *polarization* as well as of colour instantly disappeared. (This paper was illustrated by coloured drawings.)

On the action of Nitric Acid on the Chlorates, Iodates, and Bromates of Potassa and Soda. By Prof. F. PENNY.

The present communication contains the details and results of some experiments undertaken with the view of obtaining additional confirmation of the correctness of the author's researches on equivalent numbers. In this he has been disappointed, as the action is attended by circumstances which render it inapplicable to so delicate a purpose as the determination of equivalent numbers. The results, however, that he has obtained are new, and he considered them of sufficient interest to be worthy the attention of the Section. In order to examine the action of nitric acid upon chlorate of potassa, a known weight of the salt was mixed in a retort with a measured quantity of the acid, and the mixture heated on a sand bath; as soon as it became warm, chlorine and oxygen were evolved in a state of mixture and not of combination, and the chlorate slowly disappeared. The solution was then evaporated to dryness, and the saline residue was found to be a mixture of hyperchlorate and nitrate of potassa, in the proportion of three equivolents of the latter to one of the former. The author expresses the reaction that takes place as follows:—

$$4(K+Cl+O_6) \text{ and } 3(N+O_5)=(K+Cl+O_8) \text{ and } 3(K+N+O_6) \text{ and } Cl_3 \text{ and } O_{13}.$$

The action of nitric acid on chlorate of potassa differs, then, from the action of sulphuric acid on the same salt. With nitric acid the salt is decomposed tranquilly, and the chlorine and oxygen liberated uncombined, whereas with sulphuric acid these gases are evolved in a state of combination, forming that dangerously explosive compound, chlorous acid. Nitric acid is therefore to be preferred for the preparation of hyper-chlorate of potassa, as with it the operation may be conducted without those violent detonations that are so apt to occur with sulphuric acid. The action of nitric acid on chlorate of soda is the same as upon chlorate of potassa. The chlorine and oxygen set free are in a state of mixture, and every four atoms of chlorate yield three of nitrate and one of hyper-chlorate. The hyper-chlorate of soda is a very soluble salt, and crystallizes in small rhombs. It is readily decomposed by heat, but is unacted upon by hydrochloric acid. It deliquesces by exposure to the air. The action of nitric acid on an iodate is very different from that on a chlorate, and is well illustrated in the case of iodate of potassa. When iodate of potassa is boiled for some time with a large excess of nitric acid, it is decomposed into potassa and iodic acid, the potassa combines with its proportionate quantity of nitric acid, forming the nitrate, and the iodic acid is deposited from the solution in minute, hard, and transparent crystals. If the acid solution of nitre, containing the iodic acid, be then evaporated, a reaction takes place; the iodic acid decomposes half of the nitre, sets free its nitric acid, and combines with the potassa, forming the biniodate. This change is completed when the mixture is dry, and if the hea be then withdrawn a definite mixture of biniodate and nitrate is obtained. If the heat be continued, a still further change occurs, the iodic acid expels the whole of the nitric acid, which is evolved as nitrous acid, and oxygen and neutral iodate of potassa remain. By adding a fresh portion of nitric acid to this iodate, the same changes may be produced by a proper regulation of the temperature. By acting upon iodate of soda with nitric acid, Prof. Penny has obtained a biniodate of soda, and by adding a considerable excess of of iodic acid to a solution of iodate of soda he has found a teriodate of soda. Both of these salts are anhydrous. The biniodate of potassa contains one atom of water. He also finds that crystals of iodate of soda contain different quantities of water, according to the strength of the solution from which they have deposited. From a hot and strong solution of this salt crystallzies in acicular tufts, and these crystals contain two atoms of water. If the solution be rather weak, long four-sided prisms are obtained, and these contain six atoms of water. If a solution of iodate of soda be evaporated spontaneously, large irregular prisms deposit, and these contain ten atoms of water. They effloresce rapidly by exposure to the air, and lose in this way eight atoms of water. The action of nitric acid upon bromate of potassa was next examined, and was found to differ remarkably from the actions of this acid on the chlorate and iodate. Neither hyper-bromate nor bibromate is produced, but merely nitrate of potassa. The nitric acid sets free the whole of the bromic acid, and this, at the moment of its liberation is resolved into its elements, bromine and oxygen. In conclusion, the author remarks that the action of nitric acid on these three classes of salts affords a ready method of distinguishing them from one another.

On the tests for Sulphuric Acid when thrown on the Person. By R. D. Thomson, M. D.

The object of the author was to discuss the accuracy of the modes of testing sulphuric acid when employed for criminal purposes, and especially when thrown on the person. A case had lately occurred to him in practice, and which was brought before the last sessions of the Central Criminal Court, which proved that the mode of determining the presence of *free* acid by *mere* testing was by no means satisfactory. A woman in a fit of rage threw a quantity of oil of vitriol at the face of a cab-master in the neighbourhood of Euston Square, and before the unfortunate sufferer could wash off the acid two minutes had expired; the consequence was, loss of vision in the eye. The author stated, that having attentively considered this case, and made a series of experiments on the eyes of dead animals, he had discovered that this kind of blindness was perfectly curable, and he had accordingly proposed an operation for this purpose in a paper read at the Medical Section. But besides having his face injured, the hat of the man was discoloured also by the acid. This article of dress was sent to the author, to determine the nature of the agent in this work of destruction. The result of this experiment was, that the injured hat, as well as an uninjured one, contained sulphuric acid, as tested by nitrate of barytes, and a solution of the soluble matter of both states of this article of dress afforded an acid reaction. It was therefore necessary to adopt some method which would afford a discriminatory test between the free and combined acid; the usual mode, viz. by boiling with carbonate of lead, and concluding, if any insoluble sulphate of lead was formed, that the acid existed in a free state, was found to be totally fallacious, because carbonate of lead decomposes sulphate of soda, contrary to the opinion stated in works of medical jurisprudence. Besides, it was shown that many of the usually so called neutral sulphates exhibit, in reality, an acid reaction upon test-paper, as in the instances generally of sulphates of potash, iron, soda, barytes, and also in the cases of alum, &c.; and hence the excess of acid attached to these salts would be apt to act as free acid upon the barytes test. The author, therefore, concludes, that the only demonstrative proof which chemistry affords is a quantitative analysis. Thus he found the entire hat to contain ·356 per cent. of sulphuric acid, probably in the state of alum or copperas, and the injured hat 1.379 per cent.; or, in other words, the hat had received by the injury 1.023 per cent. of free sulphuric acid. Here there was afforded clear evidence of the nature of the agent employed to effect the injurious object, which could not have been conclusive if the matter examined had only amounted to a drop or stain. The author directed attention to a point connected with sulphuric acid in a medico-legal point of view, viz. that the oil of vitriol of commerce always contains, in this country, nitric acid, in addition to various other impurities. Barruel has stated, that sulphuric acid is capable of dissolving platinum. The author has not been able to satisfy himself that it dissolves any sensible quantity of gold-leaf. Barruel attributes the property, which he states it to possess, of dissolving platinum, to the sulphuric acid assuming the function of muriatic acid. But the author is not aware of any experiment which would authorize this conclusion. He is rather inclined to attribute the action, if such an occurrence takes place, to the muriatic acid which is present in all the oil of vitriol prepared from sulphur that he has examined. It is given out in sensible quantities when a solid oil, such as cocoa nut oil, is acted on by sulphuric acid. This he ascertained several years ago, when examining some Indian oils, and Dr. Kane has since corroborated the fact of the existence of muriatic acid in oil of vitriol, although the author has not been able to observe the solution of any sensible quantity of gold-leaf by the action of oil of vitriol *per se;* yet if a few drops of muriatic acid be added, the action becomes very powerful, and by the application of heat platinum also is dissolved. These facts, therefore, prove that whenever we have oil of vitriol we may expect also nitric acid. The author added, that he knew of no certain mode of detecting the presence of nitric acid save by the property which it possessed of dissolving gold and platinum by the addition of muriatic acid. Pure morphia has no action upon nitric acid. It is the resin which generally accompanies that alkaloid which produces the characteristic yellow colour. But the author found that preparations of opium in which the resin was excluded, afforded no colour when nitric acid was added. From an examination of numerous cases of poisoning by opium which had appeared before the Middlesex coroners, he had come to the conclusion that the resin-of-opium test for nitric acid, afforded only an auxiliary method of arriving at the truth, as its characters were frequently usurped by other organic substances.

On the Resin of Sarcocolla. By Professor JOHNSTON.

The resin of carcocolla of commerce is separated by water into three portions: 1. A gum (A) which does not dissolve in water or alcohol, but which is in a great measure washed out by means of the former solvent. 2. A portion (B) insoluble in water, but soluble in alcohol, which is of a resinous aspect, and is represented by $C_{40}H_{32}O_{14}$. The hydrate, is $C_{40}H_{32}O_{14}+3HO$ when dried at 60°. This portion B, is separated (decomposed?) by bases into two or more organic compounds, the alcoholic solution giving with neutral acetate of lead a salt containing an organic constituent represented by $C_{40}H_{25}O_{16}$. Ammonia throws down from the mixed solutions a second salt of lead, the constitution of the organic constituent in which has not yet been determined. 3. The portion taken up by water from the crude sarcocolla, when evaporated to dryness, is separated by alcohol or ether into a soluble (C) and an insoluble portion (D). 4. The soluble portion C dried at 212, gave discordant results approaching to $C_{40}H_{32}O_{15}$, but when treated with bases gave salts containing organic constituents of a different constitution. A neutral acetate of lead throws down a salt represented by $P6O+C_{40}H_{28}O_{15}$, and the subsequent addition of the neutral *tris*-acetate a salt represented by $2P6O+C_{40}H_{32}O_{16}$. 5. The portion D, insoluble in alcohol, but soluble in water, consists of a gum and of a substance which is precipitated by neutral acetate of lead in curdy flocks. The investigation is still in progress, and the results are to be considered as open to correction.

On Resins. By Professor JOHNSON.

In this paper the author drew attention to the following facts, apparently established by a table of analytical results, which he exhibited, and had printed:—1. That the resins differ from each other in the quantity of oxygen they contain. 2. That those in which the atoms of oxygen is the same, the hydrogen may vary, and that this is another cause of difference in the properties of the resins. 3. That in all the resins hitherto carefully analyzed, the number of atoms of carbon is constant. 4. That the resins, as a natural family, may be represented by a general formula containing two variables. 5. That the known resins divide themselves into two groups, possessing unlike chemical and physical properties. That of one of these groups, colophony, may be considered as the type, and that it is represented by $C_{40}\,H_{32} + x\,O\,y$; that gamboge, or dragon's blood, may be considered as the type of the other group, which is represented by $C_{40}\,H_{24} + x\,O\,y$.

On a New Salt obtained from Iodine and Caustic Soda. By Prof. FRED. PENNY.

While examining the action of iodine on carbonate of soda, a salt was obtained, which crystallized in regular six-sided prisms, and which gave by analysis sodium, iodine, and oxygen, in proportions not corresponding to any known compound of these elements. The same salt was also prepared by saturating a solution of caustic soda with iodine, and allowing the solution to evaporate spontaneously. At first, this salt was thought to be the same as that described by Mitscherlich in his elements of Chemistry, and to which he gives the following composition $NaI + NaO, IO_5 + HO_{20}$; but the analysis geve very different results. Professor Penny gives the following characters of this salt:—It is white and inodorous, has a sharp, saline taste, crytallizes in short six-sided prisms, is soluble in cold and hot water, and is decomposed by alcohol into iodate of soda and iodide of sodium. It effloresces by exposure to the air, and is very readily decomposed by heat; water in abundance is first evolved, and then oxygen with a trace of iodine. Its solution is perfectly neutral to test papers, gives a pale lemon-yellow precipitate with acitate of lead, yellowish white with nitrate of silver, and a fine bright yellow with pernitrate of mercury. It is not affected by solution of starch, but instantly decomposed with the precipitation of iodine by nitric, sulphuric, acetic, and hydrochloric acids. The latter acid in excess converts it wholly into chloride of potassium. He detailed a remarkable circumstance attending the formation of this salt from iodine and caustic soda. When the solution is evaporated spontaneously, long prismatic crystals of iodate of soda deposit; but as the evaporation continues, these crystals are re-dissolved, and are replaced by those of the new salt. In one experiment this change was very striking. The solution on Saturday night had deposited an abundance of fine crystals of iodate of soda, but on Monday all these had disappeared, and a crop of the new salt had crystallized. The prior deposition of iodate of soda generally occurs in the preparation of this salt; and from other experiments of the author, it seems necessary that there should be excess of iodide of sodium present in the solution, and that the solution should be strong, in order that the salt may form. When this salt is dissolved in water, and the solution evaporated spontaneously, crystals of iodate of soda deposit, but very few of the new salt will form. The salt may also be procured by pouring a saturated solution of iodide of sodium on crystals of iodate of soda, and setting them aside for some days. The crystals will be dissolved and be replaced by crystals of the new salt. Prof. Penny then gave the details of his analysis of this salt, and the following formula, as agreeing best with his results:—$Na_5\,I_5\,O_{12} + 38\,H\,O$; or regarding it as a compound of iodate and iodide, it may be thus represented:—$3\,Na\,I + 2\,Na\,O\,I\,O_5 + 38\,H\,O$. According to this view, it is the sesqui-iodide of iodate of soda.

On the Mode of detecting Minute Portions of Arsenic.

By Dr. Clark, of Aberdeen.

This mode had been applied by the author to the detection of arsenic in commercial specimens of the metals tin and zinc. Grain tin, made in Cornwall, contains arsenic, which seems to be the occasion of the peculiar smell of the hydrogen evolved from that metal by the action of acids. All the specimens of commercial zinc that the author had happened to try were found to contain arsenic. Pure muriatic acid, diluted with distilled acid, is poured upon the metal, and the hydrogen evolved is passed first through a solution of nitrate of lead, and next through a solution of nitrate of silver. Nitrate of lead seems not acted upon by arseniuretted hydrogen,—at least, when in very small proportion; but were any sulphur present in the metal, sulphuretted hydrogen would be evolved in consequence, and the solution of nitrate of lead would be blackened, which, however, the author did not observe ever to occur. But nitrate of silver seems immediately to be acted upon by most minute portions of arseniuretted hydrogen. A bluish black precipitate is formed, which, to judge from a qualitative analysis, appears to be an arseniuret of silver. This bluish black precipitate may be collected with remarkable facility, from its falling readily from the solution, which it leaves perfectly clear. Heated in a small tube, so that the matter heated comes into contact with the air, the bluish black precipitate evolves arsenious acid, which, by the liquid tests, may be further satisfactorily recognized. Antimony produces a similar precipitate, so that the mere appearance of the precipitate is not enough, without the production and recognition, by the usual methods, of the arsenious acid. By a few evident modifications, this method may be applied to medico-legal investigations.

Dr. R. D. Thomson had found that the electrical method of Mr. E. Davy was inapplicable, in consequence of the deposition of a black matter from the zinc, which he had considered to be bitumen. Dr. Clark has, however, proved it to be arsenic.

On a New Mode of estimating Nitrogen in Organic Analysis. By Professor BUNSEN.

The qualitative methods at present employed for the analysis of azotized bodies were shown to be defective, for it is impossible to employ these processes when the nitrogen and the carbon are in small proportion to each other. Prof. Bunsen's process consists in introducing the substance to be analyzed, after having mixed it with oxide of copper, into a glass tube. A few slips of metallic copper are then added, and the tube is fixed to Dobereiner's apparatus for producing hydrogen. This gas is conducted through it until all the atmospheric air is expelled, giving the tube a rotatory motion at the same time, in order to dislodge any air which might be retained between the particles of the oxide of copper. The tube is now hermetically sealed, and introduced into an iron vessel filled with gypsum. The gypsum must be still moist when the tube is introduced, in order that it may be firmly wedged. Thus prepared, it is introduced into the common oven used for organic analysis, and surrounded with red-hot coals. If the tube be of strong green glass it never bursts. When the combustion is completed, the tube is placed below a graduated glass receiver standing over mercury, and the point cut off. The gas which had a pressure of several atmospheres now rushes into the jar. The carbonic acid is absorbed by a ball of hydrated potash, which is introduced into it, and the remaining gas must be nitrogen, for all the hydrogen must have been converted into water by the oxygen of the oxide of copper. The results obtained by this method agree with theory to the second and often to the third decimal place.

MISCELLANEOUS ARTICLES.

On the Cultivation and Growth of Electrotypes.

Without entering into the merits or demerits of the various modes of proceeding, which have been placed before the public, in the process of forming electrotype, I cannot help thinking that there are some theoretic points of very great importance, which remain to the present moment, unexplained; indeed, nearly untouched. But as all the processes of art are based on unvarying theoretical principles, and must consequently be prosecuted with much greater facility, and with better success, when guided by theoretical laws, than under other circumstances; a brief view of the theoretical principles in the process of forming electrotypes may possibly be interesting to many readers of the "Annals."

Whenever an electric current from a voltaic battery is made to traverse an aqueous solution of a metallic salt, sulphate of copper, for instance, a decomposition of the solution is accomplished, and the liberated particles of the metal assemble at the negative pole. And the oxygen and acid matter assemble at the positive pole; and the terminal negative plate in the solution, has its surface, next to the positive plate, soon covered with a coating of copper. If instead of having two plates only in the solution, there were several, perfectly unconnected with each other, as is shown in fig. 4, plate vi, every plate would become electro-polar, having a positive and a negative surface, as indicated by the letters p n, p n, &c. The positive side of each plate would become oxidized, and the negative side would receive copper from the liquid: and the deposition of copper on the negative side of each plate would form a new compact plate of copper. And if any engraving were on the negative side of any of these plates, or on all of them, the new plates, (the electrotypes) would be complete pictures in relief, of the original engravings. When a single pair of metals is used, and an engraved copper plate is one of them, and a piece of zinc the other, the deposition of copper, from the solution in which they are placed, will be on the engraved copper plate. It was in this way that the electrotype was formed, from which the print accompanying this number was taken.

A wire was soldered to the back side of the engraved plate, and another wire to a similar piece of zinc. The former, with its face upwards, was placed in a solution of sulphate of copper, and the latter in water in a porous paper tray above it. The two wires were tied together by a thin copper wire, which formed the voltaic circuit. The liquids were changed every 24 hours. In five days the first crop was removed from the engraved plate. This first crop was then furnished with a wire and made to assume one side of a new voltaic pair, with a new zinc for the other metal. And by placing this new voltaic pair in similar liquids, and in the same manner, as in the first process; a *second crop* of electrotype was formed on the face of the first one. This second crop, of course, is a fac simile of the original engraved plate; and in six days became 4 ounces heavier than it. We have other plates growing at the Victoria Gallery, from which prints will be taken, and presented to the readers of the "Annals."

Description of the Dial Plate of Professor Wheatstone's Electro-Magnetic Telegraph.

Figure 6 of plate vii., is a front view of the dial plate of the telegraph, on which are placed twenty-five letters of the alphabet, and five indexes. The indexes are placed in a horizontal line on the letters L, M, N, O, P, one on each: and by means of magnetic needles placed on the same axis, behind the dial plate, and those needles placed within spiral conductors in the usual way, the indices can be deflected either towards the right or the left, according to the direction of the electric current which traverses the conductor. When only one needle is deflected, it indicates the letter on which it is placed. All the other letters are indicated by being pointed at by two needles. The letter F, is pointed at by two needles in the figure, and is consequently the letter indicated by the telegraph. Other letters on the dial plate are pointed in a similar manner.

On the remarkable diffusion of Coralline Animalcules from the use of Chalk in the arts of life, as observed by Ehrenberg.—An examination of the finest powdered sorts of chalk which are used in trade, has afforded Prof. Ehrenberg the following result: that even in this finest condition, not merely the inorganic part of the chalk is become separated, but that it remains mixed with a great number of well-preserved forms of the minute shells of coral animalcules. As powdered chalk is used for paper hangings, Prof. Ehrenberg also examined these, as well as the walls of his chamber which were simply washed with lime, and even a kind of glazed vellum paper called visiting cards, and obtained the very visible result—demonstrating the minuteness of division of independent organic life; that those walls and paper-hangings, and so, doubtless, all similar walls of rooms, houses, and churches, and even glazed visiting cards prepared in the above-mentioned manner, (of which cards, however, many are made with pure white lead without any addition of chalk,) present, when magnified three hundred diameters, and penetrated with Canada balsam, a delicate mosaic of elegant coralline animalcules, invisible to the naked eye, but, if sufficiently magnified, more beautiful than any painting that covers them.—*Annals of Natural History, p. 286, No. 24, for December, 1839.*

Auroral belt of May 29, 1840.—About 9h. 20m. P. M. of Friday, May 29, 1840, a luminous belt, spanning the heavens from east to west, was seen by several persons in this city. When fully formed, about 9h. 22m., its width was from 3° to 5°, being widest and most luminous on the western portion; its altitude, at the highest part, about 85° above the southern horizon. Its light was similar and equal to that of ordinary auroral streamers. The extremities of the belt were 10° or 20° above the horizon, but their position was not particularly noted, and may have varied 10° or more from the E. and W. points. The northern edge of the belt was well defined; the southern was not very distinct. The belt slowly drifted southward, at the rate of about a degree per minute. At 9h. 30m., at which time the belt was brightest and most perfect, its northern edge was projected on Arcturus. Just before the belt reached this star, there was a slight bending, concave to the north, in that part of the belt which lay not f[illegible]ast of the meridian. This occurred near that region of the heavens in which (at this town) an auroral corona is manifested. The belt soon began to fade, and by 9h. 45m. was nearly extinct, but for ten minutes longer, a small remnant of it was visible in the southwest, which, just before it disappeared, passed to the south of Regulus. The summit of the belt was, at vanis[illegible]g, about 10° south of Arcturus. This belt was apparently constituted in part of beams obliquely transverse to its length, but this character was on this occasion less conspicuous than has commonly been noticed in other cases. During the whole time the sky was obscured by haziness and partially by clouds. There was some auroral light about the northern horizon, but it had no visible connection with the belt. Soon after 10h. this light increased exceedingly; numerous streamers rose to the altitude of 50°, and auroral waves flashed up nearly or quite to the coronal point.

This auroral belt was seen at New York city, and doubtless at many other places. If observations upon the position of the edge of the belt at given times were made at any considerable distance north or south of New Haven, we might have the means of finding approximately its height above the earth. If any such observations were made, it is to be hoped that they will be given to the public.

E. C. HERRICK.

New Haven, Connecticut.

Lectures on Electricity, Magnetism, &c.

LECTURE II.

Having, in the first lecture, given specimens of the electric, the magnetic, and the calorific classes of phenomena, I will now proceed to offer to your notice a few other preliminaries which will be necessary to be understood before we can enter very far into the study of electricity. In the first place, then, I must present to your notice a very well established fact respecting a property of atmospheric air, which is applicable to all the gases, and also to the electric matter. When the air within the receiver of an air-pump has become attenuated by the action of the pump, it still occupies the whole capacity of the receiver, and does not settle, as water would do, into the lower part of the receiver, so as to occupy that part only. Let us, for example, suppose that the receiver originally contained a quantity of air which we will call 100. If, now, by the action of the pump, 50 parts were to be withdrawn, the receiver would retain the other 50 parts only, or just one-half of the original quantity. But these 50 remaining parts of the air would still occupy the whole capacity of the receiver. Suppose, now, that the pump is again set to work, and that it withdraws from the receiver just one-half of the 50 parts that were left by the first operation; it is easy to understand that since the half of the 50 parts has left the receiver, there can be only 25 parts remaining. But these 25 parts, which are only a quarter of the original quantity, do not subsist in their original dimensions, and so occupy one quarter only of the receiver; nor do they subsist in one-half of the capacity of the receiver, in their dimensions previous to the last operation of the pump; but absolutely fill the whole capacity of the receiver as decidedly as the 100 original parts filled it. And in the same manner the whole capacity of the receiver would be occupied by any remaining portion of air, even after that portion had become too small for the pump to affect it any longer. Now in all these cases, it is obvious that the air has expanded by virtue of some inherent power with which it is naturally endued. This power is usually called *repulsion*: and it is admitted by all philosophers that the particles of air have a natural inherent repulsive force, by means of which they are continually endeavouring to recede from one another. Hence it is that air becomes expansible to an amazing degree, and any portion of it may be made to occupy a space immensely greater than that which it occupies naturally at the surface of the earth.

On the other hand, any portion of the air at the earth's surface may be condensed into a smaller and smaller compass than that which it naturally occupies. If, for instance, an inverted glass tumbler were to be held just over the surface of the water contained in a glass jar, it would contain a certain quantity of air, which would occupy the whole capacity of the tumbler; but if this tumbler with its contained air, were to be pressed down into the water, the air would no longer occupy the whole capacity, but would be compressed into a less space, and a portion of water would enter the lower part of the inverted vessel: and the deeper in the water the confined air was taken, the less space would it occupy. This is a very decisive experiment, and the simplest I can think of for showing the compressibility of air. A small piece of cork may be placed on the surface of the water beneath the tumbler, which will always indicate the height to which the water ascends inside, at different depths, and consequently show the space occupied by the air. Having now become acquainted with these two facts, the expansibility and the condensibility of air, we learn that air has the quality of being *elastic.* But it must not be forgotten that this *quality of elasticity* which air possesses is a mere consequence of the natural *inherent repulsion* of its particles.

By keeping in view the consequences of the attribute of repulsion which air possesses, whilst contemplating electrical phenomena we shall be enabled to account for a great variety of facts which would otherwise appear inexplicable. The electric matter, or, the electric fluid, as it is more frequently called, is much more highly elastic than common air, and therefore can be condensed and attenuated by employing proper means to a very great extent; but its motions, when in the act of expanding, are performed with such an immense degree of activity that, although several philosophers have attempted to ascertain its velocity, their efforts have hitherto been unavailing.

Besides the quality of elasticity in common with air, and other kinds of gaseous matter, the electric fluid possesses others peculiar to itself. Its activity is superior to that of any other known kind of matter: it enters into the pores of the most compact solids, and is to be found in every kind of tangible matter. It constitutes a portion of the atmosphere, and frequently accumulates to an amazing extent in the clouds, gradually increasing in density, till its elasticity becomes sufficiently great to enable it to burst from its aerial prison in a compact form, and exhibit itself in all the majesty and splendour of lightning.

It is a remarkable fact that the motions of the electric fluid are much more facilitated by some classes of bodies than by others. The metals are considered to facilitate the progress of the electric fluid to a greater extent than any other class of bodies whatever But the metals themselves, as individual bodies, vary very considerably in the degree of facility which they respectively offer to the motions of the electric fluid: copper offering the greatest facility of any known body, and lead, or iron, perhaps, the least of any of the metals. But it would be impossible, in the present condition of the science, to give a correct table of the various degrees of facility which different bodies offer to the motions of the electric fluid: for although much has been attempted to be done, and much more pretended to have been done, in determining so important a particular in electricity, it is lamentable in the extreme to have to acknowledge that but very little has absolutely been accomplished in this interesting inquiry.

Those bodies which offer comparatively great facilities for the motion of the electric fluid, are usually called *conductors;* and those which offer the least facility, being supposed to present an absolute *resistance* to the motions of the fluid, have been called *non-conductors.* Now, as the terms *conductors* and *non-conductors* of electricity, are well known from their long use, and as I am not disposed to attempt to supplant by others, any familiar technicalities, such as these, which have been of considerable benefit in the promotion of the science, I can find no objections to place before my readers the following tables of what has been considered *conductors* and *non-conductors* of electricity, which I find in Mr. Singer's excellent "Elements of Electricity":—

CONDUCTORS.

All the known metals,
Well burnt charcoal,
Plumbago,
Concentrated acids,
Powdered charcoal,
Diluted acids and saline fluids,
Metallic ores,
Animal fluids.
Sea water,
Spring water,
River water,
Ice and snow,
Living vegetables,
Flame,
Smoke,
Steam,
Most saline substances,
Rarified air,
Vapour of alcohol and æther,
Most earths and stones.

NON-CONDUCTORS.

Shell-lac, amber, resins,
Sulphur, wax, jet,
Glass, and all vitrifications; talc,
The diamond, and all transparent jems,
Raw silk, bleached silk, dyed silk,
Wool, hair feathers,
Dry paper, parchment, & leather
Air, and all dry gases,
Baked wood, dry vegetable substances,
Porcelain, dry marble,
Some silicious and argillaceous stones,
Camphor, elastic gum, lycopodium
Native carbonate of barytes,
Dry chalk, lime, phosphorous,
Ice at — 13° of Fahrenheit's thermometer
Many transparent crystals, when perfectly dry,
The ashes of animal and vegetable substances,
Oils, the heaviest appear the best,
Dry metallic oxides.

Mr. Stephen Gray, a pensioner of the Charter House, was the first person to discover the conducting power of metals, and to ascertain the great difference, in this respect, between a metallic wire, and a cord of hemp, or silk. This discovery was made on the 3d of July, 1729, it was perfectly accidental, and occurred from the circumstance of substituting a metallic wire for the suspension of an electrized body, in lieu of a silken cord which had broken. Dr. Priestley, at the suggestion of Dr. Franklin, seems to have been the first philosopher who undertook a series of experiments, for the purposes of ascertaining the different degrees of conducting power possessed by different bodies. Several other philosophers have also paid considerable attention to this subject, though, as I have before stated, little more has been accompished than the ascertaining of a few general facts: for there still remains much difference in the tables given by different authors. The following table is taken from Cavallo's "Complete Treatise of Electricity," 2nd edition published in 1782:—

CONDUCTORS.	NON-CONDUCTORS.
Gold,	Glass, and all vitrifications, even those of metals,
Silver,	All precious stones: the most transparent the best,
Copper,	All resins & resinous compounds,
Brass,	Amber,
Iron,	Sulphur,
Tin,	Baked Wood,
Quicksilver,	All bituminous substances,
Lead,	Wax,
Semi-metals,	Silk,
Animal and vegetable charcoal,	Cotton,
The fluids of the human body,	All dry animal substances, as feathers, wool, hair, &c.
All fluids, excepting air and oils,	Paper,
The effluvia of flaming bodies,	White sugar,
Ice,*	Sugar candy,
Snow,	Air,
Most saline substances, the best being metallic salts,	Oils,
Soft stoney substances,	Calces of metals,
Smoke,	The ashes of animal and vegetable substances,
The vapours of hot water,	All dry vegetable substances,
Highly attenuated air.	All hard stones, the hardest the best.

Professor Cumming, in his translation of Demonferrand's "Manual of Electro-Dynamics," gives the following table of the conducting powers of metals—

Silver,
Copper,
Lead,
Gold,
Brass, zinc,
Tin,
Platina,
Palladium,
Iron.†

It would be useless to give any more tables of the conducting and non-conducting powers of different kinds of matter, as there are no two that agree in every particular. For my own part, I am of an opinion that all bodies are conductors more or less, metals being the best class of conductors, and vitrious and resinous substances being about the worst. Much, however, will depend upon the extent of the electric force employed, and much again upon the length of the bodies upon which that force has to operate.

When any body in a state of electrization is supported on a non-conductor, as by a glass stem, or suspended by silk or other non-conductor, it is said to be *insulated.* There are many other technicalities which I shall have to notice as I proceed, but it will not be necessary in this place to introduce any more than those already mentioned.

---

* According to Achard, ice conducts the electric fluid whilst it remains above a certain temperature, but is not a conductor below that temperature.

† In all these tables, those bodies which are first in the list, are considered the best of their kind; and the others take precedence of all those below them.

The motions of light bodies by the action of sealing-wax, glass, &c., already noticed in the last lecture, are phenomena of a high interesting character, and are a portion of those which must necessarily be regarded as of an elementary character, independently of a knowledge of which no plausible hypothesis of electricity could possibly be formed: on which account it will be necessary to recur to them again, and to point out other experiments from which similar results may be obtained. But it must not be expected that because the results, by various modes of experimenting, are similar or of precisely the same character, that they should be of precisely the same extent, or degree of power. The light emanating from two burning candles of different dimensions, may be, and generally is, of precisely the same character, but the *intensity* of the light, from the two sources, may be very different: or, we may say, the *quantity* of light proceeding from one of the candles is very different to the *quantity* of light proceeding from the other. If similar reasoning be applied to the display of electrical phenomena, we may easily understand that, notwithstanding the identity of the *character* of the motions produceable from different sources of electric action, the quantity or intensity of those motions may be very different. And as some sources are sufficiently vigorous to put into motion bodies of a considerable magnitude, and others so exceedingly feeble as to require the employment of the most delicate apparatus for their detection, it will be necessary, before proceeding to other experiments, to describe such instruments or pieces of apparatus as may be wanted for carrying on those experiments with which we ought to be made familiar as soon as possible.

The instruments which are usually employed for the detection of feeble electric action, are called *electroscopes,* of which we have several forms. The simplest electroscope, and one which may be frequently employed, is merely a single fibre of flax, silk, or any other such flexible article as will bend to slight electric forces. The fibre may be supported in any manner you please, so that it be permitted to hang freely in a vertical direction. Fig. 4, plate vii., is an electroscope of this kind, where the fibre *f* is supported by, and hangs freely from, the wooden stem *s*. Having rubbed a stick of sealing wax against the sleeve of your coat, present it to the lower end of the suspended fibre, and you will see that it bends towards the sealing wax; and if you bring them sufficiently near to each other, the fibre will adhere to the wax for some considerable time. In this experiment you have an electric *attraction* exhibited as decidedly as by the motions of the pieces of paper in the former experiments: but if the fibre be not very dry and warm, you have not that jumping to and fro, as with the pieces of paper, for the fibre of the electroscope clings to the wax without leaving it, till the electric force is so far exhausted as to be no longer able to hold the fibre to the surface of the wax. The action will, in many instances, continue a long time, and by paying attention you may observe the fibre to change places on the surface of the wax, and this very frequently, if you accommodate the wax the motions of the fibre, by moving the former so as to facilitate the motions of the latter. If, however, the fibre be very dry and somewhat warm, it will sometimes recede from the surface of the wax, in the same manner as the pieces of paper, and will lean towards the stem *s*, if very near to it, and even strike against it, and after remaining attached to it a short time, will again return to the waz: and repeat these motions several times, till the electric force is too far exhausted to produce them any longer.

I will not detain you, in this place, with an explanation of the cause of the electroscope fibre continuing to be attached to the surface of the excited wax under some circumstances, and not under others; because I am desirous of first making you acquainted with the structure and method of using another simple electroscope, which will exhibit the principles upon which they are founded in better perfection, than by that made of a single fibre.

Fig. 5, plate vii, will represent the form of a very simple electroscope which may be used to great advantage in some electric inquiries. It consists of a glass stem fixed in a wooden foot, and a projecting horizontal brass wire arm, terminated with a small brass ball, When the foot of this instrument is made of nicely turned and polished mahogany, and the brass arm and its ball well polished and lacquered, the instrument assumes a very pretty appearance. Over the farther end of the horizontal arm is hung a flaxen fibre, to each end of which is attached a very small ball of the pith of the elder. As the glass stem of this instrument is a non-conductor, it is incapable of carrying off any of the electric action of the horizontal arm, or of the fibres and their balls; and as the atmospheric air is also a non-conductor, all that part of the instrument which is supported by the glass stem is insulated. As, however, glass has a great tendency to collect moisture on its surface, the stem of this instrument must be kept warm, and occasionally wiped with dry cloth to preserve insulation as far as possible. If the surface of the glass be covered with a good coating of lac-varnish, the insulation may be maintained for a long time without much trouble.

Let us now again excite the stick of sealing wax, and afterwards present it to the upper side of the horizontal arm of the electroscope, fig. 5. The pith balls will diverge from each other, as represented in the figure, before the wax comes into contact with the metallic arm. But if the wax be withdrawn without touching the metallic arm, the balls will again colapse, and show no electric action.

Excite the wax again, and bring it into contact with the arm of the electroscope, drawing its surface over the arm. The pith balls will diverge as before, and will remain divergent, even when the wax is withdrawn. And if the room, in which the experiment is made, be dry and warm, and the air perfectly still, the balls will remain divergent for a long time, even several hours.* But in all cases the divergency will gradually lessen from the first moment that the excited sealing wax is withdrawn from the electroscope, and, eventually, the divergency entirely disappears. Precisely the same kind of phenomena are displayed by the action of any excited body whatever, provided its electric forces be sufficiently powerful.

Hence you may employ excited glass, amber, sulphur, paper, &c., in your experiments with this instrument, and the pith balls will diverge with each excited body. Dry writing paper rubbed with indian-rubber, becomes highly electric ; and so does coarse brown paper when drawn quickly between the coat sleeve and a woollen table cloth ; or between the coat sleeve and the trousers. When the paper is made pretty warm before the friction is given to it, and the knuckle presented afterwards to its surface, a crackling noise will be heard, and sometimes sparks will be seen between the knuckle and the paper. This experiment answers best during frosty weather. Similar phenomena may be produced by stroking the back of a cat. Puss often becomes uneasy by this treatment, and the hairs of her back and tail brush out in a very strange manner.

I must now bring forward an experiment the results of which are something different to any I have yet offered to your notice. Excite the sealing wax as before and draw it over the arm of the electroscope fig. 5, and when taken away the balls will remain divergent. Again excite the wax, and again make it approach the arm, on the upper side, but without touching it, you will observe the balls separated further than before, but as you withdraw the wax again, the balls will fall to their former position. The balls may be made to separate further from, or approach nearer to, each other, for several times, by alternately advancing the excited wax to the arm of the instrument, and withdrawing it from it. If, after the balls have been divergent by the first application of the wax, the latter be again excited and then presented towards the balls, you will observe them to recede from it: and with a little practise, you may deflect the balls from the wax in any direction you please.

ERRATA.—Page 198, in the heading of article XXV. for "Van Kobell," read "Von Kobell: *and* the same in the heading at the top of each left hand page of that article. Line 6 of that article, for "into" read "on to."—Page 199, line 2, from bottom of page, for "Demerara," read "Damara." Same page, first line in the note, for "end" read "and." In the article "Electrotype," in pages 237 and 238, all that part which is below Mr. Cartwright's letter in page 238, is to be read after the word "engraving," at the end of the third paragraph, page 237.

ANNALS OF ELECTRICITY &c. Plate 7. Vol. 5.

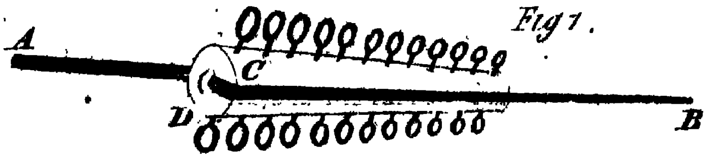


Fig 1.

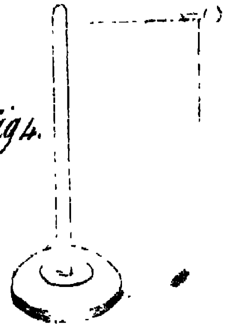


Fig 4.

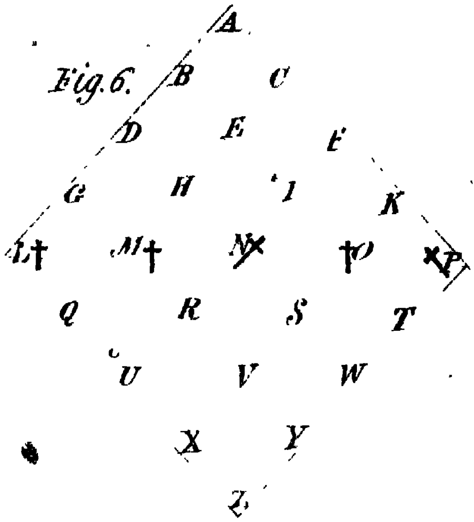


Fig. 6.

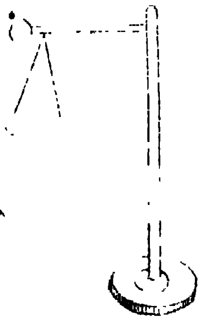


Fig. 5.

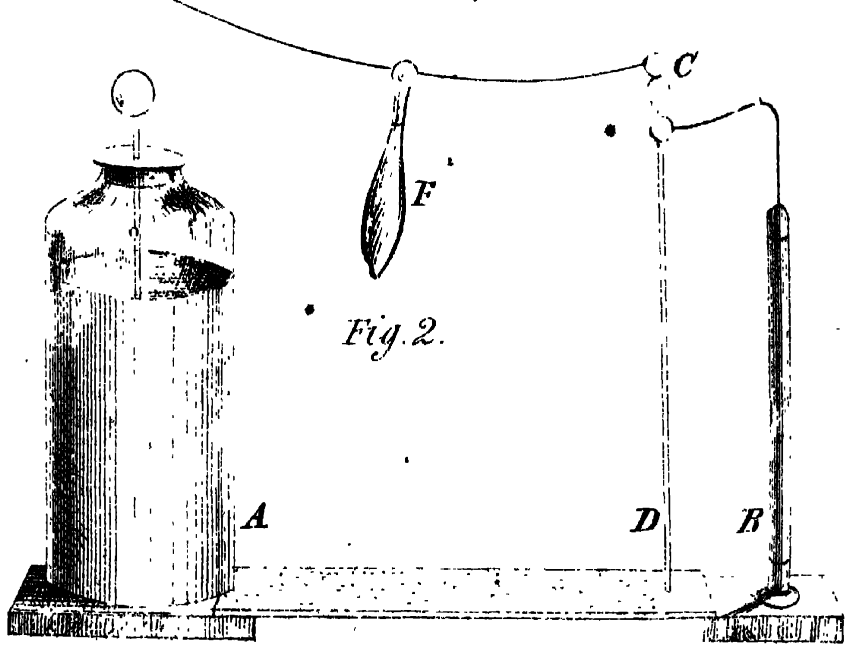


Fig. 2.

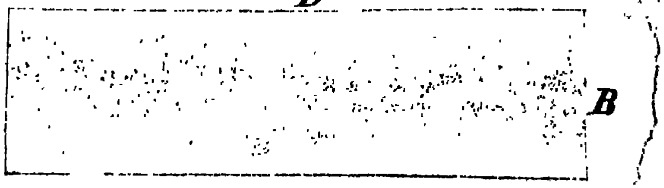


Fig. 3.

THE ANNALS
OF
ELECTRICITY, MAGNETISM,
AND CHEMISTRY;
AND
Guardian of Experimental Science.

JULY, 1840.

I.—*Report of the Committee appointed by the Admiralty to examine the Plans of Lightning Conductors, of* W. Snow Harris, Esq. F. R. S. *and others.* Abridged.

We beg to observe before detailing the cases which have been brought before us, that we do not consider it to fall within the province of the present report to enter into the general question of the efficacy of conductors in affording protection against the injurious effects of lightning, as this would lead to an investigation of the first principles of electrical action; and the fact of their efficacy may be considered to be established beyond all doubt, by the experience of the last 80 years, and the unanimous opinions of scientific men of all countries.

With reference to the first point to which their Lordships' memorandum directed our attention, viz., "Whether, in cases where ships not having lightning conductors have been struck by lightning, it appears that other ships in company having conductors have not been struck, or have escaped injury?" we beg to adduce the following cases:—

1. In 1815, H. M. S. Norge, was struck by lightning at Jamaica, and lost her maintop-mast and topgallant-mast, whilst the Warrior, 74, which was lying close to her, with her conductor up, received no injury, though the electric fluid was observed absolutely to stream down it. Amongst many other ships which were in Port Royal Harbour at the time, none received any damage but a merchant vessel, which, like the Norge, had no conductor up.

2. In 1824, H. M. S. Milford, whilst in ordinary in Hamoaze, was struck by lightning, and the foremast and foretop-mast (both very small spars, for the purpose only of making signals) were shattered; she had no conductor up. The Caledonia, of 120 guns, with her lower masts in, and her conductor up, was lying about 80 fathoms distant, and received no injury.

3. In 1824, H.M.S. Phæton was struck by lightning, and the foremast and foretop-mast were totally shivered. The Adventurer was at anchor within a cable's length, with her conductor up, and escaped without any damage, though supposed to have been struck more than once upon that occasion.

4. In 1830, H. M. S. Ætna, when coming to, off Corfu, was struck by lightning, three heavy discharges descending by the conductor, and passing to the water without injuring the spars. The Madagascar and Mosquito, which were in company at the time, and had no conductors up, were repeatedly struck, and received considerable injury.

5. In 1837, the Cochin tank-vessel, in Trincomalee Harbour, was struck by lightning, and her foremast (without a topmast) was shivered, whilst H. M. S. Winchester, at the distance of two cables' length, was uninjured, though the lightning was seen to pass down her conductor.

6. In 1837, the Pelican, sloop-of-war, whilst on the coast of Africa, was struck by lightning on the foremast, and lost her topmast and topgallant-mast; the conductor was not up at the time. The Waterwitch, at two cables' distance, had her conductor up, and escaped injury.

7. In 1838, H.M.S. Ceylon, in Malta Harbour, was struck by lightning, and her pole, foretop-mast, and foremast were shivered; she had no conductor up, and was lying close to the Talavera, Bellerophon, and Dock-yard Sheers, all of which had conductors up at the time, and met with no injury.

These cases have been fully authenticated.

In addition to these instances of the decided protection afforded by conductors, and the disastrous consequences which have arisen from the want of them, we beg to call their Lordships' attention to the case of the New York Packet.

It appears that on her passage to Liverpool, in 1827, this ship was struck by lightning and sustained considerable injury. The conductor was not up at the time; but the weather continuing stormy, it was got out and triced up to the mast head. The ship was a second time struck by a most severe stroke of electricity, which fused the chain, but passed into the water without committing further damage.

It would be easy to multiply instances of the local protection afforded by metallic bodies accidentally present in ships which have been struck by lightning, as well as cases in which single ships have escaped injury by means of a conductor; many such have been adduced in evidence before us; but these cases apply rather to the general question of the advantage of conductors.

Under this head of the Report, however, we may perhaps be allowed to state to their Lordships the result of our inquiries with regard to the common prejudice, that conductors have the power of attracting a flash of lightning, which in their absence would not have fallen on the ship in which they are fitted.

The numerous cases of accidents to ships without conductors, and the comparatively rare occurrence of lightning having been noticed to strike on a conductor, would tend to negative such a supposition; and it may be observed, that in several instances the electricity has been seen to strike down on the surface of the water at no great distance from a ship fitted with a conductor. This phenomenon occurred in Plymouth Sound, within a moderate distance of the Caledonia, whilst fitted with Mr. Harris's conductors; and in the instances of the Milford, Cochin, and Ceylon, already mentioned, these ships with very short spars and no conductors, were struck by lightning when within a few hundred feet of ships with considerably higher masts and conductors up; and in the instance of the Cochin tank-vessel alone was the electric fluid observed to descend on the conductor of the ship which was lying near her (the Winchester), thus affording evidence, either of the little influence exerted by conductors in inducing or attracting an explosive discharge, or of their efficacy in harmlessly and imperceptibly conveying the electricity to the water.

As the objection of the attractive power of conductors has been brought forward by the Surveyor of the Navy, as especially applicable to those of Mr. Harris's principle, it is right to state, in addition to the cases above-mentioned, that with regard to Mr. Harris's, no facts have come under our knowledge which would lead us to coincide in his opinion; but on the contrary, amongst the several ships fitted on Mr. Harris's plan, which have for many years past been employed in tropical climates, and were exposed, as stated by their commanding officers, to very severe lightning, we have found great difficulty in obtaining direct evidence of their having been struck at all; and in two or three instances only has the fact been satisfactorily observed, and no case of injury has been recorded.

Professors Faraday and Wheatstone have been consulted on this point; and it is their unequivocal opinion that conductors possess no inherent property of attracting or inviting a discharge from a cloud at a distance.

If there be a projecting object, like a mast, within a moderate distance of the point from which the discharge takes place, the electricity will descend by it, whether fitted with a conductor or not, as affording a line of less resistance than it would meet with from the non-conducting property of the air.

"The radius within which it has been considered that a conductor will determine or conduct the electricity is double its own length, provided the discharge takes place within that space, but it has no power to cause the discharge;" on the contrary, "at all times its tendency is to draw off the electricity from the atmosphere, and thereby diminish the liability to an explosion."

In concluding our remarks on this first head of the inquiry, we beg to observe that every search has been made for cases of injury sustained by ships fitted with conductors, and though several statements to that effect have been brought under our notice, not one has been substantiated.

And no instance, so far as we are aware of, has ever occurred of a ship sustaining injury when struck by lightning if the conductor was up to the mast-head, and the continuity uninterrupted to the water.

With reference to the second head of the inquiry, namely:—What conductors have been used in ships, either of the Navy or in those belonging to private merchants? we beg to state, that the conductors which have hitherto been used in the Navy, consist of a copper chain, composed of rods of about two feet in length, and ·175, or about one-sixth of an inch in diameter, with an eye at each end. These bars are linked together by rings and the conductor terminates in a rod of the same dimensions, which tapers to a point, and is made with a turn in it near the base, to receive the line to which it is attached throughout its whole length, for stopping to the topgallant backstay when triced to the mast-head.

It should be observed that these conductors are not issued to every ship, but only supplied when demanded, and one only is allowed.

A chain of similar form, composed of either copper or iron, is said to be used occasionally in merchant vessels, but we have had no opportunity of inspecting one.

In the French Navy, a metallic rope composed of mixed metal wire, is attached to the mast-head immediately under the truck, leads down to the top-gallant cross-trees, and thence by the topgallant backstay to the channel, and descends into the water. A copper spindle of about three feet in length, tapering from an inch to a point, is screwed into the mast-head, nine inches of the upper end being hardened and gilded.

This description was obtained by Mr. Rice, foreman of shipwrights, at Chatham Dockyard, from the officers of the French frigate Calypso, in 1832, when under repair. A piece of it was produced for our inspection, composed of three strands of eight wires each, and measuring one-eighth and a half inch in circumference.

Mr. Harris's conductors, which have been fitted for trial in the ships named below*, are composed of two plates of copper rivetted together, so as to form an elastic and continuous line of metal; the inner plate being one-sixteenth and the outer one-eighth of an inch in thickness, their breadth varying according to the class of the width of the plates which have hitherto been used, as they are considered to be unnecessarily large, and the subject will be discussed in the sequel.

These plates are inserted in dovetailed grooves, in the after part of the masts, and extend from the truck to the keelson; a copper plate of the same dimensions is led over the caps, and the continuity is preserved at all times by a tumbler on the caps, consisting of a short copper bar with a hinge at the base, by which it leans against the conductor of the topmast, whether fidded or housed; and their lordships will perceive by reference to the drawings which accompany the Appendix, that a stop is placed on the exterior by which the tumbler is prevented from falling backwards.

Copper plates of equal dimensions to those on the lower masts are placed under the heels and steps of the masts, and are thence led along the keelson in contact with the copper fastenings.

In order to insure connexion with the copper sheathing, bolts are driven transversely through the keel, so as to meet those passing down from the keelson.

Copper plates are likewise led along the underside of the beams of the lower and orlop decks to the principal copper fastenings, and ultimately terminate in the sheathing, thereby combining all the chief masses of metal in the hull and spars of a ship with the conductors, and affording by means of its ultimate connexion with the copper sheathing a vast surface in contact with the water for the dispersion of the electricity.

With reference to the third head of the inquiry, namely: What the objections are to the conductors now in use? we beg to state, that the chief objections of a practical nature which have been urged against the common chain conductors, are that not being fixtures, they are seldom ready when required, are kept packed in a box, and usually stowed away in the store-rooms, and when thunder squalls arise suddenly and unexpectedly, as frequently occurs, especially in the tropics, the damage is done to the ship before they can be got out and triced up. At all times there is danger in tricing them up, as it is usually done when lightning is anticipated.

---

* Actæon, Asia, Beagle, Belvidera, Blanche, Caledonia, Dryad, Druid, Forte, Revenge, Saphire, St. Vincent, Spartiate.

In 1834, on board the Thunderer, the men had not left the conductor five seconds when the lightning descended with extreme violence; and in one instance, on board a vessel in the mouth of the Mississippi, several men were struck dead at the moment of hoisting one up.

In dark nights the difficulty of tricing them up properly is greatly enhanced, and in heavy weather, when much needed, it has been found impracticable to get them up at all.

Their construction is very slight, and the rings not being welded together, a trifling strain breaks them.

In the event of a topmast or topgallant-mast being carried away the conductor is likely to be lost, and at any rate the ship is unprotected until it can be got in, and triced up to another mast. This case occurred to the Jupiter.

As conductors, their capacity is not sufficient for the safe transmission of heavy discharges of electricity, and in several instances the metal has been fused or disjointed. This occurred to the Dublin and Ætna.

In short, we cannot but regard them as a temporary and inadequate expedient.

By not being permanently fixed, the security of the ship is left to the opinion of the commanding officer as to their utility at all, or necessity at the moment.

They are not calculated to be applied in all weathers, are subject to all the casualties to which the ship's rigging is exposed, and liable to lead to serious accidents by the end being brought in board, the continuity interrupted, or the end lifted out of the water.

4. What the advantages or disadvantages of Mr. Snow Harris's conductors, as compared with others?

The advantages to be derived from the adoption of Mr. Harris's plan are, the removal at once of all the objections and liabilities to which the common chain conductor is exposed.

A continuous line of metal from the truck to the water is permanently fixed, and if it be found necessary to strike any of the masts, or one or more be carried away, a safe conductor will still remain. By its connexion with the detached masses of metal used in the fastenings of the hull, and its final junction with the copper sheathing, the important advantages of great electrical capacity are obtained, and of ready means under all circumstances, for the rapid diffusion of the electricity over a vast surface of metal in contact with the water.

Professor Faraday stated to the committee, that it was his opinion that " Mr. Harris's conductors met every case that he could possibly conceive to occur, and offered no one disadvantage or objection whatever;" and Professor Wheatstone stated, that,' he could see no objection whatever to Mr. Harris's conductors in a scientific point of view."

A committee of the Royal Society, appointed in 1823, to consider the merits of these conductors, as well as Dr. Wollaston and Sir Humphry Davy, have stated their approval of the principle of Mr. Harris's plan.

With reference to the disadvantages of Mr. Harris's conductors, we beg to state that all the objections which have been brought against them have, to our minds, been sufficiently removed by the evidence adduced before us; it will be proper, however, to state these objections to their Lordships, with the facts and opinions which have influenced our conclusions.

The objections may be divided under the following heads:—

1. Those of a scientific nature, involving principles of electrical action.

2. Those of a practical description, as tending to injure and weaken the spars.

3. The indirect objection on account of expense.

1st. Theoretical objections.

First. That Mr. Harris's conductors attract the lightning.

This applies equally to all conductors, and has been already refuted.

Secondly. That danger arises from the "lateral explosion."

Mr. Martyn Roberts has objected to the conductors being led through the body of the ship, on account of the dangers of lateral explosion, which he considers might set fire to the ship, or ignite the magazine. His hypothesis is, "that when a discharge of electricity passes along a conductor, visible sparks would be thrown off from the sides of the conductor to any metallic or other conducting body within a moderate distance, and be capable of igniting inflammable substances." "And that such lateral discharges would be in proportion to the capacity and proximity of the secondary conductors, with reference to the volume of electricity passing down the primary conductor."

Professor Faraday stated, that "he was not aware of any phenomenon called lateral dischage, which was not a diversion or division of the primary current, and that all liabilities to a diversion of the main charge would decrease in proportion to the capability and goodness of the primary conductor; that in proportion as the number of the metal bolts are connected with the conductor would the probability of a lateral diversion diminish."

"It was his opinion that a lateral discharge could not be obtained from Mr. Harris's conductor, provided the continuity were not interrupted; and from the increased dimensions of the plates at the lower extremity, and the complete mode of connexion with the fastenings of the hull, the electricity would be so rapidly diffused that he doubted whether, with any intentional contrivance, the magazine could be ignited from the sides of the conductor. He could not but appeal to the evidence of experience to prove the efficacy and safety of Mr. Harris's plan; ships fitted with his conductors had been exposed to severe lightning, and the electricity had been known to descend by them with perfect security to every thing on board; nor was there, so far as he could learn, any instance on record of lateral explosion."

Professor Wheatstone stated, with regard to lateral explosion, that "all the cases with which he was acquainted were those of a partial diversion of the main current where the conductor was not of sufficient capacity of conduction, in which case a portion of the electricity distributes itself to any tolerably good conductor within a moderate distance.

"This, however, had only been known to occur from a very small wire, and from a conductor of the dimensions proposed by Mr. Harris, would be impossible, with such atmospheric discharges as we are aquainted with.

"The liability to such lateral diversion would be diminished in proportion to the means of diffusion; and considering the mode proposed by Mr. Harris for connecting the principal fastenings, &c., of the hull with the conductors, no danger need be antcipated from leading the electricity through the body of the ship or within a few feet of the magazine so long as the continuity was maintained."

Notwithstanding this evidence, Mr. Martyn Roberts subsequently communicated to the committee that he had made further experiments on a larger scale, which favoured his idea of the danger to be anticipated from lateral explosion."

Professor Wheatstone was in consequence requested to attend to receive from Mr. Roberts himself an explanation of the experiments which he had instituted; and we beg to subjoin the further opinion of Professor Wheatstone on the subject, which he communicated in writing.

"When the known conditions of a good lightning conductor are fulfilled, it is physically impossible that it should occasion the least accident to the building or ship to which it is attached. When injury does occur to a ship provided with one, it is because this conductor is not sufficient to carry off the whole of the discharge; the ship is then only partially protected—damage is done; but this damage must be in all cases immeasurably below what would have been produced by the whole discharge, had it not found any conductor to transmit it to the water. The danger to be apprehended from the division of the discharge may be reduced to almost nothing by increasing the dimensions and conducting power of the bars or plates which transmit the electricity, and by keeping good conductors, not connected with it, out of its immediate vicinity.

"It has been proved beyond doubt that electricity follows the best conducting path which is open to it; and that when it finds a metallic road sufficient to conduct it completely, it never flies to surrounding bodies greatly inferior in conducting power.

"The experiments of M. de Romas, made in France, with the electrical kite, immediately after Franklin's first attempt, might satisfy the most timid in this respect. 'Imagine,' writes he to the Abbe Nollet, 'that you see sheets of fire, nine or ten feet long and an inch broad, which made as much or more noise than the reports of a pistol. In less than an hour I had certainly 30 sheets of these dimensions, without counting a thousand others of seven feet and under. But what gives me the greatest satisfaction in this new spectacle is, that the largest sheets were spontaneous, and notwithstanding the abundance of fire which formed them, they constantly fell on the nearest conducting body. This constancy gave me so much security that I did not fear to excite this fire with my discharger, even when the storm was violent; and when the glass branches of this instrument were only two feet long, I conducted wherever I pleased, without feeling the smallest shock in my hand, sheets of fire six or seven feet long, with the same facility as those of only six or seven inches.

"The wire of the kite was insulated, and the sparks drawn by a metallic conductor held in the hand by means of an insulating handle, and communicating with the ground by a chain. The human body is known not to be one of the worst conductors; yet because it was two feet further than a far more perfect one, it received none of the discharge, even though the conducting path was an interrupted one.

"The phenomenon to which the name of lateral explosion has been generally given was the first observed by Henly, more than half a century ago, and has been subsequently experimented upon by Priestly, Cavallo, and more recently by Biot.

"I conceive it has no application to lightning conductors, but as it has been brought forward as an objection to Mr. Harris's plan by Mr. Roberts, it may be necessary to say a few words respecting its real nature.

"It takes place during the discharge of an electric battery, that is at the moment of the union of the positive and negative electricities accumulated on the opposite coatings of the jars; no part of these accumulated electricities has anything to do with the effect, which arises solely from the induction of that small portion of electricity which remains free on one of the surfaces of the battery or the conducting bodies attached thereto.

"This is the explanation of the phenomenon given by the best authorities, and, as Biot observes, theory and experiment unite in demonstrating to us that it is incomparably less than the direct discharge.

"Even, therefore, were lightning conductors liable to this lateral discharge, it would be easy to prevent any material damage arising from this cause; but after attentively considering the subject, and Mr. Roberts's objections, I am still of opinion that, in the case of lightning conductors, the lateral discharges that sometimes occur and produce mischief, arise solely from the insufficiency of the conductor to carry off the whole of the electric fluid which enters, as I have above stated, and the remedy to which is obvious."

The evidence of the officers who had served in H. M. ships fitted on Mr. Harris's plan, and had witnessed the effects of lightning descending by the conductors, as well as the absence of any case, so far as we can learn, of lateral explosion, even from the common chain conductor of such inferior capacity, so fully bear out the opinions of Professors Faraday and Wheatstone, that we do not hesitate to state our entire conviction of the futility of the objection on account of "lateral discharge."

Thirdly. That Mr. Harris's conductors do not afford a continuous line of solid metal. This objection will embrace the ninth question in their lordship's instructions, namely, "Whether the continuity can be preserved in all probable circumstances, and whether the danger is not increased in case of interruption of the conductor, or of its being of inadequate dimensions?"

On this point again we beg to quote the opinions of Professors Faraday and Wheatstone.

Professor Faraday stated, that "the conducting power of the plates would be but little diminished by the continuous solidity of the metal being interrupted, so long as the portions of the conductor remained in contact; that even supposing the tumblers on the caps were, through accident, to be open to the extent of half an inch or an inch, no injury would be caused to the surrounding woodwork by the electricity leaping from one point to the other; that rope, or any substance of small conducting capacity, if placed between the two points, would perhaps be destroyed, but the probability appeared so small of any accident occurring whereby the continuity could be interrupted, that he should not hesitate to say there would be no objection to Mr. Harris's plan on that score."

Professor Wheatstone stated, "he was of opinion that if the copper plates of which the conductors were composed were in mechanical contact, there would be no danger of an explosive discharge along the line of junction; and that their capability for carrying off the electricity would be so little diminished by a slight interruption of the continuous solidity of the metal, that there could be no objection to them on the ground of being formed of separate pieces of copper.

"That the continuity of the conductors appeared to be sufficiently provided for by the tumblers on the caps, and that no danger need be anticipated supposing they were opened by accident to the extent of an inch or two, as the electricity would pass from one point to the other without damaging the contiguous woodwork."

Their lordships will perceive by reference to the description and drawings of Mr. Harris's plan that every means have been adopted to ensure the preservation of the continuity under all possible circumstances, and in no case is an interruption of any consequence likely to occur.

Fourthly. The danger of accidents to men in contact with the conductors at the moment of the electricity decending.

No instance of any accident of the sort has been known to occur.

On board H. M. S. Beagle, the lightning was seen to strike the conductor, and though it passed within eight inches of the purser's head, who was asleep in his cabin at the time, he experienced no ill effects, beyond being woke by the general concussion.

Professor Faraday stated, that he believed a man would receive no injury if he were leaning against Mr. Harris's conductor when the electricity descended, and that any opinion to the contrary must be only assumption.

2. Objections of a practical description, as injuring and weakening the spars.

The surveyor of the navy and Mr. Edye stated that they were of opinion that Mr. Harris's conductors injure the spars.

Frist. That the nails by which they are fixed split the spars; and when the masts are strained by carrying sail, the wet might get into the splits on the weather side, and cause injury.

Secondly. That the conductors weaken the spars.

In support of the opinion that the nails split the spars, the surveyor of the navy considered he was borne out by the report of survey on the Caledonia's spars, from which Mr. Harris's conductors had been stripped.

The officers of Plymouth Dockyard state, in their report of the 15th June, 1839, that "if the conductors had been allowed to remain in the spars, there would not have been any objection to their re-issue to the ship or ships to which they belonged."

"That if the conductors had not been removed from the topgallant-masts and flying jib-boom, 'the rents occasioned by the nails would not have been apparent,' and the necessity for reducing the spars on account of the grooves made for the reception of the conductors would not have existed." "In several instances the injury done to the smaller spars may be attributable to the great number of nails used for fastening the conductors, and which rendered the small spars of the Caledonia unserviceable."

We beg to observe, however, in this instance, that nails are stated to have been used of two inches and a half in length to fix the copper plates, of only three-sixteenths of an inch, on a spar of six inches and a half in diameter.

The foreman of the Mast Department at Plymouth states, that "none of the spars of the Spartiate and Forte when returned into store were rendered unserviceable from the conductors, and in every instance the plates were as securely fixed as when first fitted in; that no injury from nail-holes would ever render a re-conversion of a spar necessary; and that they would never be rendered inapplicable for other or inferior purposes, if the conductors were kept in."

The officers of the Portsmouth Dockyard state that "they are not aware that any injury to the masts or spars was attributable to the application of Mr. Harris's conductors." Captain Fitzroy stated, that "in H. M. S. Beagle, when under his command, he had never found the spars split by the nails, and did not consider that they were likely to be weakened or injured by them, as the nails were flattened at the point, and passed between instead of cutting the fibres of the wood; but allowing such to be the case, no wet could ever penetrate if the masts were kept properly greased."

Secondly. That the conductors weakened the spars, (which embraces the sixth question of their lordships' instructions, namely, "Whether the conductors of Mr. Snow Harris can be so fitted as not to weaken the spars in which they are placed?")

Captain Fitzroy stated, that "the copper plates appeared to strengthen rather than weaken the spars. In so small a vessel as a 10-gun brig, the Beagle, the spars were found to be improved rather than injured, and though exposed for five years to continued service, the same spars remained at the present moment in the Beagle, with the exception of the topgallant-masts."

Commodore Pell stated, in proof that Mr. Harris's conductors were not injurious to the spars, that after four years' service the Forte was paid off with the same masts, top-masts, topgallant-masts and royal-masts.

Captain the Hon. F. Grey, Commander Turner, Captain the Hon. W. Wellesley, and Commander Norcott, who had served in ships fitted with Mr. Harris's plan, were also equally of opinion that the introduction of the copper plates tended rather to strengthen the spars. Experiments were made to ascertain the point in 1831, in Portsmouth Dockyard, under the superintendence of Mr. Harris and Mr. Rice, in the presence of several distinguished officers, &c., by which it will be seen that a spar (a jib-boom) was undoubtedly strengthened by the application of the plates, and in certain positions increased in stability upwards of a sixth; thus confirming the opinions of the officers above mentioned, who had tested the fact by experience in actual service.

Thirdly. Objections on the score of expense.

With reference to the fifth point of their lordships' instructions, namely, "What the comparative expense is of different descriptions of lightning conductors?"

The accompanying Table shows the cost of Mr. Harris's, and the common chain conductor for each class of H. M. ships.*

---

* In this Table the expense of the common conductors is omitted, but we have added this information from another table which we find in the Appendix.—Ed. M. M.

Class of Ships.	Expense of fitting each Ship with Harris's Conductors.			Expense of common Conductors.		
No. of Guns.	£.	s.	d.	£.	s.	d.
120	365	17	8	3	2	8½
84	350	15	7	3	1	3
74	317	18	6	2	16	10½
50	286	15	10	2	13	11½
46	236	1	7	2	8	1½
28	161	18	11	1	19	8
18	119	7	2	1	19	8
10	102	12	7	1	15	0

From this account it is obvious that the adoption of Mr. Harris's plan would be accompanied with a very considerable increase of expense, but we propose to show in the sequel by what means certain reduction in their cost may be effected.

We now beg to state, with reference, to the tenth head of the inquiry, namely, "Whether any other mode of fixing lightning conductors does not possess the same or greater advantages than Mr. Harris's?" That Mr. Martyn Roberts submitted to us a proposition for avoiding the dangers he considered likely to arise on the adoption of Mr. Harris's plan, from the alleged lateral explosion, by means of a rope composed of annealed copper wire, to be led from the truck down the afterpart of the masts to the lower mast-head, and thence as a backstay to the copper sheathing, to which it is to be soldered or brazed. This plan differed only from the conductor used in the French navy in its mode of application.

Mr. Edye also submitted a plan for obviating the supposed objections to Mr. Harris's conductors from their passing through the hull of the vessel, consisting of Mr. Harris's copper plates as far as the head of the topgallant-mast, (or if necessary to the top-mast head), and wire-rope back-stay on each side down to the copper sheathing.

Before entering into the merits of these two plans of very similar construction, we cannot but remark on the circumstance that the chief objections urged by Mr. Edye and the surveyor of the navy against Mr. Harris's conductors equally apply to Mr. Edye's proposition, namely, the injury to the small spars from the nails by which the copper plates are fixed, and the tendency of the conductors to attract lightning.

Mr. Edye, in his evidence, states, "he considers Mr. Harris's plan would decidedly weaken the spars, and the nails unquestionably injure by causing splits and admitting the wet, as was found in the case of the Caledonia;" while his own proposition is to apply Mr. Harris's plates to the royal-masts and topgallant-masts if thought necessary, these masts being the most liable to the injury he so unquestionably states must, in his opinion, ensue.

Professors Faraday and Wheatstone were shown the drawings and descriptions of these conductors.

The former observed with regard to Mr. Edye's, that "there was no doubt that if they could be kept in their places under all circumstances, and the rope was of sufficient capacity to carry off the electricity, they would be efficacious; but in his opinion, their liability to derangement was far greater, their capacity less; nor were they in any one point equal to Mr. Harris's; and he should greatly prefer the latter."

Professor Wheatstone stated, that "Mr. Edye's plan of a conductor appeared to him to be liable to all the casualties to which the common chain conductor was exposed. If it could always be kept permanent, and the wire ropes were of sufficient capacity, there would be no doubt they could lead off the electricity; but their liability to accidents was an insuperable objection.

"Mr. Roberts' plan appeared very similar to the metallic rope conductors used in the French navy, and was objectionable on the same grounds as Mr. Edye's."

We entirely concur in the opinions of these gentlemen, and beg to observe that both Mr. Roberts's and Mr. Edye's plans appear to us to be equally subject to all the liabilities to which the rigging of a ship is exposed in common with the chain conductor; in the event of a topmast being carried away, the ship is left unprotected, and thus, in the hour of danger, they are liable to become useless.

The weight of the wire ropes in Mr. Edye's plan would be a great objection, especially when it is considered that the whole of this weight rests on the head of the topgallant-mast alone.

A wire rope, of three-fourths of an inch in diameter, from the topgallant-mast head (on each side) of a first rate and an 18-gun brig, as compared with the weight of their hempen backstays, would be as follows, namely:

	Hemp.	Wire.
First-rate: topgallant backstay...	246lbs.	357lbs.
18-gun brig..........................	92	268

And as it is necessary to provide conductors for each masthead, and their capacity must be the same, whether in a first-rate or sloop-of-war, the comparative excess of weight would be still greater in the fore and mizen masts.

In carrying sail, especially in dry weather, when the rigging is slack, the metal not being affected in the same degree as the hempen ropes, the strain would probably be so great on the conductor, as to carry away the topgallant-mast, or the wire backstay.

It has been proposed to place a globe of glass on the masthead in lieu of a conductor, on the hypothesis that, from the non-conducting property of glass, it would serve as a repellant to lightning; but Professor Faraday considered "it would not be a preventive, but would rather tend to increase the liability to an explosive and to a more violent rather than to a silent discharge, and would therefore increase the danger."

Professor Wheatstone was of opinion that "such a proposition was an absurd notion, and would be dangerous in the extreme, inducing, in many instances, an explosive discharge, where a conductor might have silently drawn off the electricity."

After maturely considering the several points now discussed, and the evidence, both practical and theoretical, which has been submitted to us, we are unanimously of opinion, that of all the plans of conductors which we have had under our consideration, that proposed by Mr. Harris affords the best means of preventing the injurious effects of lightning.

We now propose to show by what means the expense of fitting Mr. Harris's conductors may be reduced.

In considering this question, it will be necessary to divide it into three separate heads, namely:

First, Dimensions of the plates of the conductors.

Secondly, Abbreviation of the conductors.

And, Thirdly, The number required in each ship.

First, in order to ascertain the feasibility and safety of reducing the size of the copper plates originally proposed by Mr. Harris, it is necessary to enter into the question of the requisite dimensions of metallic rods to insure protection against lightning.

The capability or power of a metal rod for the safe transmission of electricity is in direct proportion to the area of section or its metallic contents.

A copper rod, of half an inch in diameter, has never been known to be fused or heated red hot by an atmospheric discharge of electricity, and thus a standard of sufficiency is afforded with which all conductors may be compared.

On consideration of this fact, and the rare occurrence of the common chain of one-sixth of an inch being fused, we were led to conclude that Mr. Harris's plates were larger, especially in the lower masts, than experience seemed to require for the safe conduction of electrical discharges, and as their dimensions varied in the different classes of ships, and it was apparent that whatever was requisite for one was necessary for all, without reference to the size of the ship, we desired Mr. Rice to prepare a table of the comparative dimensions of Mr. Harris's conductors on the scale originally fixed by him and that proposed by us, and Mr. Harris having expressed his entire acquiescence in the reductions, we beg to recommend the following scale of dimensions for the copper plates for the masts and spars of ships of all classes in the event of these conductors being used in future in H. M. Navy, namely:—

	Inches in width.
Lower masts and bowsprits	4
Topmasts and jib-booms	3
Topgallant-masts, and flying jib-booms	$2\frac{1}{2}$
To taper from hounds to truck from	2 to $1\frac{1}{2}$

The plates remaining of the same thickness in all, viz. $\frac{3}{16}$ths of an inch.

These reductions will effect a commensurate diminution in the expense, and the following account shews the cost of fitting each class of H. M. ships on the scale proposed; viz.

	Total Expense of the Conductors.	Value of copper as "old copper," when no longer serviceable as conductors.	Actual Cost to the Crown.
	£.	£.	£.
First-rates	258	133	125
Second-rates ...	246	127	119
Third-rates	230	119	111
Fourth-rates ...	214	110	104
Fifth-rates	192	99	93
Sixth-rates	151	85	66
Sloops	98	47	51
Brigs	87	43	44

In order to remove any doubt as to the security with which these reductions may be effected, we beg to quote the following opinions.

Professor Faraday stated, that "he had no doubt the reductions of the copper plates proposed by the committee could be effected with entire security."

Professor Wheatstone stated, that "in the Report of the Committee of the Academy of Sciences of Paris, appointed to investigate the utility of lightning conductors, it is mentioned that there is no instance on record of an iron rod of half an inch in diameter being fused or made red-hot by a flash of lightning; and considering that the capacity of copper for the conduction of electricity was from six to eight times greater than that of iron, and that the area of the section of Mr. Harris's conductors at the mast head was ·4688, of a square inch, and in the lower masts ·7500, whilst that of an half-inch rod was ·1970, he felt convinced that they were perfectly safe."

Secondly. Abbreviation of the conductors.

It appeared to us to be a question of great importance to consider, whether the time and expense of docking ships for the purpose of drawing the copper bolts to effect the metallic continuity with the keelson bolts and the copper sheathing might not be dispensed with in cases of emergency, when ships were required to be fitted out with expedition, and at the same time that they might possess the advantages of these permanent conductors. We therefore submitted to Professors Faraday and Wheatstone, whether sufficient security would be afforded against lightning if the conductors were led down no farther than the orlop-deck in line-of-battle ships and frigates, and to the lower deck in sloops and brigs, and the metallic connexion with the copper sheathing maintained alone by the transverse copper bands leading under the beams.

We beg to subjoin their opinions.

Professor Faraday stated, that "he is of opinion there would be no objection to cutting off the lower portion of the conductors, say from the lowest deck, provided that four, or even only three of the transverse copper bands leading under the beams to the copper sheathing remained, as these would afford ample means for the dispersion of the electricity.

"There would be no danger whatever from the electric discharge being thus deflected at right angles from the perpendicular line, as it would always take the line of least resistance, and is totally independent of momentum."

Professor Wheatstone stated, that "if, for the sake of economy, the conductors were carried no further than the lowest deck instead of to the keelson, he was of opinion that the transverse copper plates under the beams, if connected with the copper sheathing by conductors of sufficient capacity, would afford ample means for carrying off the electricity, and that there could be no objection to such an alteration, provided always the continuity could be equally well maintained."

The reduction in the expense of fitting the conductors on this plan, as compared with the former account, is shewn in the following estimate, viz, :—

	To the Lower Deck. Total Cost.	To the Keelson. Total Cost.
	£.	£.
First-rates	229	258
Second-rates	218	246
Third-rates	206	230
Fourth-rates	191	214
Fifth-rates	171	192
Sixth-rates	136	151
Sloops	88	98
Brigs	79	87

By this plan of abbreviating the conductors, a reduction of about one-seventh of the expense of fitting ships would be effected; and as there appears to be no objection on scientific grounds, it may be resorted to as a safe expedient in cases of emergency. But we still would beg to recommend that the copper plates be carried down to the keelson, in all ships which may be built or docked in future.

Thirdly. The number of conductors required in each ship.

The question of the necessity of having conductors fitted to each mast and the bowsprit depends upon the confidence to be placed in the supposition that a conductor protects within a radius of double its own length, a supposition which may be considered to stand in need of confirmation by further experience.

In stormy weather, when accidents are most likely to occur, a ship may carry away a mast, and if not fitted with a conductor to each, might possibly be exposed at a moment when protection was most needed.

Professors Faraday and Wheatstone were of opinion, "that extreme cases should be provided for," and we would therefore recommend that the three masts as well as the bowsprit should be always fitted with the conductors.

While concluding our remarks on the mode of applying Mr. Harris's conductors, we beg to state to their Lordships, that though it is our decided conviction that no danger is to be feared from the assumed lateral explosion, yet if it be deemed advisable, for the sake of obviating any doubts which may still exist in the minds of some, we see no objection whatever to the copper plates on the fore-mast being placed on the fore part of the mast, whereby the mast itself will intervene between the conductors and the magazine.

Having now completed our remarks on the several points to which their Lordships' instructions directed our attention, we trust we have shewn, from the evidence of facts derived from the experience of many years, as well as by the opinions not only of scientific, but professional, men, the efficacy of Mr. Harris's lightning conductors; and considering the number of lives which have been lost by lightning; the immense amount of property which has been destroyed, as shewn by Mr. Harris, and is still exposed without adequate protection; the inconvenience which has arisen, and is still liable to arise, from the loss of the services of ships at moments of great critical importance; the difficulty of procuring new spars in times of war on foreign stations, (not to mention the great expense of wages and victuals for the crews of ships while rendered useless till repaired; we again beg to state our unanimous opinion of the great advantages possessed by Mr. Harris's conductors above every other plan, affording permanent security at all times, and under all circumstances, against the injurious effects of lightning, effecting this protection without any nautical inconvenience or scientific objection whatever, and we therefore most earnestly recommend their general adoption in the Royal Navy,

We have &c. (signed)
A. M. Griffiths, Rear-Admiral.
Jas. A. Gordon, Rear-Admiral.
Jas. Clarke Ross, Captain.
J. F. Daniell, Professor of Chemistry.
Jno. Fincham, Master Shipwright.

II. *A Letter to Professor Faraday, on certain Theoretical Opinions.* By R. Hare, M. D., Professor of Chemistry in the University of Pennsylvania.*

Dear Sir,

I have been indebted to your kindness for several pamphlets comprising your researches in electricity, which I have perused with the greatest degree of interest.

You must be too well aware of the height at which you stand, in the estimation of men of science, to doubt that I entertain with diffidence, any opinion in opposition to yours. I may say of you as in a former instance of Berzelius, that you occupy an elevation inaccessible to unjustifiable criticism. Under these circumstances, I hope that I may, from you, experience the candor and kindness which were displayed by the great Swedish chemist in his reply to my strictures on his nomenclature.

---

* Communicated by the Author.

I am unable to reconcile the language which you hold in paragraph 1615, with the fundamental position taken in 1155. Agreeably to the latter, you believe ordinary induction to be the action of *contiguous* particles, consisting of a species of polarity, instead of being an action of either particles or masses at "*sensible distances.*" Agreeably to the former, you conceive that "assuming a perfect vacuum was to intervene in the course of the line of inductive action, it does not follow from this theory that the line of particles on opposite sides of such a vacuum would not act upon each other." Again, supposing "it possible for a positively electrified particle to be in the centre of a vacuum an inch in diameter, nothing in my present view forbids that the particle should act at a distance of half an inch on all the particles forming the disk of the inner superficies of the bounding sphere."

Laying these quotations before you for reconsideration, I beg leave to inquire how a positively excited particle, situated as above described, can react "inductrically" with any particles in the superficies of the surrounding sphere, if this species of reaction require that the particles between which it takes place be contiguous. Moreover if induction be not "an action either of particles or masses at *sensible* distances," how can a particle situated as above described, "*act at the distance of half an inch on all the particles forming the disk of the inner superficies of the bounding sphere?*" What is a sensible distance, if half an inch is not?

How can the force thus exercised obey the "well known law of the squares of the distances," if as you state (1375) the rarefaction of the air does not alter the intensity of the inductive action? In proportion as the air is rarefied, do not its particles become more remote?

Can the ponderable particles of a gas be deemed contiguous in the true sense of this word, under any circumstances? And it may be well here to observe, that admitting induction to arise from an affection of intervening ponderable atoms, it is difficult to conceive that the intensity of this affection will be inversely as their number as alleged by you. No such law holds good in the communication of heat. The air in contact with a surface at a constant elevation of temperature, such for instance as might be supported by boiling water, would not become hotter by being rarefied, and consequently could not become more efficacious in the conduction of heat from the heated surface to a colder one in its vicinity.

As soon as I commenced the perusal of your researches on this subject, it occurred to me that the passage of electricity through a vacuum, or a highly rarefied medium, as demonstrated by various experiments, and especially those of Davy, was inconsistent with the idea that ponderable matter could be a necessary agent in the process of electrical induction. I therefore inferred that your efforts would be primarily directed to a re-examination of that question.

If induction, in acting through a vacuum, be propagated in right lines, may not the curvilinear direction which it pursues, when passing through "dialectrics," be ascribed to the modifying influence which they exert?

If, as you concede, electrified particles on opposite sides of a vacuum can act upon each other, wherefore is the received theory of the mode in which the excited surface of a Leyden jar induces in the opposite surface, a contrary state, objectionable?

As the theory which you have proposed, gives great importance to the idea of polarity, I regret that you have not defined the meaning which you attach to this word. As you designate that to which you refer, as a "species of polarity," it is presumable that you have conceived of several kinds with which ponderable atoms may be endowed. I find it difficult to conceive of any kind which may be capable of as many degrees of intensity as the known phenomena of electricity require; especially according to your opinion that the only difference between the fluid evolved by galvanic apparatus and that evolved by friction, is due to opposite extremes in quantity and intensity; the intensity of electrical excitement producible by the one, being almost infinitely greater than that which can be produced by the other. What state of the poles can constitute quantity—what other state of intensity, the same matter being capable of either electricity, as is well known to be the fact? Would it not be well to consider how, consistently with any conceivable polarization, and without the assistance of some imponderable matter, any great difference of intensity in inductive power, can be created?

When by friction the surface is polarized so that particles are brought into a state of constraint from which they endeavor to return to their natural state, if nothing be superadded to them, it must be supposed that they have poles capable of existing in two different positions. In one of these positions, dissimilar poles coinciding, are neutralized; while in the other position, they are more remote, and consequently capable of acting upon other matter.

But I am unable to imagine any change which can admit of gradations of intensity, *increasing* with remoteness. I cannot figure to myself any reaction which increase of distance would not lessen. Much less can I conceive that such extremes of intensity can be thus created, as those of which you consider the existence as demonstrated. It may be suggested that the change of polarity produced in particles by electrical inductions, may arise from the forced approximation of reciprocally repellent poles, so that the intensity of the inductive force, and of their effort to return to their previous situation, may be susceptible of the gradation which your electrical doctrines require. But could the existence of such a repellent force be consistent with the mutual cohesion which appears almost universally to be a property of ponderable particles? I am aware that, agreeably to the ingenious hypothesis of Mossotti, repulsion is an inherent property of the particles which we call ponderable; but then he assumes the existence of an imponderable fluid to account for cohesion; and for the necessity of such a fluid to account for induction it is my ultimate object to contend. I would suggest that it can hardly be expedient to ascribe the phenomena of electricity to the polarization of ponderable particles, unless it can be shewn that if admitted, it would be competent to produce all the known varieties of electric excitement, whether as to its nature or energy.

If I comprehend your theory, the opposite electrical state induced on one side of a coated pane, when the other is directly electrified, arises from an affection of the intervening vitreous particles, by which a certain polar state caused on one side of the pane, induces an opposite state on the other side. Each vitreous particle having its poles severally in opposite states, they are arranged as magnetized iron filings in lines; so that alternately opposite poles are presented in such a manner that all of one kind are exposed at one surface, and all of the other kind at the other surface. Agreeably to this or any other imaginable view of the subject, I cannot avoid considering it inevitable that each particle must have at least two poles. It seems to me that the idea of polarity requires that there shall be in any body possessing it, two opposite poles. Hence you correctly allege that agreeably to your views it is impossible to charge a portion of matter with one electric force without the other. (*See par.* 1177.) But if all this be true, how can there be a "positively excited particle?" (*See par.* 1616.) Must not every particle be excited negatively, if it be excited positively? Must it not have a negative, as well as a positive pole?

I cannot agree with you in the idea that consistently with the theory which ascribes the phenomena of electricity to one fluid, there can ever be an isolated existence either of the positive or negative state. Agreeably to this theory, any excited space, whether minus or plus, must have an adjoining space relatively in a different state. Between the phenomena of positive and negative excitement there will be no other distinction than that arising from the direction in which the fluid will endeavor to move. If the excited space be positive, it must strive to flow outward; if negative, it will strive to flow inward. When sufficiently intense, the direction will be shewn by the greater length of the spark, when passing from a small ball to a large one. It is always longer when the small ball is positive, and the large one negative, than when their positions are reversed.*

But for any current it is no less necessary that the pressure should be on one side comparatively minus, than that on the other side, it should be comparatively plus; and this state of the forces must exist whether the current originates from a hiatus before, or from pressure behind. One current cannot differ essentially from another, however they may be produced.

In paragraph 1330, I have been struck with the following query, "What then is to separate the principle of these extremes, perfect conduction and perfect insulation, from each other; since the moment we leave the smallest degree of perfection at either extremity, we involve the element of perfection at the opposite ends?" Might not this query be made with as much reason in the case of motion and rest, between the extremes of which there is an infinity of gradations? If we are not to confound motion with rest, because in proportion as the former is retarded, it differs less from the latter; wherefore should we confound insulation with conduction, because in proportion as the one is less efficient, it becomes less remote from the other?

In any case of the intermixture of opposite qualities, may it not be said in the language which you employ "the moment we leave the element of perfection at one extremity, we involve the element of perfection at the opposite." Might it not be said of light and darkness, or of opaqueness and translucency; in which case to resort to your language* again, it might be added "especially as we have not in nature, a case of perfection at one extremity or the other." But if there be not in nature, any two bodies of which one possesses the property of perfectly resisting the passage of electricity, while the other is endowed with the faculty of permitting its passage without any resistance; does this affect the propriety of considering the qualities of *insulation* and conduction in the abstract, as perfectly distinct, and inferring that so far as matter may be endowed with the one property, it must be wanting in the other?

---

* See vol. 1. p. 489, of these Annals.

Have you ever known electricity to pass through a pane of sound glass? My knowledge and experience create an impression that a coated pane is never discharged through the glass unless it be cracked or perforated. That the property by which glass resists the passage of electricity, can be confounded with that which enables a metallic wire to permit of its transfer, agreeably to Wheatstone's experiments, with a velocity greater than that of the solar rays, is to my mind inconceivable.

You infer that the residual charge of a battery arises from the partial penetration of the glass by the opposite excitements. But if glass be penetrable by electricity why does it not pass through it without a fracture or perforation?

According to your doctrine, induction consists "in a forced state of polarization in contiguous rows of the particles of the glass" (1300); and since this is propagated from one side to the other, it must of course exist equally at all depths. Yet the partial penetration suggested by you, supposes a collateral affection of the same kind, extending only to a limited depth. Is this consistent? Is it not more reasonable to suppose that the air in the vicinity of the coating gradually relinquishes to it a portion of free electricity, conveyed into it by what you call "*convection.*" The coating being equally in contact with the air and glass, it appears to me more easy to conceive that the air might be penetrated by the excitement, than the glass.

In paragraph 1300, I observe the following statement: "*When a Leyden Jar is charged, the particles of the glass are forced into this polarized and constrained condition by the electricity of the charging apparatus. Discharge is the return of the particles to their natural state, from their state of tension, whenever the two electric forces are allowed to be disposed of in some other direction.*" As you have not previously mentioned any particular direction in which the forces are exercised during the prevalence of this constrained condition, I am at a loss as to what meaning I am to attach to the words "some other direction." The word *some*, would lead to the idea that there was an uncertainty respecting the direction in which the forces might be disposed of; whereas it appears to me that the only direction in which they can operate, must be the opposite of that by which they have been induced.

The electrified particles can only "return to their natural state" by retracing the path by which they departed from it. I would suggest that for the words "*to be disposed of in some other direction*," it would be better to substitue the following, "*to compensate each other by an adequate communication.*"

Agreebly to the explanation of the phenomenon of coated electrics afforded in the paragraph above quoted (1300), by what process can it be conceived that the opposite polarization of the surfaces can be neutralized by conduction through a metallic wire? If I understand your hypothesis correctly, the process by which the polarization of one of the vitreous surfaces in a pane produces an opposite polarization in the other, is precisely the same as that by which the electricity applied to one end of the wire extends itself to the other end.

I cannot conceive how two processes severally producing results so diametrically opposite as insulation and conduction, can be the same. By the former, a derangement of the electric equilibrium may be permanently sustained, while by the other, all derangement is counteracted with a rapidity almost infinite. But if the opposite charges are dependent upon a polarity induced in contiguous atoms of the glass, which endures so long as no communication ensues between the surfaces; by what conceivable process can a perfect conductor cause a discharge to take place, with a velocity at least as great as that of the solar light? Is it conceivable that all the lines of "contra-induction" or depolarization can concentrate themselves upon the wire from each surface so as to produce therein an intensity of polarization proportioned to the concentration; and that the opposite forces resulting from the polarization are thus reciprocally compensated? I must confess, such a concentration of such forces or states, is to me difficult to reconcile with the conception that it is at all to be ascribed to the action of the rows of *contiguous ponderable particles.*

Does not your hypothesis require that the metallic particles, at opposite ends of the wire, shall in the first instance be subjected to the same polarization as the excited particles of the glass; and that the opposite polarizations, transmitted to some intervening point, should thus be mutually destroyed, the one by the other? But if discharge involves a return to the same state in vitreous particles, the same must be true in those of the metallic wire. Wherefore then are these dissipated, when the discharge is sufficiently powerful? Their dissipation must take place either while they are in the state of being polarized, or in that of returning to their natural state. But if it happen when in the first mentioned state, the conductor must be destroyed before the opposite polarization upon the surfaces can be neutralized by its intervention. But if not dissipated in the act of being polarized, is it reasonable to suppose that the metallic particles can be sundered by returning to their *natural state* of depolarization?

Supposing that ordinary electrical induction could be satisfactorily ascribed to the reaction of ponderable particles, it cannot, it seems to me, be pretended that magnetic and electro-magnetic induction is referable to this species of reaction. It will be admitted that the Faradian currents do not for their production require intervening ponderable atoms.

From a note subjoined to page 37 of your pamphlet, it appears that "on the question of the existence of one or more imponderable fluids as the cause of electrical phenomena, it has not been you intention to decide." I should be much gratified if any of the strictures in which I have been so bold as to indulge, should contribute to influence your ultimate decision.

It appears to me that there has been an undue disposition to burden the matter, usually regarded as such, with more duties than it can perform. Although it is only with the properties of matter that we have a direct acquaintance, and the existence of matter rests upon a theoretic inference that since we perceive properties, there must be material particles to which those properties belong; yet there is no conviction which the mass of mankind entertain with more firmness than that of the existence of matter in that ponderable form, in which it is instinctively recognized by people of common sense. Not perceiving that this conviction can only be supported as a theoretic deduction from our perception of the properties; there is a reluctance to admit the existence of other matter, which has not in its favor the same instinctive conception, although theoretically similar reasoning would apply. But if one kind of matter be admitted to exist because we perceive properties, the existence of which cannot be otherwise explained, are we not warranted, if we notice more properties than can reasonably be assigned to one kind of matter, to assume the existence of another kind of matter?

Independently of the considerations which have heretofore led some philosophers to suppose that we are surrounded by an ocean of electric matter, which by its redundancy or deficiency, is capable of producing the phenomena of mechanical electricity, it has appeared to me inconceivable that the phenomena of galvanism and electro-magnetism, latterly brought into view, can be satisfactorily explained without supposing the agency of an intervening imponderable medium by whose subserviency the inductive influence of currents or magnets is propagated.* If in that wonderful reciprocal reaction between masses and particles, to which I have alluded, the polarization of condensed or accumulated portions of intervening imponderable matter, can be brought in as a link to connect the otherwise imperfect chain of causes; it would appear to me a most important instrument in lifting the curtain which at present hides from our intellectual vision, this highly important mechanism of nature.

Having devised so many ingenious experiments tending to show that the received ideas of electrical induction are inadequate to explain the phenomena without supposing a modifying influence in intervening ponderable matter, should there prove to be cases in which the results cannot be satisfactorily explained by ascribing to them ponerable particles, I hope that you may be induced to review the whole ground, in order to determine whether the part to be assigned to contiguous ponderable particles, be not secondary to that performed by the imponderable principles by which they are surrounded.

But if galvanic phenomena be due to ponderable matter, evidently that matter must be in a state of combination. To what other cause than an intense affinity between it and the metallic particles with which it is associated, can its confinement be ascribed consistently with your estimate of the enormous quantity which exists in metals? If "a grain of water or a grain of zinc, contain as much of the electric fluid as would supply eight hundred thousand charges of a battery containing a coated surface of fifteen hundred square inches," how intense must be the attraction by which this matter is confined? In such cases, may not the material cause of electricity be considered as latent, agreeably to the suggestion of Œrsted, the founder of electro-magnetism? It is in combination with matter, and only capable of producing the appropriate effects of voltaic currents when in act of transfer from combination with one atom to another; this transfer being at once an effect and a cause of chemical decomposition, as you have demonstrated.

---

* This view is precisely that which we have given at page 270, vol. 1, of these Annals; and, consequently, in strict conformity with the theory of electro-magnetism and magnetic electricity, which we have there explained. We are glad to find an acknowledgement of the principles of our theory from so eminent an experimental philosopher as Dr Hare; it gives us some hopes that it will soon be more generally acknowledged as affording the simplest and most natural mode of explaining electro-magnetic and magnetic-electrical phenomena, that has hitherto been offered to the notice of philosophers.—Edit.

If polarization, in any form, can be conceived to admit of the requisite gradations of intensity, which the phenomena seem to demand; would it not be more reasonable to suppose that it operates by means of an imponderable fluid existing throughout all space, however devoid of other matter? May not an electric current, so called, be a progressive polarization of rows of the electric particles, the polarity being produced at one end and destroyed at the other incessantly, as I understood you to suggest in the case of contiguous ponderable atoms.

When the electric particles within different wires are polarized in the same tangential direction, the opposite poles being in proximity, there will be attraction. When the currents of polarization move oppositely, similar poles coinciding, there will be repulsion. The phenomena require that the magnetized or polarized particles should be arranged as tangents to the circumference, not as radii to the axis. Moreover, the progressive movement must be propagated in spiral lines in order to account for rotatory influence.

Between a wire which is the mean of a galvanic discharge, and another not making a part of a circuit, the electric matter which intervenes may, by undergoing a polarization, become the medium of producing a progressive polarization in the second wire moving in a direction opposite to that in the inducing wire; or in other 'words an electrical current of the species called Faradian may be generated.

By progressive polarization in a wire, may not stationary polarization, or magnetism be created; and reciprocally by magnetic polarity, may not progressive polarization be excited?

Might not the difficulty, above suggested, of the incompetency of any imaginable polarization to produce all the varieties of electrical excitement which facts require for explanation, be surmounted by supposing intensity to result from an accumulation of free electric polarized particles, and quantity from a still greater accumulation of such particles, polarized in a latent state or in chemical combination?

There are, it would seem, many indications in favour of the idea that electric excitement may be due to a forced polarity, but in endeavouring to define the state thus designated, or to explain by means of it the diversities of electrical charges, currents and effects, I have always felt the incompetency of any hypothesis which I could imagine. How are we to explain the insensibility of a gold leaf electroscope, to a galvanized wire, or the indifference of a magnetic needle to the most intensely electrified surfaces?

Possibly the Franklinian hypothesis may be combined with that above suggested, so that an electrical current may be constituted of an imponderable fluid in a state of polarization, the two electricities being the consequence of the position of the poles or their presentation. Positive electricity may be the result of an accumulation of electric particles, presenting poles of one kind; negative, from a like accumulation of the same matter with a presentation of the opposite poles, inducing of course an opposite polarity. The condensation of the electric matter, within ponderable matter, may vary in obedience to a property analagous to that which determines the capacity for heat, and the different influence of dialectrics upon the process of electrical induction may arise, from this source of variation.

With the highest esteem, I am yours truly,
ROBERT HARE.

III.—*Description of a New Electro-tome.* By THOMAS WRIGHT, Esq. In a Letter to the Editor.

Sir,

I have lately contrived a self-acting Electro-tome, an account of which I send you herewith, in hope that you may think it worthy of insertion in your valuable journal.

I am, Sir,
Yours respectfully,
THOMAS WRIGHT.

Knutsford, 24th March, 1840.

In fig. 1. pl. 1, A is the end of an electro-magnetic coil, to which it is desired to apply the Electro-tome, B is a block of wood morticed into the foot-board of the machine, through which pass the small brass bars C D and E F; at D is soldered a copper wire, which is bent round the end of the bar E F, so as to form a spring pressing on the same at G; to the end of the spring at G is soldered a piece of tinned iron H, one inch broad and three inches long, bent in a U form as shewn at I. On the bar C D is a binding screw at D′, and one end of the primal wire of the coil is soldered to E.

To put the machine in action, one of the poles of a battery is screwed into D, and the other to the unattached end of the primal wire; by this means the current passes along D G E, through the coil, and out at L, magnetizing the core of iron wires in the coil, these immediately attract the piece of tinned iron H, and thus contact is broken at G with inconceivable rapidity: indeed, when the spring is bent close, and the magnet is approximated very nearly to the wires, the motion of the break produces a continued loud hum; the shocks however are not so strong as when the magnet is further from the break, and more time thus allowed for the developement of magnetism in it.

The electro-magnet which I employ is formed of a bundle of annealed iron wires, two feet long and about half an inch in diameter; over this is coiled forty yards of copper wire, 1-10th of an inch thick, (this is too thick when a small battery is intended to be used,) and over this, soldered to it, and forming one continuous coil, are wrapped 100 yards of copper wire, 1-15th of an inch thick; the battery connexions are made with the ends of the primal or thick wires; the shocks and decompositions are obtained from the whole length.

The primal current of this magnet gives a strong shock; (even with three square inches of zinc in the battery described hereafter;) the shock from the whole length is however intolerable, when the ends of the wires are touched with small pieces of tinned iron held between the dry tips of the finger and thumb of each hand; and when the cylindrical conductors attached to the machine are held loosely with dry hands, a continued crackling is heard, as if the electric fluid was passing through the thin space of air between the conductors and the hands.*

Mercury must not on any account be used as part of the electro-tome to the above coil, as the greater part of the secondary current passes in the dense and *vaporous* spark, occasioned by the combustion of that metal.

Instead of copper in the battery, I use common tea-chest lead, which I find to answer better. It is prepared by cutting strips of it in the form of a feather, so as to offer a great number of points for the conduction of the electric fluid, these are then placed round the inside of a jar, and the ends turned over the rim, and bound in their places with a bright copper wire firmly twisted round them, to serve as negative conductor. The zinc and diaphragms as in Daniels, the solutions as in Mullin's batteries.

---

[*] Since the receipt of this letter, we have been favored with a sight of Mr. Wright's very neat instrument, at the Royal Victoria Gallery of Practical Science, Manchester; and can testify as to its powers fully answering to the description.—EDIT.

IV.—*Description of a New Electro-magnetic Machine* By THOMAS WRIGHT, Esq. In a Letter to the Editor.

Dear Sir,

The following description of an Electro-magnetic Machine will, I hope, prove interesting to the readers of your "Annals."

I am
Yours respectfully,
THOMAS WRIGHT.

Knutsford, March 28, 1840.

Fig. 2, gives a sectional view of the apparatus. It consists of two concentric cylinders of copper joined together at the bottom; the inner one A encloses an electro-magnetic coil, the end of the bobbin appearing at the top. The dotted lines represent concentric cylinders of porous earthenware, with a cylinder of zinc between them. To the cylinder A is soldered a wire at B, bearing a U shaped piece of tinned iron at its extremity, and pressing with a slight spring against C, which is a bent brass bar in conjunction with one end of the primal coil. D is the other end of the primal coil attached by a binding screw to the zinc; this is a modification of my self-acting electro-tome. The primal and thin coils are joined to form one coil: the end of the thin coil is attached to the binding screw on the bobbin at E. The shock is taken from thin spirals attached to C and E; sparks, &c, from wires attached to C and D.

Professor Henry has found that electric induction does not take place (or very imperfectly) when a closed circuit is interposed between the primal and superimposed coils; but I have not seen it remarked that magnetic induction follows the same law. The following experiment however seems to shew that it does:—

Being desirous of getting rid of the oxidizing spark which takes place on the breaking of the battery current, I joined the two brass bars of the electro-tome, described in my last letter with a fine iron wire two yards long,* in hopes that the secondary current, on account of its superior intensity, would pass *through it, though the battery current could not.* This *answered my expectation fully;* the break continued to work *as usual, and the spark disappeared entirely.* [This experiment was performed in the dark.]

---

* I am afraid I shall be thought pedantic if I call this wire the "deuterode," or "path of the secondary" current, but as I hope to make it a source of utility in another course of experiments, I have thought it advisable to give it a specific name.

I then took hold of the ends of the super-imposed coil, and was much surprised to find that it did not give the slightest shock, even when its extremities were placed in the mouth: thinking that the insulation must be imperfect, I disjoined the "denterode," the shocks were then as powerful as usual, but on joining it again they instantly ceased.

V.—*Description of an Electro-magnetic Engine.*—By Mr. Uriah Clarke. In a Letter to the Editor.

Sir,

Enclosed I send you (for publication in the Annals, if you please,) a drawing and description of, as I think, an original mode of applying voltaic agency for the purpose of acquiring motive power.. Among the varied forms of rotatory movements, hitherto made public, I believe the general principle has been to apply the magnetic action in a *lateral* manner. This circumstance, together with the very limited sphere of action of even the most powerful magnets, has, perhaps, been the cause why large machines and small ones have been so disproportioned in their effects, that the latter seem to furnish no data for calculating the power of the former.

In the machine which I am about to describe to you, the magnet is made to act in a *direct* manner upon a reciprocating bar, which bar communicates its motion to a crank, just at the most favorable point, viz., when it (the crank) is nearly in a horizontal position, and when the bar is making a close approach to the magnet; consequently, the most intense action of the magnet is applied to the crank in that part of its revolution where it is most effective. Now, a slight consideration of the principle of the crank will shew the importance of this object, which is obtained by joining the reciprocating bar to the crank, by means of a chain, or other flexile communication. (How far this principle may be applicable for maximizing the effect of a given power upon cranks generally, I leave with mechanics to determine—it is indispensable here.)—When the crank has moved below the horizontal position, the reciprocating bar falls upon a rest, which prevents any percussion taking place on the ends of the magnet. The bar remains upon the rest until the crank has made the lower part of its revolution, during which time contact with the battery is broken, and the bar, of course, is disengaged from the magnet; it (the bar) is then lifted by the crank, as it passes over the centre, when battery communication being again made, another impulse is given. It will, no doubt, occur to you, that a number of magnets may be used with a succession of cranks. I trust that figure 3, plate 1, will be sufficiently intelligible. A is the chain communicating between the bar and crank. B is the reciprocating bar, and C is the electro-magnet. The break is here omitted to avoid confusion. I effect it by means of a small eccentric upon the shaft of the fly-wheel. I would observe, in conclusion, that the above contrivance is not a mere embryo of the mind; for I have now a working model in actual operation, which I invented nearly two years ago. It has been at work occasionally for the last eighteen months, and, as I have made no secret of it, it has been seen by a great number of professional gentlemen and scientific friends, amongst whom I take the liberty of naming a friend and correspondent of yours, Mr. James Mitchell, of this place.

I am, Sir,
Very respectfully yours,
URIAH CLARKE.

Leicester, March 16th, 1840.

VI.—*On the new Metal Latanium.* By JAMES C. BOOTH AND CAMPBELL MORFIT.

[From the Journal of the Franklin Institute.]

A notice of the discovery of this element having already appeared in one of our Scientific Journals, it occurred to us that an account of some of our experiments with it might present a subject of sufficient interest to the readers of the Institute Journal.

The name is derived from the Greek λαντανειν, to lie hid;* it is called in Swedish and German, Lantan, but in English Latanium, for the sake of euphony and in accordance with the generally received termination of the names of the elements. The ordinary method of obtaining cerium by precipitation with the bisulphate of potassa, threw down a bi-salt of Latanium at the same time, the latter constituting two-fifths of the whole saline mass. The method of separating the two depends on the ready solubility of oxide of latanium in dilute acid after ignition, a property lost by cerium under the same circumstances. From its nitric solution, it may be best thrown down as a white, chrystaline carbonate, by carbonate of ammonia, and from this its other compounds may be formed. The dry chloride heated with potassium was reduced to a grey metallic powder possessing a dark lead-color, and capa-

* From its concealment hitherto in the compounds of cerium.

ble of being flattened together by pressing. It is slowly converted into oxide in the air, and in cold water into a hydrated oxide with the evolution of hydrogen. An effervescence takes place in hot water.

It has two isomeric states. The ordinary salts possess a faint reddish tinge, but when the yellowish red oxide is heated in hydrogen gas, it becomes white with a faint shade of green, and dissolves with more difficulty in acids, forming salts which possess a greenish hue.

With bisulphate of potassa it forms a slowly soluble salt, which, however, does not precipitate like the corresponding salt of cerium, unless the latter be also present in the solution. Its atomic weight is lower than that adopted for the oxide of cerium.

The above notice is mainly extracted from Berzelius' letter to Poggendorff, published in Nos. 4 and 5 of Poggend. Annals for the present year. Our experiments were as follows:

Having prepared the sulphate of cerium and potassa by the ordinary methods from the mineral cerite, it was dissolved in a large quantity of boiling water, and the hydrated oxides of cerium and latanium precipitated by caustic potassa. These were dissolved in nitric acid after being thoroughly washed, evaporated to dryness, and heated in a platinum crucible until all the nitric acid was expelled. The oxides remained of a light reddish brown color, and were transferred to a glass containing nitric acid diluted with 60 to 80 times as much water. After digesting about two hours in a gentle warmth, the latanium was dissolved and oxide of cerium remained of a reddish-brown color. The solution treated with caustic potassa threw down the white hydrated oxide of latanium, much more bulky and gelatinous in appearance than alumina. It is exceedingly difficult, if not impossible, to wash it out thoroughly, for after edulcoration for several days, the liquid passing through the filter still gave indications of a solid matter, and almost led to the belief that the oxide was slightly soluble in water.

On re-dissolving hydrated oxide in nitric acid, evaporating to dryness, and heating to redness, the dry oxide remained of a brick-red color, differing therefore from the oxide of cerium by a lighter hue, and by containing less of a brownish shade. On treating this oxide as before with very dilute nitric acid, a small portion of oxide of cerium remained, proving that this mode of separating the two metals is not accurate, and that we must await further experiments for the discovery of a more perfect method.

Carbonate of latanium, as thrown down by carbonate of soda, is a voluminous white precipitate, and, like the hydrated oxide, very difficult of edulcoration, for after obtaining the chloride from it, crystals of common salt were also visible. Agreeably to the observations of Mosander, therefore, the carbonate of ammonia is the best precipitant.

Sulphate of latanium is readily formed by the solution of the oxide, or carbonate, in dilute sulphuric acid, evaporation to a small bulk by heat and exposure to self-evaporation, while delicate needles of a flesh-red color collect in little groups on the bottom of the capsule.

The chloride is similarly formed by means of chloro-hydric acid and evaporation. It forms a light yellowish green crystaline mass, in which no determinate form was observed.

The quantity of latanium in our possession was so small, amounting only to a few grains, that the operations were necessarily conducted slowly, and prevented our pursuing them quantitatively. Should we be enabled to obtain a larger amount, we may give more interesting results, without, however, trespassing on the field legitimately belonging to the discoverer.

VII.—*Remarks on the Imperfect Elasticity of Glass Threads used in Torsion Instruments.* By W. H. Goode, Chemical Assistant in the Laboratory of the University of the City of New York.

[From the Journal of the Franklin Institute.]

From the superior elasticity which glass enjoys over most substances, particularly the metals, threads of this material have replaced the metallic wires with which the needles of torsion instruments were at first suspended. The late Dr. Ritchie employed them in his improved torsion galvanometer, which emulates the torsion balance of Coulomb, for accuracy in measuring small forces. It is necessary, however, to the perfection of torsion instuments, that the elasticity of the thread of suspension, though a feeble, should be a constant force; and also that the thread itself should suffer no alteration of its conditions by the amount of force exerted upon it. Perfectly elastic substances only, fulfil these requirements; on all imperfectly elastic bodies torsion acts irregularly, and impresses a change upon them which prevents their return to their normal position, after it has been removed; such, for example, is the change effected in the conditions of metallic wires, filaments of silk, hair, &c., which, being imperfectly elastic, fail to return to zero when released from torsion.

The impression has become general that threads of glass, of a certain degree of tenuity, unlike the substances already mentioned, are not permanently affected by force exerted on them; but are capable always of regaining their original position; their elasticity is therefore considered to be perfect,—and measures effected with instruments to which they are adjusted, rigidly accurate. Having observed that the needle of a galvanometer suspended by a glass thread did not return to zero, after the instrument had been employed in a series of observations, it became a subject of enquiry to ascertain the source of error. For this purpose, threads of different diameters were suspended, in a manner similar in all respects, to that of a torsion balance. Two small uprights were placed on opposite sides of the little glass needle, near its opposite ends, which served as obstacles to it when torsion was made, and prevented it from rotating along with the micrometer. On some point of the thread, another little glass needle, carrying an upright arm, was cemented. This arm served as an index for the thread, and marked its position on a scale pasted on the opposite side of the, jar; it was observed through a small hole in a plate of metal, placed ten or fifteen inches distant;—an improved method of observing a vertical index introduced by Professor Draper.

The thread being freely suspended, its index and that of the micrometer at their respective zeros, a torsion of three revolutions of the micrometer was exerted on the thread for five minutes; when released from this force it did not resume its position at zero, but varied from it in the direction opposite to that in which the torsion had been made. If the micrometer be now kept at zero, it will be found that the thread partially recovers itself, after the lapse of several hours; the amount of error is consequently diminished, but it never returns accurately to its original position. If instead of releasing the thread by returning the micrometer to zero, the thread be released from the obstacles, the same result will be obtained; in consequence of the impetus it requires in spinning round, this latter is probably the less accurate method of observation.

A variety of experiments were performed with different threads, to ascertain if the alteration of the zero bore any constant relation to the amount of torsion employed. If such were the case, a system of compensation could be adopted which would free instruments fitted with glass threads from error.

The result of the observations made with one thread are given, for convenience, in a tabular form; they are analogous to those obtained with others; and indicate that the amount of the alteration of the zero, for the same threads, for different degrees of torsion, bears no constant proportion to the amount of force employed.

TABLE I.

	No. of degrees of Torsion.	Error of Zero.	Duration of Torsion.	Thermometer
1	360°	2°	5′	70°
2	720°	10°	5′	70°
3	1080°	13°	5′	70°
4	1440°	20°	5′	70°

The thermometer was carefully noted in these experiments, lest the temperature of the room should vary during the prosecution of them; for it is not known what influence changes of temperature may exert on the elasticity of glass. After a lapse of twelve hours the thread had not returned to zero: another zero being assumed the following table of errors was obtained;

TABLE II.

	No. of degrees of Torsion.	Error.	Duration of Torsion.	Thermometer
1	360°	2°	5′	70°
2	360°	3°	5′	70°
3	360	3°	5′	70°
4	720°	6°	5′	70°
5	1080°	9°	5′	70°
6	1440°	12°	5′	70°

The thread was now left freely suspended for two hours; another needle and index were then adjusted to it, parallel to the first, but at twice the distance of the first from the micrometer: two points of the thread could now be observed and the effect of torsion on a double length noted. Up to 1080 the error of its two indexes was the same in amount; for that degree of torsion, the farther index varied more from its zero than the nearer, and consequently required the micrometer to pass through a greater space to restore it to that point. For a torsion of 1440° the error of the more distant index from the micrometer exceeded that of the nearer, three degrees. The tabular results are as follows;

TABLE III.

	No. of degrees of Torsion.	Error.	Duration of Torsion.	Thermometer
1	360°	3°	5′	70°
2	720°	6°	5′	70°
3	1080°	6°	5′	70°
4	1440°	*	5′	70°

* Upper Index 5°. Lower one 8°.

The result indicated in the last experiment of the preceding table, has been observed to take place repeatedly when high degrees of torsion have been employed and continued for a long period. It is probably due to slight inequalities in the diameter of the threads and to their being differently annealed in different portions of their length; force exerted on them would, under such circumstances, produce more decided effects on some portions than on others, and the effort made by the thread to recover itself would also occasion in different parts of it, different degrees of error.

An inspection of these tables will shew that the whole amount of error, for each series of observations, is decreasing; in the first it amounted 20°, in the second to 12°, in the third to 6°. It would therefore appear that a certain amount of torsion could be exerted on the thread for a certain period, for which it would afford no error of the zero. One thread which has been operated with, exhibited this effect of force exerted on it, in a very marked manner; for a torsion of two revolutions of the micrometer, its zero altered 10°, but for four revolutions there was no alteration. It was broken in making another observation.

For degrees of torsion less than that at which this effect takes place, the error of the zero will continue to exist; as it is observed, in the two last tables that for one and two revolutions of the micrometer, the error is nearly constant.

It has been shewn that a force amounting to one revolution of the micrometer exerted for five minutes, produced a permanent deflection of the zero of the thread, two degrees. An attempt was made to ascertain the influence of smaller degrees of force on the thread, exerted for a longer period. A torsion of 270 produced, in five minutes, a permanent deviation of the zero 1¾°; 180° of torsion in the same period occasioned no error. A torsion of 90° continued for half an hour, produced an alteration of 2 ; for 45° of torsion continued one and a half hours, it altered 3°. The permanent alteration of the zero of a glass thread appears to be occasioned, either by a large amount of force exerted upon it, or by smaller forces acting for a comparatively long period. The amount of this alteration is modified by a variety of circumstances; the diameter of the thread—the uniformity with which it cooled in being drawn—the amount of force to which it has been subjected, and its duration—combine in producing this effect; which, influenced by so many causes, must necessarily be variable in amount.

It is not probable that any two threads will afford the same numerical value of their respective errors of zero, for the same force exerted on them. We cannot be certain that they are in precisely the same condition—that they are identical in composition, or that they are equally well annealed. Both, however, will fail to return to zero after a certain amount of torsion has been exerted on them, by a quantity which diminishes for every series of observations; and which for a certain amount of torsion, becomes nothing.

The effect of this error of the zero on instruments constructed for the purpose of measuring small forces by torsion, is to cause the deviations of the needle, when the thread is newly suspended to be less than they ought to be, and less than they ever are afterwards; in each succeeding observation the force acting on the needle has to overcome not only the forces which keep it in equilibrio, but that also which deflects the thread from its zero; as this latter increases with the increase of torsion and with the time it occupies, the last deflections of the needle are much less than the first of the same series.

This kind of error will be found in every series of experiments, though it will vary in amount; but it will still be sufficient if not corrected for, to vitiate results requiring to be obtained accurately. In using torsion instruments, after each observation, the error of the zero ought to be ascertained and the due correction applied. But where it can be done, it is advisable first to ascertain the amount of torsion necessary in five minutes to cause an alteration of the zero; and then to ascertain the period of time which that amount of torsion intended to be employed requires to produce the same effect, and not transcend these limits, by employing a greater amount of force on the thread, or using the instrument continuously for a longer time than it will afford correct measures.

VIII.—*On the Course of the Electrical Discharge, and on the Effects of Lightning on certain Ships of the British Navy, &c. &c.* By W. Snow Harris, Esq. F.R.S.

In the instance I last quoted of damage to H.M.S. Rodney by lightning, it will be remembered that there was no regular metallic line through which the forces in action could become neutralized. The electrical agency had therefore to find for itself such a general course, as upon the whole opposed the least resistance to its progress; and it is evident that in this case its path was determined on the general principles before laid down in sec. 17.

25. I shall now proceed to state a few cases of damage to certain other ships of the navy, where metallic bodies happened to be so disposed about the rigging and hull, as to approximate in some measure to the conditions of experiment 2, sec. 18,* and consequently to that perfect state of defence against the expansive force of the electrical discharge in which a ship would become placed, by perfecting the conducting power of the masts, and uniting them into one general continuous system with the metallic masses in the hull, and with the sea.

These cases are particularly interesting, and conclusive of the general question of the protection to be afforded by such a system.

No. 1.—In September 1833, H.M. ship Hyacinth had both the fore and main-top masts and top-gallant masts destroyed by lightning in the Indian Ocean. The electric fluid shivered these masts from the truck to the heel of the topmast, as indicated by the waving black line *a b* in fig. 4. pl. I. which represents the effects on the main mast; at the point *b*, it became assisted by the chain topsail sheet leading to the deck at *c*, and so did no further damage to the mast; at *d* it received further assistance from the copper pipe of Hearle's patent pump, leading to a small well at *e*, and thence by a second pipe through the ship's side under water, and by this passed safely into the sea.†

26. Now it is evident here that a heavy discharge of lightning which shivered completely a sloop of war's main-top mast and top-gallant mast varying from 11 inches to a foot in dia-

---

* Annals of Electricity, &c. Vol. iv. p. 492.

† These circumstances are minutely detailed by Capt. Blackwood, who commanded the ship at the time, and may be seen in his interesting letter on the subject, in the Nautical Magazine, vol. viii., p 116.

meter through a length of at least 80 feet, was conducted without damage or fusion by an iron chain and a short copper pipe. It is therefore important to state the dimensions of these metallic bodies. Now the iron chain consisted of links 2¼ inches long, made of iron rod ½ inch in diameter. It reached from the lower yard to the deck, a distance of about 50 feet.

The pump consisted of copper pipe 4 pounds to the square foot; it was 3 inches in diameter, and about the $\frac{1}{11}$th of an inch thick, extending through a distance of about 10 feet.

The effects on the foremast were very similar, they are omitted therefore for the sake of brevity.

27. It is not a little remarkable, that five years after this, in 1838, this same ship was again struck by lightning, whilst at anchor in Penang Bay, and again lost her main-top mast and top-gallant mast in a similar way, the lower mast being preserved by her chain topsail sheets.

28. No. 2.—In 1830, the Athol, of 28 guns, was struck by lightning on her foremast, in the Bight of Biaffra: at this time the topsails were lowered on the caps and the other sails furled, as shewed in fig. 5. This ship had chains for hoisting the topsails which lay in the direction of the topmast as indicated by the dotted line *b c*. She had also a chain for topsail sheets, which led along the lower masts as indicated by the line *d e*. When the electrical explosion fell on the truck it shivered the top-gallant mast in pieces so far as the commencement of the chain at *b*; here being assisted by the chain, it passed on *without* any damage to the topmast, which is extremely worthy of remark, because in the former case, where there was no chain, the topmast was destroyed.

Having reached the point *c*, where the chain terminated, it passed *with* damage over the head of the mast, until again being assisted by the lower chain *d e*, it passed *without* damage to the deck; on reaching the deck at *e*, it passed by means of a bolt through a beam in the forecastle upon the chain cable, and thence into the sea.*

29. These effects are similar to the former, and shew the protection afforded by the chains, and their power of conducting heavy discharges of lightning without any of the ill consequences insisted on by Mr. Sturgeon; since in both cases the chains were in the vicinity of large metallic masses, viz. the iron hoops, iron-bound blocks, &c. about the masts, and

---

* An interesting and authentic account of this circumstance will be found in the Nautical Magazine, vol. viii., p. 114.

in both cases the lightning passed through the hull. Now as all the laws of nature are general, not partial, it is reasonable to infer, that if Mr. Sturgeon's view of a lateral explosion were true, it ought to apply in such papable cases as these, more especially when he says he can produce a lateral explosion at 50 feet distance with a jar of only "a quart of capacity."

30. No. 3.—The effects of lightning on H.M.S. Snake, is another striking instance of the general laws we have been contending for. The phenomena are detailed with peculiar clearness by Capt. Milne in the March number of the Nautical Magazine. The electric fluid entered main truck, shivered royal mast, splintered top-gallant mast; then over *chain* main topsail tye *without* damage to within 8 feet of the deck so far as the topsail halliards.

Finding, as observed by Capt. Milne, an obstruction here in the ropes, it again seized on the mast, and became divided at the saddle of main boom; one portion passed out of quarter-deck port to the sea, the other to lower deck and down the mast, and distributed itself over the hull, affecting persons below. The mast, on being examined at Halifax, was sprung about the partners 2 inches deep and 15 inches round, and was *perfectly burst asunder at the step:* hence the shock had extended to the heel, the electric matter, consequently, must have passed by the metallic bolts in the keelson to the sea.

It is further stated, and it is *a most important fact,* that a seaman *aloft on the cross trees,* at the time, did not experience any sensation whatever.

31. No 4.—The Buzzard brigantine was struck by lightning on the Coast of Africa, in February 1838, and lost her top-gallant and topmast, under precisely the same circumstances as those of the Hyacinth, the lower mast being preserved by the chain topsail sheet*.

32. No. 5.—The Fox revenue cutter was struck by lightning in March 1818. The mast was furrowed and otherwise damaged in every part *except where it was coppered;* as appears by a minute made at the time by the master mast-maker at the Plymouth dock-yard.

Now the copper usually placed about a cutter's mast is not the $\frac{1}{32}$nd part of an inch in thickness. In this case it remained perfect.

33. No. 6.—The spire of a church at Kingsbridge in Devon-

---

* This case was given me by the commander Lieut. Fox. I was myself on board the vessel on her arrival. The particulars are noted in her log.

shire was struck by lightning in June 1828, and fearfully damaged. This case is particularly worthy of notice.

The lightning fell on an iron spill, *a*, *b*, fig 6, supporting the weather-cock, about 7 feet in length and 1 inch in diameter. On this it produced no visible effect, nor did *any damage arise to the stone-work about the rod.* It was not until the rod ceased at the point *b* that the masonry was rent.*

34. No. 7.—Extract from a letter from Lieut. Sullivan, of H.M.S. "Beagle," addressed to the Editor of the Annals of Electricity, &c. &c., relative to the protection afforded by a continuous conductor attached to the mast of H.M.S. Beagle.

"Having considered your communication in the Annals of Electricity on marine lightning conductors, containing observations on the stroke of lightning which fell on the masts of H.M.S. Beagle, I think it fair, both to Mr. Harris and the naval service, to describe the phenomena I witnessed on that occasion; first stating, that at the time of my joining the Beagle in 1831, previously to her leaving England, I had no acquaintance with Mr. Harris, and certainly *no bias* in favour of the conductors with which the ship was fitted. I may therefore claim to be considered an impartial observer.

"At the time alluded to, I was first Lieutenant of the Beagle, and was attending to the duty on deck. She was at anchor off Monte Video, in the Rio de la Plata, a part of the world very often visited by severe lightning storms. Having been on board H.M.S. Thetis at Rio Janeiro a few years before, when her *foremast was entirely destroyed by lightning*, my attention was always particularly directed to approaching electrical storms, and especially on the occasion alluded to, as the storm was unusually severe. The flashes succeeded each other in rapid succession, and were gradually approaching; and I was watching aloft for them when the ship was apparently wrapt in a blaze of fire, accompanied by a *simultaneous* crash, which was equal if not superior to the shock I felt in the Thetis; one of the clouds by which we were enveloped had evidently burst upon the vessel, and as the mainmast appeared for the instant to be in a mass of fire, I felt certain that the lightning had passed down the conductor on that mast; the vessel was shaken by the shock, and an unusual tremulous motion could be distinctly felt. As soon as I had recovered from the surprise of the moment, I ran down below to state what I saw, and to see if the conductors below had been affected; and just as I entered the gun-room, the purser,

---

* MS. letter with a drawing, dated July 11, 1828, from the Rev. G. F. Wise, late Vicar of Kingsbridge.

Mr. Rowlett, ran out of his cabin, (along the beam of which a main branch of the conductor passed) and said that he was sure that the lightning had passed down the conductor, for at the moment of the shock he heard a sound like rushing water passing along the beam. Not the slightest ill consequence was experienced; and I cannot refrain from expressing my conviction, that had it not been for the conductor the results would have been of very serious moment.

"This was not the only instance where we consider that the vessel had been saved from being damaged by lightning by Mr. Harris's conductors; and I believe that in saying I had the most perfect confidence in the protection which those conductors afforded us, I express the opinion of every officer and man in the ship.

"Not being sufficiently acquainted with electrical experiments, I cannot remark upon those you have adduced in support of your opinions detrimental to Mr. Harris's conductors.

"I can, therefore, only repeat my conviction that the Beagle was struck by lightning in the usual may, and certainly without any *lateral* explosion or other ill effects similar to those you insist on in your Annals of Electricity."

35. Now these facts are totally subversive of all Mr. Sturgeon has advanced concerning his destructive lateral explosion in the way of objection to the fixing conductors in ships' masts, and prove in the most conclusive manner the protecting power of such conductors: his statement, therefore, that "destructive lateral discharges will always take place when the vicinal bodies are capacious and near the primitive conductor or to any of its metallic appendages," is clearly fallacious.

36. I is allowed by writers on inductive science, that we wander from the true path of philosophical inquiry, and take up that of assumption and conjecture, directly we cease to verify our principles by an appeal to facts. In order to arrive at a general law of nature, it is requisite to examine carefully a great number of facts bearing directly on the question at issue, and shew, that the principle we assume is common to them all; for if in any case the assumed principle is decidedly negatived, it is at least a powerful exception; and it *may* be sufficient to overturn our whole theory.

If such exceptions are numerous, any theory which cannot include them is decidedly untenable.

It has been well observed by Abercrombie,* that in deducing a general principle, "when the deduction is made from a full examination of *all* the individual cases, and the general fact shewn to apply to them all, that is truth; when is it deduced from a small number of observations and extended to others to which it *does not apply*, this is falsehood."

---

* On the Intellectual Powers.

37. In applying these principles, we find Mr. Sturgeon's assumed lateral explosion decidedly negatived in all the cases just cited, since we do not find any such occur in the passage of heavy discharges of lightning along the masts, &c.; we do not find, as asserted by him, any thing like electrical waves produced by the discharge through a conductor situated close to the magazine. Thus in the case of the Hyacinth, No. 1. the copper pump *d e*, fig. 1, was a conductor near the after magazine. Yet the electric shock, in passing down this and through the ship's side, did not cause "intense sparks among the powder barrels, whose metallic linings and hoops reciprocally interchange them."*

38. Again, we do not find in the passage of a dense explosion of lightning that the sailors are necessarily subjected to lateral discharge, since in the case of the Snake, it may be observed that a seaman aloft on the cross-trees did not experience *any sensation* whatever, although the top-gallant mast was shivered, and a terrific shock darted from the heel of it to the chain topsail tye. Now if Mr. Sturgeon's views were practically sound, this man ought to have been killed on the spot by a "*lateral discharge*," as he says happened to a seaman called Wilson in the case of the Rodney.

39. Mr. Sturgeon, therefore, if he still adheres to his theory, is at last reduced to the necessity of supposing, that his lateral discharge may sometimes occur, and sometimes not, which is manifestly in the teeth of his own hypothesis. This instance just quoted of the little effect experienced by persons in the vicinity of heavy electrical discharges is by no means a solitary one, as the following extract from a letter from Admiral Hawker, with which he favoured me relative to the damage done to the Mignomne, very fully shews:—

"The circumstances of the Mignomne being struck by lightning were these: she had been on shore, and was going to Port Royal, Jamaica, attended by the Désirée; we had a day I think the hottest I ever experienced in the West Indies, without a cloud. After sunset we observed clouds rising up from every part of the horizon with thunder and lightning. I ordered the topsails to be lowered in case of squalls, and we ran down towards Port Royal: about midnight the heavens seemed to be one continued flame, and soon after the main topmast was shattered into probably fifty pieces, scattering the splinters in all directions; the mainmast was split down to the keelson, and a sulphurous smell came up from the hold, which occasioned some to cry out that the ship was on fire. Two men were killed in the main-top, being burnt black, and having some splinters sticking in them, and a man who was sleeping on the lower deck with his head on a bag (for the ship having been on the rocks for three days there were no hammocks) near the armourer's bench was found dead, with one black speck in his side; *another man sleeping by him was not hurt.*"

---

* Sturgeon's Memoir, Annals of Electricity, &c. vol. iv.

40. The number of instances in which dense explosions of lightning have passed very near to persons without causing any serious injury to them is remarkable.

Thus in the case of the Buzzard, No. 4, before mentioned; the explosion at the time of shivering the topmast passed so near to a seaman called Robert Purk, that it actually tore the shirt from his arm: he very kindly shewed me the shirt, and pointed out the place where he was standing. Lieut. Fox, who commanded this vessel, and who was good enough to send me an account of the damage, &c. sustained, says, in allusion to this circumstance, "The lightning took a strip out of the shirt about two inches wide from the shoulder to the wrist without hurting him."

No. 9.—In the instance of the Hawk cutter, lately struck by lightning on the west coast of Erris, snd scarcely damaged, it appears that the electric matter in passing down the main hatchway passed between a man and a boy. Neither were hurt; the latter experienced a shock only. It also passed close to another man lying across a hammock about the same spot, who jumped up and thought his neckhanderchief was one fire; the latter experienced a temporary effect only on his right arm.

41. All these cases evidently show, that no damage occurs from a shock of lightning *out of its direct path.* It may, however, divide in the absence of any good conducting course, and branch out into a variety of other courses (as already observed) and seize either wholly or partially upon bodies which happen to lie in certain points, as clearly shown in all these cases, and in the partial fusion of the leaf-gold given in experiment 2,* of my last communication.

We may also expect to find an *expansive* effect of greater or less force in the vicinity of a discharge of *free electricity* under the form of a dense spark, in *a bad conducting interval;* as observed by Dr. Priestley, "the air being suddenly displaced gives a concussion to all the bodies which happen to be near it."

---

* Annals of Electricity, &c. vol. iv. p 492.

42. It is clear therefore that in all cases where injury or death has occurred, as in those before given in the Mignonne, Rodney, &c., it has been the result of the passage of the electric agency, either wholly or partially, through the animal body, and not from the result of any *lateral explosion* of electricity, such as described by Mr. Sturgeon. If, as he says, such explosions in all cases of proximity to the primitive charge necessarily arise, such proximity to the passage of a dense shock of lightning would be in all cases fatal, which is evidently not the case.

43. I have now to consider briefly a few instances of the power of metallic bodies to transmit heavy discharges of lightning.

In the case above quoted of the Hyacinth, we observe, as already remarked, that a flash of lightning which shivered the top-mast and top-gallant mast passed over a small iron chain and copper tube without fusing either. A similar result ensued in the second instance of the Hyacinth being struck by lightning; also in the case of the Athol and Buzzard, and Snake, and in a great variety of others, too numerous to detail here.

In the case of the Fox, No. 5, it is seen that the shock of lightning which damaged the mast, was conducted without fusion or damage by sheet copper of $\frac{1}{12}$nd of an inch in thickness placed in the wake of the gaff. This is conclusive of the fallacy of Mr. Sturgeon's assertion, that any conductor applied to the mast would, under the operation of lightning, be "probably peeled from the wood."

In the case of the Kingsbridge spire, No. 6. The lightning which shivered the tower, fell on a cylindrical iron rod of an inch diameter without producing any effect on it.

In the case of the Rodney, the flash which set the top on fire and splintered the masts, was conducted by a short copper funnel for top-gallant rigging without fusion.

In the case of the Beagle, No. 7, a shock of lightning passed down the conductors without producing any effect on them.

No. 10 A house was struck at Tenterden; the lightning fell on an iron bar three-quarters of an inch square, but produced no effect on it.*

No. 11. A stroke of lightning fell on Mr. West's house, at Philadelphia, having a conductor terminating in a brass rod ten inches long and *a quarter of an inch in diameter;* only a few inches of the point were melted, but no damage occurred to the building.[*]

---

* Philosophical Transactions.

No. 12. On the 19th of April, 1827, one of the large New York packets, whilst in the Gulf Stream, was assailed by two most awful strokes of lightning twice in the same day. The first shock was productive of serious and destructive effects. The second shock fell on a pointed conductor subsequently hoisted to the main-mast head. This conductor consisted of an iron chain, having links of a quarter of an inch thick and two feet in length, and turned into hooks at each end, connected by rings of the same thickness, and one inch annular diameter. This conductor was *attached* to an iron rod placed at the mast head, half an inch thick and four feet long. The explosion fell in a *concentrated* form, and with an awful crash upon this rod. Although the small chain below was disjointed and some of the links fused, yet this pointed iron rod was only fused for a few inches. *The ship in the second case escaped danger.*

Now these are authenticated cases, and there are numerous others which I might adduce, to shew how perfectly *capacious* and *continuous* conductors transmit shocks of lightning.

44. No good instance can be adduced in which conductors of great capacity have been even moderately heated by lightning. I do not admit Mr. Sturgeon's "on dit" respecting the conductor passing through the Nelson Monument in Edinburgh. It is really no evidence whatever on a scientific question. "*It is said* (observes Mr. Sturgeon) that the lightning rod passing through the Nelson Monument became so hot by lightning that it could not be touched by the hand by *the first person* who visited it afterwards. Allowing a few minutes to have elapsed between the flash and the person entering the monument, the probability would be that the conductor had been made red-hot." This is of the same character with all Mr. Sturgeon's data; it is generally surmise, the *shew* without the reality; it just amounts to nothing.

45. I am aware that it has been also *supposed* that the great conductors of St. Paul's church were heated by lightning, but it is only a *supposition.* The conductors were not examined before the lightning, which was said to have fallen on them, occurred, so that we cannot be certain that the observed appearances were not originally present after the forging of them; it is, besides, very unlikely that a stroke of lightning should have fallen on this building, capable of rendering bars of iron, six inches wide and one inch and a half thick, red-hot, without

---

[*] Philosophical Transactions.

destroying the thin copper covering the ball and cross on the dome of the building, and without the crash of the thunder having been heard over the whole city, no mention of which is made; when St. Bride's steeple was struck, the latter was peculiarly remarkable.

46. There is another instance on record of the effects of lightning on an iron rod, in Port Royal, Jamaica, mentioned in the transactions of the Royal Society, the evidence of which seems very incomplete. Two men are said to have perished by lightning near the church wall: that is not improbable: but, on subsequently looking inside the wall, a bar of iron, an inch thick, and a foot in length, was found in many places wasted away to the size of a fine wire. Now it does not appear that this bar was examined *previously* to the occurrence of the lightning; hence we cannot infer that the wasting was produced by the electric fluid; more especially as similar appearances are not uncommon in bars of iron erected in churchyards in this country, and which have evidently resulted from oxidation and time.

47. Seeing then how much evidence we have from actual experience of the protective effect of regular conductors of the *worst* kind, and their power of transmitting dense explosions of lightning, we may reasonably infer that a conductor of copper, equal to a rod of an inch diameter, and *extending the whole length of the mast*, would be proof against any discharge of lightning ever experienced, as, I think, is shewn by the cases in which ships fitted with my conductors have been struck by shocks of lightning without damage.

48. Exceptions, however, have been taken by Mr. Sturgeon to the phenomena described by the officers who either commanded or were in the ships. Thus Captain Turner, in describing the shock of lightning which fell on the Dryad frigate on the coast of Africa, says, that "he saw the lightning on the conductor on the fore-mast, and saw it during another flash run down the mizen-mast; that all the men there heard a loud whizzing noise." Captain Fitzroy and Lieut. Sullivan also mention similar phenomena. Now the exceptions taken are these, viz., that no noise is ever produced by electricity entering a conductor, and that we cannot produce a "running light" upon a conductor carrying an electrical charge.

These exceptions, however, are rather captious objections to forms of expression, than to the facts themselves; it is easy to shew from experience that luminous appearances are often attendant on discharges of both natural and artificial electricity.

Thus in the case of the Hawk, No. 9, the account states that "the vessel was apparently enveloped in a flame of lightning;" whilst, in the case of the Beagle, Lieut. Sullivan says, "on looking aloft, the ship was apparently in a blaze of fire." In the case of the Snake, No. 3, the electric fluid is said to have *descended* with an instantaneous explosion of a vivid purple color.

When H.M.S. Norge was struck by lightning in Port Royal harbour, the electric fluid was observed (to use Admiral Rodd's expression) to "absolutely stream down a conductor attached to the mast of H.M.S. Warrior," close by.

Such phenomena are besides remarkably close to the results of experiments: thus a heavy shock of electricity, passed over a metallic wire, in a partially exhausted receiver, will exhibit a transiently passing light on its surface.

49. The whizzing noise is quite in accordance with common electrical effects. It invariably occurs when a good conductor receives and disarms an explosion by a pointed extremity. Mr. Sturgeon, however, asserts that "no such noise is ever produced by the *fluid entering* a metallic conductor." This is mere sophistry; let any one attempt to discharge a highly charged battery by an acutely pointed conductor. A great part of the charge will immediately rush through or towards the point with a whizzing noise. Now the stratum of cloud may be either positively or negatively electrified, and whether the one or the other, it is clear that the rush of electricity from a charged surface toward a point, or from a point towards an undercharged surface (according to Franklin's hypothesis) will be always attended by a whizzing noise.

50. The protection which continuous conductors would afford if well and efficiently applied to ships is, I think apparent in all the preceding cases, and when we consider that the masts are themselves conductors of electricity, and that by their position alone they determine the course of the discharge into the body of the hull, it becomes the more requisite to affix to them good conductors, which quickly disperse and reduce the electrical action to a state of quiescence.

We have I think fair evidence of this in the trials hitherto made with the continuous fixed conductors applied to certain ships of the British Navy.

51. These ships have been exposed more or less in all points of the world. Lightning has not fallen upon them *oftener* than other vessels not so fitted; and *and when it has done so no damage* has arisen in any way, or has any destructive lateral effect, such as that contended for by Mr. Sturgeon, taken place. His comparison, therefore, of the effects of lightning on the Rodney with the "*probable* effects" (as he terms it) on my

conductors, although he can find no instance of such ***probable effects***, is therefore purely hypothetical. If Mr. Sturgeon has no good authenticated fact to oppose to the mass of evidence I have adduced, of what avail is any hypothetical or loose opinion he may find it *convenient* to advance?

52. Before concluding this communication, I cannot refrain from pointing out the apparent inconsistencies of his views on this point. Having described my conductors as dangerous and objectionable in every possible way, as calculated to induce oblique flashes of lightning to strike the ship to the destruction of the sailors' lives, the sails, rigging, &c. &c., he says, sec. 221, on discovering that he could not conveniently apply his own rods above the top-mast head, "as *however every chance of danger to the men and every species of damage* to the vessel ought *strictly to be avoided,* it still appears desirable to furnish the top-gallant rigging with conductors; and perhaps those which would give the least trouble to the men, would be strips of *copper let into grooves of the masts according to the plan proposed by Mr. Harris.*" Now, I think, it must be clear to any one, that if any system be so objectionable as he would have it believed, on the grounds above stated, it must be equally objectionable on the top-gallant masts; the lives of the sailors are just as much exposed there as at any other point, perhaps more so. Mr. Sturgeon himself admits that two men were killed there in the case of the Rodney. But by his admission above quoted, my method is not objectionable in the top-gallant mast, but is on the contrary calculated to avoid "every species of damage to the vessel and every chance of danger to the men;" if so, it must be equally efficient on the top-mast, lower mast, &c. This sort of traverse sailing, to use a nautical phrase, is not a little amusing, and is, I believe, quite unprecedented in any paper on science.

53. In order that no mistake may arise in respect of what I have advanced relating to lateral explosions, I may in conclusion simply state, that I do not deny the expansive force of a dense electrical explosion, and its destructive effect on ***imperfect*** and ***non-conductors.*** I do not deny its effect in causing expansion in the surrounding air, which I rather choose to call with Priestley, "the lateral FORCE of electrical explosions," than *a lateral explosion of electricity.* I do not deny this in the absence of any regular system of conductors, or that the discharge may divide in several directions, and in distributing itself over the hull, may cause dense sparks and other electrical appearances in various parts of the vessel, but which would not appear, if a perfect system of conduction, such as I have proposed, were resorted to.

I do, however, deny the probability of any lateral discharge of electric matter from conducting bodies transmitting an accumulation between oppositely charged surfaces, as assumed by several persons imperfectly acquainted with ordinary electrical action, and lately by Mr. Sturgeon; and, I maintain, that neither artificially, nor in the course of nature, can any instance of such lateral explosion be authenticated.

I am, &c.
W. Snow Harris.

Plymouth, March 14, 1840.

P.S. It has been insisted on by Mr. Sturgeon, that a shock of lightning, descending a continuous conductor on the mast, would magnetize every chronometer in the cabin, &c.—(Memoir, Sect. 207.)

This assumption is completely negatived by the cases above quoted. In fig. 4, an awful discharge descended an iron chain, and yet no magnetic effect was observable on the neighbouring compasses, or on the chronometer in the cabin. It is only in the *absence* of continuous conductors we find such magnetic effects, and even then their occurrence is comparatively rare. Really, Mr. Sturgeon makes so many random assertions, it is almost impossible to attend to them all.

IX.—Mr. Sturgeon's *Fifth Letter to* W. Snow Harris, Esq. F.R.S. *on Marine Lightning Conductors.*

Sir,

When I had finished my last letter to you,* I made up my mind to decline any further notice of your impotent productions in defence of that extraordinary, unnecessarily expensive, and certainly the most unscientific and dangerous plan of marine lightning conductors, that could possibly have been thought of by any one claiming the character of an electrician. But finding, in the preceding article, a few descriptions of the effects of lightning on shipping, which, if correct, can hardly fail to be interesting to the electrician, I have not hesitated to give them a place in these Annals; and as you have laid considerable stress on these events, as sure indications of the infallibility of your electrical philosophy and lightning conductors, I have again ventured a few remarks, not with any hope of convincing you of your errors, or rather of your acknowledging them, but to shew you that a very different explanation to that which you have attempted, would look quite as plausible to account for some of the effects produced by lightning on those vessels; and in order to facilitate the comparison, I will, as on former occasions, follow the numerical order of your own paragraphs.

---

* Annals of Electricity, &c., vol. iv. p. 496—500.

I think that I may very justly remark, as an introductory proposition, not difficult of demonstration, that, *if there be no motive beyond the propagation of truth*, one of the causes of your committing so many errors in calculating on the effects of lightning, is simply by your imagining that *all discharges of lightning are alike powerful.*

In paragraph 26, you have, no doubt, given a very exact account of the dimensions of the chain topsail-sheet, and of the copper pipe of Hearle's pump. Then, because each link of the former consisted of two sides, it was *virtually* composed of two iron rods, each of which was half an inch diameter from one end to the other; and as the latter "was three inches in diameter," the copper sheet of which it was made was uniformly nine inches broad. Hence the topsail-sheet and Hearle's pump were no mean conductors, even compared with your own; for the copper pipe of the pump was nearly of the same transverse dimensions as the *mean* of yours on the *lower masts;* of much greater transverse dimensions than yours on the *top-masts;* twice the transverse dimensions of yours on the *top-gallant masts;* and more than twice the transverse dimensions of your royal conductors. Moreover, since "the effects on the fore-mast were very similar" to those produced on the main-mast, we are led to believe that each mast received only one half of the *main stroke.* And again, by considering also, that *each mast alone* would carry some portion of the lightning to the sea, then, taking all these circumstances into account, and also the probability of this flash being very far from the most formidable that occurs, I cannot see that this case is any proof either of the efficiency or inefficiency of your conductors, nor can I see what advantage you could think of gaining in defence of them, by bringing such a circumstance forward, in which the conductors which were not injured were, in some parts of the circuit, more than four times the dimensions of yours.

Paragraph 28 may possibly be a very correct account of the effects of *a* flash of lightning on the Athol; and so far as *description* is concerned, it is an interesting paragraph. But I do not agree to what you say in paragraph 29, viz., that "these effects are similar to the former, and shew the protection afforded by *the* chains, and their power of conducting *heavy* discharges of lightning;" because, if I did, I should have to acknowledge that all "*heavy* discharges of lightning" were of precisely the same power; which would be the very opposite to the views which I take of these operations of nature; and, what would be worse still, if possible, I should have to acknowledge that *half a flash of lightning* ought to produce precisely the same effect as the whole flash would do. This, as I first observed, is one of the rocks on which you so frequently founder.

The effects of lightning on H.M.S. Snake, as described in paragraph 30, are also very curious and interesting; more especially if Captain Milne's account of the route of the electric fluid be correct. Perhaps you can explain why the electric fluid jumped from "within eight feet of the deck" to "the saddle of the main boom;" and by what route, from "the saddle of the main boom one portion passed out of the quarter-deck port to the sea."

Paragraph 31 is an obvious indication of the limited views which you have of lightning, and of the correctness of my first remark in this letter, viz., that you consider all flashes of lightning to be of the same power; and that the power of half a flash is equal to that of the whole flash. This inference, you will find, is clear enough, when you compare the description of the two events alluded to, in this paragraph, in which you say the Buzzard "lost her top-gallant and top-mast, under precisely the same circumstances as those of the Hyacinth;" for it is obvious that the Hyacinth's main-mast was struck by only *half* of the original flash; whereas the mast of the Buzzard was struck by the *whole* flash.

The only inference which, in a philosophical point of view, can be drawn from your 32d paragraph, is that the Fox revenue cutter was struck by a comparatively feeble stroke of lightning; although, from the manner in which you appear to apply the case, that paragraph becomes demonstrative of your belief that all flashes of lightning are of precisely the same power.

You seem to be very desirous to shew your readers, that "the copper usually placed about a cutter's mast is not the $\frac{1}{32}$nd part of an inch in thickness;" and that "in this case it remained perfect." Now it strikes me that some of your readers will ask, why you did not give them the *other* dimensions of the copper? A candid, scientific reasoner would not have left them in doubt on a point of such essential importance in varying the effects of a flash of lightning on the metal. Even you own tinsel experiments (figs. 4, 5, and 6, plate x., vol. iv.) must have taught you that a broad strip of gold leaf may remain perfect, though it be traversed by an electric discharge which would destroy a *narrow* strip of the same thickness;

and that a still more powerful discharge might have destroyed both of them. Whether or not there be a fatality attending your philosophy, over which you have no control, is not for me to determine; but there seems something curious enough in placing the power of a flash of lightning, which just "furrowed" the mast of the Fox cutter, on a par with the power of that flash which produced such tremendous havoc on board the Rodney, or with that which destroyed "both the fore and main-top masts and top-gallant masts" of the Hyacinth!!!

With respect to the effects of lightning on "the spire of a church at Kingsbridge," as described in paragraph 33, I can have no doubt of your having a very correct account from the Rev. G. F. Wyse; and I have only to request the same favour on your part, whilst you read an account of another flash of lightning, the effects of which were also described by a reverend gentleman.

On the 28th of April last, the bishop of Nova Scotia, in company with the Rev. Mr. Cardwell, called at this Institution, and entered into a conversation with me on the subject of atmospheric electricity. The bishop, who is well acquainted with electricity, described several curious effects of lightning which had come under his own observation. On one occasion, where lightning had struck a conductor which was fastened close to the wall of a building, a portion of the brick-work behind the conductor was crushed to powder, and a deep furrow, parallel to the conductor, was made in the wall from top to bottom.

At another time, the bishop saw a flash of lightning ascend from the earth to the clouds, lifting up, in its passage, a great quantity of the soil and other earthy matter from the surface of the ground to a great height.

These two remarkable circumstances being described by an eye-witness of such high authority as the bishop of Nova Scotia, may justly be regarded as exceedingly interesting events in the history of atmospheric electricity. The furrow being made in the wall behind the conductor, is an excellent contrast to the effects of lightning on the spire of the church at Kingsbridge, and shews that, although the iron rod was sufficient to conduct a flash of lightning of a certain force with safety; yet, the lateral forces of a still more powerful flash might possibly not only furrow, but totally destroy the spire to which it is attached.

Your 34th paragraph is merely a copy of Lieut. Sullivan's letter already printed in these Annals,* and, therefore, I can have nothing to remark upon it in this place, excepting that I may be permitted to say, that it is an exceedingly interesting description of the appearances on board the Beagle at the time she was *supposed* to be struck by lightning, and, unquestionably, is the best description of those appearances that has hitherto been given; and so very different to that given by Captain Fitz-Roy,† that they scarcely appear to relate to the same event. I do not see, however, that even Lieut. Sullivan's description can be considered to be "totally subversive of all" that I have "advanced concerning destructive lateral explosions." That officer candidly acknowledges that he is not sufficiently acquainted with electrical experiments to offer any remarks on those which I have adduced. Now, sir, had you also acknowledged that you were not sufficiently acquainted with atmospheric electricity to offer any remarks on those phenomena which I have described in my fourth memoir, I should have considered that you also were enjoying the same honourable feelings.

---

* Vol. iv. p. 327.

The philosophy of paragraph 36 is exceedingly good; and Abercrombie's doctrine is perfectly applicable in the present instance; for if your ideas of atmospheric electricity had been formed from a sufficient number of facts collected from your own observations, they would have been much more comprehensive than at present; but from a want of that experience so essential to the formation of a sound judgment of all the variety of atmospheric electrical operations, and, to distinguish one class of them from another, your views of this branch of the science are necessarily very limited; and, having confessed that you have no acquaintance whatever with atmospheric electrical *waves*, it is not to be expected that you can have any knowledge of the splendid phenomena which they produce on high elevated conductors. Hence it is that you have fallen into error by supposing that the Dryad and Beagle were struck by lightning; though to a person well experienced in electrical kite experiments, it becomes obvious enough that the lightning never struck either vessel in the cases alluded to. And a person only *moderately* acquainted with electro-magnetism would know well that the *primitive* discharge never traversed the Beagle's main-mast conductor on that occasion.

Whilst writing my fourth memoir, I was particularly careful in advancing no experimental facts but those with which I was quite familiar: hence it was that I described no other electric kite experiments than those I had myself made, nor any of those splendid phenomena witnessed by other experimenters, whilst exploring the atmosphere in a similar manner: hoping, from the confidence I then placed in your candor, that your desire to promote truth would have induced you to allude to them yourself; especially those phenomena seen by M. de Romas at his kite-string. Those phenomena were of a similar character to some of those which I have described,* and were obtained under similar circumstances; and had you brought them forward in your papers, as I expected you would have done, they would have given a fair opportunity to your readers to form a just comparison between them and the phenomena seen on board the Beagle and the Dryad.†

---

* Vol. iv. p. 327.

Your reasoning in paragraph 37 is curious enough, implying that, because the Hyacinth was not *blown up*, there could be no lateral sparks!!!

Paragraph 38 is a twin-sister to its predecessor, and implies that, because the man in the cross-trees was not killed, there could be no lateral discharge!! Why ought the man to have been killed? That he had a very narrow escape, no one will deny, when the circumstances are properly understood. "The top-gallant mast was shivered, and a *terrific shock* darted from the heel of it to the chain topsail tye." Now this "*terrific shock*" was one of those cases in which that kind of lateral force is produced which Priestly calls the *lateral explosion*, and which you have been forced to acknowledge in paragraph 53. When you were writing that *confessional* paragraph, in which you say, "I do not deny the expansive force of a *dense electrical explosion*, and its destructive effects on *imperfect*, and *non-conductors*," I suppose you had forgotten the "seaman aloft on the cross-trees," close to the "*terrific shock*," or *dense electrical explosion*!!"

With respect to Wilson, who was killed in the Rodney, since there were no marks to be found either on his body or his clothes, it is fair to infer that he suffered from the lateral forces, and not by the primitive discharge which killed his shipmate.

In paragraph 39, you again attempt to lead your readers astray by telling them that, by my theory, as you are pleased to call it, a "lateral discharge may sometimes occur, and sometimes not." If, instead of insulting your readers by thus attempting to impose upon their credulity, you had referred them to paragraph 203 of my fourth memoir, page 176, vol. iv. of these Annals, the only inference which they would have drawn would have been the following:—"As the extent of electro-displacement, in vicinal bodies, depends upon the magnitude and intensity of the primitive discharge or *main stroke,*" and as those conditions may probably vary with almost every flash of lightning, some flashes may be of such feeble powers as to produce no very formidable *lateral* effects; although others may be sufficiently powerful to exert lateral forces productive of the most serious consequences."

---

* See my Fourth Memoir.

† M. de Romas's experiments are described in the appendix to this letter.

Admiral Hawker's account of the effects of lightning in the Mignomne, is very interesting. As "the main-mast was split down to the keelson," there can be no question about the principal charge being transmitted in that direction; and, consequently, the man who was killed "near the armourer's bench," suffered either by the *lateral* force of the main stroke, or by so small a portion of the latter, as to produce no serious *lateral* effects on his shipmate who was "sleeping beside him." Moreover, the two men who were killed in the main-top, "being burnt black," shews pretty clearly that they suffered from a very superior force to that which killed the man below, whose external injuries were only "one black speck in his side." The main top-mast being shattered to pieces, and the splinters being scattered "in all directions," is another instance of the formidableness of *lateral explosions*.

I am much obliged to Robert Purk for shewing you his shirt; for as the man was not injured, it is pretty clear that neither he nor his shirt were struck by the lightning; and that it was the *lateral* force which "took a strip out of his shirt about two inches wide from the shoulder to the wrist without hurting him." The most probable *immediate* cause of the shirt being torn was a sudden distention of the air within the sleeve. See paragraph 40.

The cases which occurred in the Hawke cutter were obviously the effects of *lateral electric forces*. The "boy experienced a shock only," not being hit by the lightning. Neither did the lightning *strike*, but only "*passed close* to another man," who only experienced a temporary effect in his left arm." I am not certain that you could have produced better data than these to prove the injurious effects of lateral explosions.

Your mode of accommodating your philosophy to facts is truly curious and ingenious, and as nearly *opposite* to that exercised by profound reasoners as any one could be led to expect. In paragraph 41, you say "that no damage occurs from a shock of lightning *out of its direct path*." This beautiful philosophical inference is a master-piece of its kind, emanating, as it obviously does, from the fact which you described the moment before, in which you say, "that the electric matter, in passing down the main hatchway, passed *between* a man and a boy;" and that these persons, who were "out of the *direct path*" of the lightning, were both affected by it.

From the above specimen of your philosophy I pass on to your case No. 12, in which I am glad to find that you view the effects of lightning on the New York packet more seriously than in paragraph 7.* The best account of the damage done in this vessel is that given by Mr. Rich.† The spindle was fused for several inches of its length, and the chain "conductor was literally torn to pieces and scattered to the winds."

With respect to the Nelson Monument at Edinburgh, I have nothing to add to the fact which I have previously stated; excepting that I may here remark, that your attempt to place that fact or any other which bears on this important topic, in the back-ground, indicates a desire to evade those cases which ought to be particularly attended to. It is extreme cases of the effects of lightning that ought to be guarded against; and if those cases be not provided for, with regard both to the dimensions and position of the metal, no conductor can give the necessary protection.

Your obvious *intentional* attempt, in paragraph 48, to pervert the meaning of some of those points on which I have touched in my fourth memoir, tends to excite a strong suspicion that the whole of your perversions have emanated from some *unaccountable motive*, of a very different nature to that which would have been expected from a person of your pretensions. How dared you attempt to make it appear that I have said "that we cannot produce a running light upon a conductor carrying an electric discharge"? How dared you venture to palm upon your readers such a palpable barefaced untruth? My language on this topic is the following:—"We cannot produce any thing like a *running light* when the conductors are sufficiently good and capacious to conceal the motion of the fluid; though such a phenomenon may easily be produced by the employment of inferior conductors.‡

With respect to the appearances on board the Dryad and Beagle, I have already expressed my opinion pretty clearly; and I have not met with any statement of the facts which has the least tendency to alter that opinion.

That the Hawk cutter should appear as if "enveloped in a flame of lightning," from the luminous effects of the flash which struck her, appears probable enough, although it is certain that nothing of the kind occurred; for "the electric matter in passing down the main hatchway, passed between a man and a boy," and therefore could not envelope the vessel. The flash, *prior* to its striking an object, produces the greatest luminous effects; and lightning passing *near* to either a ship or a house would produce similar luminous effects to those which appeared to the people on board the Hawk and the Beagle.

---

* Annals of Electricity, vol. iv. p. 487. † Ibid. p. 372.

‡ Annals of Electricity, &c. vol. iv. p. 186.

Dr. Franklin's opinion on these luminous effects of lightning will be seen in the Appendix to this letter.

Another instance of those mean attempts to pervert the meaning of certain topics of my fourth memoir appears in paragraph 52, in which you take to yourself a great deal of credit, by conveying to your readers as profound a falsehood as ever proceeded from man: stating, as you do, that I have admitted that your method of protecting the masts of ships is not objectionable. This paltry manœuvre strengthens my former suspicions, and gives a very sable coloring to the *motives* from which emanate such unjust aspersions. If you had regarded truth, and the just interests of science, whilst quoting my remarks on conductors for top-gallant masts, you would have directed your readers to paragraphs 221 and 222 of my fourth memoir,* in which I have stated, that, "instead of only *one* strip (of copper) to each mast, I should propose *three* in each, at equal distancee from each other; which, by having an exposure of metal on every side, would be a greater security to the mast than by having one strip only. And that four cylindrical copper rods, or four flexible metallic ropes, stretched from the cross-trees to the truck, parallel to the top-gallant shrouds, would afford a much better protection to the top-gallant rigging than conductors let into the masts."

It is not for me to judge of the opinions which other readers form of your philosophy, but to me you seem to have been led into the most extraordinary inaccuracies in many parts of your defence of your lightning conductors; and, perhaps, in none more so, than in the postscript to the preceding paper, in which you appear to be determined, either to mislead your readers, or, to shew your almost entire ignorance of electro-magnetic action. Permit me to ask you a few questions on this subject. Do you wish me to understand, that you, a Fellow of the Royal Society, are totally ignorant of Sir Humphrey Davy's experiments, by which that philosopher first magnetized steel needles by transmitting electric discharges from a battery of jars, through a vicinal conducting wire? Do you wish me to understand that you, a Fellow of the Royal Society, with the pretensions of an electro-magnetist, never repeated those beautiful experiments? Do you wish me to understand that you, a Fellow of the Royal Society, who, as an inventor of a marine lightning conductor ought to be a profound electrician and electro-magnetist, that *you* who are pretending to protect the British Navy and our brave tars from the effects of lightning,—that you, on whose judgment such mighty interests are to be at stake,—are entirely ignorant of the laws of electro-magnetism? If you are not entirely ignorant of the magnetic action of electric currents traversing good conductors, how dared you venture to say, that "it is only in the *absence* of continuous conductors we find such magnetic effects?" If you are not entirely ignorant of such magnetic action, how dared you venture to stain the pages of British science, to insult the dignity of the Royal Society, and, above all, to deceive the Lords Commissioners of the Admiralty, and the whole British Navy, by propagating such a palpable falehood? Will you acknowledge that you are ignorant of the magnectic action of lightning whilst traversing good conductors; or will you have to submit to the degrading position of having *wilfully* concealed that most important fact, to guard against which is one of the most essential considerations in the erection of marine lightning conductors?

---

* Annals of Electricity, &c., vol. iv. page 185.

I have paid considerable attention to the statements of professors Farrady and Wheatstone in the "Report of the Committee appointed by the Admiralty*;" but I have not been able to discover any facts, in those statements, which have the least tendency to alter my opinion of your plan of conductors, and certainly none whatever tending to invalidate any part of my fourth memoir. By quoting M. De Romas's kite experiments, professor Wheatstone has shewn, pretty clearly, that electrical discharges such as appeared on the conductors of the Dryad and the Beagle, are no sure indications of those vessels being struck by lightning; for, as will be seen in the appendix to this letter, no lightning was present at the time that the French philosopher was conducting his kite experiments, in which he saw "sheets of fire 9 or 10 feet long and an inch broad, which made as much or more noise than the reports of a pistol." Perhaps the most remarkable feature in the Report of the Committee, is the total absence of any consideration respecting the magnetic action which lightning would produce on chronometers, compass needles, &c. whilst traversing vicinal conductors; the consequences of which, in misleading the mariner, might be more fatal than those from the direct action of the lightning itself.

I remain, Sir, yours &c,

W. STURGEON.

Victoria Gallery, of Practical Science, Manchester,
June 22nd, 1840.

---

* Page 1 of this volume.

Appendix to Mr. STURGEON'S *Letter.*

Amongst Dr. Franklin's remarks on Mr. William Maine's Account of the effects of lightning on his Lightning Rod, we find the following:

"It is said that *the house was filled with its flash.* Expressions like this are common in accounts of the effects of lightning, from which we are apt to understand that the lightning filled the house. Our language indeed seems to want a word to express the *light* of lightning as distinct from the lightning itself. When a tree on a hill is struck by it, the lightning of that stroke exists only in a narrow vein between the cloud and tree, but its light fills a vast space many miles round; and people at the greatest distance from it are apt to say, "the lightning came into our rooms through our windows." As it is in itself extremely bright, it cannot, when so near as to strike a house, fail illuminating highly every room in it through the windows; and this I suppose to have been the case at Mr. Maine's; and that, except in and near the hearth, from the causes above-mentioned, it was not in any other part of the house; *the flash* meaning no more than *the light* of the lightning.—It is for want of considering this difference, that people suppose there is a kind of lightning not attended with thunder. In fact there is probably a loud explosion accompanying every flash of lightning, and at the same instant;—but as sound travels slower than light, we often hear the sound some seconds of time after having seen the light; and as sound does not travel so far as light, we sometimes see the light at a distance too great to hear the sound."—*Franklin's Letters.*

M. DE ROMAS'S *Kite Experiment.*

"The greatest quantity of electricity that was ever brought from the clouds, by any apparatus prepared for that purpose was by M. De Romas, assessor to the presideal of Nerac. This gentleman was the first who made use of a wire interwoven in the hempen cord of an electrical kite, which he made seven feet and a half high, and three feet wide, so as to have eighteen square feet of surface. This cord was found to conduct the electricity of the clouds more powerfully than an hempen cord would do, even though it was wetted; and, being terminated by a cord of dry silk, it enabled the observer (by a proper management of his apparatus) to make whatever experiments he thought proper, without danger to himself.

" By the help of this kite, on the 7th of June, 1753, about one in the afternoon, when it was raised 550 feet from the ground, and had taken 780 feet of string, making an angle of near forty-five degrees with the horizon; he drew sparks from his conductor three inches long and a quarter of an inch thick, the snapping of which was heard about 200 paces. Whilst he was taking these sparks, he felt, as it were, a cobweb on his face, though he was above three feet from the string of the kite; after which he did not think it safe to stand so near, and called aloud to all the company to retire, as he did himself about two feet.

"Thinking himself now secure enough, and not being incommoded by any body very near him, he took notice of what passed among the clouds which were immediately over the kite; but could perceive no lightning either there or any where else, nor scarce the least noise of thunder, and there was no rain at all. The wind was West, and pretty strong, which raised the kite 100 feet higher, at least, than in the other experiments.

"Afterwards casting his eyes on the tin tube, which was fastened to the string of the kite, and about three feet from the ground, he saw three straws, one of which was about one foot long, a second four or five inches, and a third three or four inches, all standing erect, and performing a circular dance, like puppets, under the tin tube, without touching one another.

"This little spectacle, which much delighted several of the company, lasted about a quarter of an hour; after which, some drops of rain falling, he again perceived the sensation of the cobweb on his face, and at the same time heard a continual rustling noise, like that of a small forge bellows. This was a farther warning of the increase of electricity; and from the first instant that M. De Romas perceived the dancing straws, he thought it not advisable to take any more sparks even with all his precautions; and he again entreated the company to spread themselves to a still greater distance.

"Immediately after this came on the last act of the entertainment, which M. De Romas acknowledged made him tremble. The longest straw was attracted by the tin tube, upon which followed three explosions, the noise of which greatly resembled that of thunder. Some of the company compared it to the explosion of rockets, and others to the violent crashing of large earthen jars against a pavement. It is certain that it was heard into the heart of the city, notwithstanding the various noises there.

"The fire that was seen at the instant of the explosion had the shape of a spindle eight inches long and five lines in diameter. But the most astonishing and diverting circumstance was produced by the straw, which had occasioned the explosion, following the string of the kite. Some of the company saw it at 45 or 50 fathoms distance, attracted and repelled alternately, with this remarkable circumstance, that every time it was attracted by the string, flashes of fire were seen, and cracks were heard, though not so loud as at the time of the former explosion.

"It is remarkable, that, from the time of the explosion to the end of the experiments, no lightning at all was seen, nor scarce any thunder heard. A smell of sulphur was perceived, much like that of the luminous electric effluvia issuing out of the end of an electrified bar of metal. Round the string appeared a luminous cylinder of light, three or four inches in diameter; and this being in the day-time, M. De Romas did not question but that, if it had been in the night, that electric atmosphere would have appeared to be four or five feet in diameter. Lastly, after the experiments were over, a hole was discovered in the ground, perpendicularly under the tin tube, an inch deep, and half an inch wide, which was probably made by the large flashes that accompanied the explosions.

"An end was put to these remarkable experiments by the falling of the kite, the wind being shifted into the east, and rain mixed with hail coming on in great plenty. Whilst the kite was falling, the string came foul of a penthouse; and it was no sooner disengaged, that the person who held it felt such a stroke in his hands, and such a commotion through his whole body, as obliged him instantly to let it go; and the string, falling on the feet of some other persons, gave them a shock also, though much more tolerable*.

"The quantity of electric matter brought by this kite from the clouds at another time is really astonishing. On the 26th of August, 1756, the streams of fire issuing from it were observed to be an inch thick, and 10 feet long. This amazing flash of lightning, the effect of which on buildings or animal bodies, would perhaps have been equally destructive with any that are mentioned in history, was safely conducted by the cord of the kite to a non-electric body placed near it, and the report was equal to that of a pistol.

"M. Romas had the curiosity to place a pigeon in a cage of glass, in a little edifice, which he had purposely placed, so as that it should be demolished by the lightning brought down by his kite. The edifice was, accordingly, shattered to pieces, but the cage and the pigeon were not struck†.

---

* Gents. Mag. for August 1756, p 378.

† Nollet's Letters, vol. ii. p. 239.

"The Abbé Nollet, who gives this account, adds, that if a stroke of this kind had gone through the body of M. De Romas, the unfortunate professor Richman had not probably been the only martyr to electricity, and advises, that great caution be used in conducting such dangerous experiments.*

"When we consider how many severe shocks the most cautious and judicious electricians often receive through inadvertence, we shall not be surprised that when philosophers first began to collect and make experiments upon real lightning, it should sometimes have proved a little untractable in their hands, and that they were obliged to give one another frequent cautions how to proceed with it.

"The Abbé Nollet, as early as the 1752, advises that these experiments be made with circumspection; as he had been informed, by letters from Florence and Bologna, that those who had made them there had had their curiosity more than satisfied by the violent shocks they had sustained in drawing sparks from an iron bar electrified by thunder. One of his correspondents informed him, that once, as he was endeavouring to fasten a small chain, with a copper ball at one of its extremities, to a great chain, which communicated with the bar at the top of the building (in order to draw off the electric sparks by means of the oscillations of this ball) there came a flash of lightning, which he did not see, but which affected the chain with a noise like that of wild fire. At that instant, the electricity communicated itself to the chain of the copper ball, and gave the observer so violent a commotion, that the ball fell out of his hands, and he was struck backwards four to five paces. He had never been so much shocked by the experiment of Leyden."†—*Priestley's History of Electricity.*

X.—*Description of a Cast Iron Voltaic Battery, and an Account of some of its Performances.* By William Sturgeon.

Having given a notice in the last Number, that I would describe this battery in the present one, I now proceed to do so.

The battery consists of ten cast-iron cylindrical vessels, and the same number of cylinders of amalgamated rolled zinc with diluted sulphuric acid. The cast-iron vessels are 8 inches high and 3½ inches diameter. The zinc cylinders are the same height as the iron ones, and about 2 inches diameter, and open throughout. The iron and zinc cylinders are attached, in pairs, to each other, by means of a stout copper wire, as seen in fig. 7, plate 1. The zinc of one pair is placed in the iron of the next, and so on throughout the series; contact being prevented by disks of millboard placed in the bottom parts of the iron vessels.

---

* Phil. Trans. vol. lii. pt. i. p. 342.

† Phil. Trans. vol. xlviii. pt. i. p. 205.

As it is my intention to embody a series of experiments made with this battery, with a number of others, in a paper which will appear in the August number of this work, I will merely notice a few, in this place, which will give a tolerably good idea of its powers.

With ten pairs in series, I have usually obtained 14 cubic inches per minute of the mixed gases from the decomposition of water, and 10½ cubic inches when the battery had been in action an hour and a half. But on the 20th inst. I obtained 20 cubic inches per minute, and this day I obtained 22 cubic inches per minute with the same arrangement; fused 1 inch of copper wire of $\frac{1}{75}$ of an inch diameter; four inches was kept white hot; and 18 inches of the same wire was kept red hot in broad daylight.

Eight inches of watch main springs was kept red hot, and 2 inches white hot, for several successive minutes.

We now employ this battery daily at this Institution.

W. Sturgeon.

Royal Victoria Gallery,
For the Encouragement of Practical Science, Manchester,
June 22, 1840.

XI.—MISCELLANEOUS ARTICLES.

Curious Remarks on the Wreck of the "Royal George."

At a recent meeting of the Geological Society, there were read, "'Remarks on the structure of the Royal George, and on the condition of the timber and other materials brought up during the operations of Colonel Pasley in 1839,' by Mr. Creuze. The Royal George was the first ship built on the improved dimensions recommended in consequence of an inquiry into the superior sailing qualities of the vessels of war in the French and Spanish services. She was commenced at Woolwich in 1746, launched in 1756, and, after bearing a very high character as a ship of war for twenty-six years, was accidentally sunk at Spithead on the 29th of August, 1782. From an examination of the various portions of the wreck recovered by the operations of Colonel Pasley, Mr. Creuze states that the great agent of the work of destruction, during the fifty-seven years since the loss of the Royal George, has been "the worm," which has gradually, by its innumerable perforations on every exposed portion of the wood work, reduced it to such a state as to enable the constant wash of the tides to abrade it layer by layer. The portion of the ship which has thus been removed is considered to be the whole of the upper part, including the topsides above the line of the middle-deck ports. The portions of the recovered timbers which had been buried in the mud were perfectly sound; and Mr. Creuze is of opinion that the bottom of the ship, which is thus protected, and too deeply inhumed to be affected by the explosion, will last for ages. Some portions of the copper have undergone so little change, that several whole sheets average the same weight per square foot as those now used in the royal navy; and this state of preservation, Mr. Creuze believes, may be accounted for on the principle applied by Sir Humphrey Davy to the protection of the sheathing of ships. The cast-iron guns which have been recovered were so much softened as to be easily abraded by the finger-nail to the depth of one-sixteenth and one-eighth of an inch, but they gradually hardened on exposure to the atmosphere. The brass guns are as sharp in their ornamental castings, and apparently as sound, as at their first immersion. A piece of two-and-a-half inch cable-layed cordage, made from a specimen of tarred rope (possibly part of the ship's old junk for sea-store, or of one of the cables used in an attempt to weigh her soon after she sunk), was found to bear 21cwt. 3qrs. 7lbs.; while a similar cable, made from yarn spun in 1830, bore only 20cwt. 1qr. 7lbs. Mr. Creuze then stated some peculiarities in the structure of the Royal George, and concluded with a descriptive catalogue of a series of specimens which accompanied Mr. Creuze's paper.

Further Particulars respecting the Royal George.

Colonel Pasley began his proceedings for the removal of the wreck of the Royal George on the 1st of this month, but up to this day (Monday) nothing very remarkable was effected. Two guns, the rudder, and a considerable quantity of timber were recovered; but as these were merely those fragments of last year's work which the inclemency of the season prevented the engineers from picking up, no serious measures were deemed necessary till yesterday. At eight o'clock in the morning, the red flags at Spithead announced that a great explosion was to be attempted: and at eleven o'clock one of those huge cylinders, which have already been described, and filled with 2,116lbs. of gunpowder, was lowered to the bottom. One of Colonel Pasley's divers (George Hall), who has acquired great expertness in these operations, descended his rope ladder a little in advance of the cylinder, and succeeded in fixing it securely to one of the lower gudgeons or braces on the rudder-post, within six or eight feet of the keel. The diver having remounted, and the vessels being withdrawn to a safe distance, the enormous charge was ignited by means of the voltaic apparatus. Within less than two seconds after the shock was felt, the sea rose over the spot to the height of about fifteen feet, or not quite half so high as it did on the occasion of the great explosions last year; a difference ascribable, probably, to the cylinder on the present occasion having been placed under the hull instead of alongside it. The commotion in the water, however, was so great, as to cause the lumps and lighters to pitch and roll at a great rate. The whole surface of the sea, for several hundred yards round, was presently covered with dead fish and small fragments of the cylinder. Amongst these were innumerable tallow candles, and a mass of butter a foot and a half in length, evidently driven up from the purser's store-room. As soon as the vast commotion in the water had subsided, and the boats had returned from the universal scramble for the candles and dead fish; the diver proceeded again to the bottom, and soon reported that the whole stern of the ship had been driven to pieces, and that, so far as he could ascertain, there was now a free and wide channel directly fore and aft the ship, from stem to stern, through which both the flood and ebb tides will rush; and thus the mud with which the hull of the Royal George has been silted for half a century will be washed out, and the way cleared for Colonel Pasley's further operations. From the auspicious manner, indeed, in which he has commenced, we may safely predict his final success; and we confidently trust that, before the season closes, Spithead will be cleared of this grievous and long-standing drawback to its efficiency as a roadstead for line-of-battle ships.

Further Particulars.

The operations have continued daily with great activity and success, two divers being employed every slack tide in slinging the fragments of the wreck. The stern-post has been got up, broken into three pieces by the great explosion of the 11th instant, together with a large fragment of dead-wood, that stood over the keel, and was also connected with the stern-post. A very curious mass, consisting of part of the lower deck, with a portion of beam, and two knees below the deck-plank, and a rider or upright knee above it, together with part of a port, and the remaining, both of the inside and outside, planking, on each side of a fragment of timber, may now be seen in the Dock Yard. A very large cable has also been got up, measuring twenty-four inches round, and about ninety fathoms in length, which was generally very sound, but has been broken into several pieces, so that the diver had to descend repeatedly for two or three days before he slung the whole of it. All is clear now above the orlop deck, except some beams of the lower deck, which still remain. This day (Saturday) red flags were hoisted on board the two lumps, at ten o'clock, as a signal that two explosions, of 250lbs. of powder each, would take place at the next slack tide, and two divers were sent down to make preparations for placing the two charges, one under the main hatch of the orlop deck, the other near the bread room. Lieut. Symonds, the executive engineer, who made all the arrangements on this occasion, as well as for the great explosion of the 11th, then sent down the charges with the divers, and having removed the lumps to a little distance, he posted himself at one voltaic battery, whilst Sergeant-Major Jones had charge of the other. Colonel Pasley then gave the word to fire, but only one explosion took place, which was effected by four cells of Professor Daniell's battery, at the distance of 240 feet. This produced the usual effect of a great commotion in the water, in the form of an inverted bowl, spreading to a considerable distance, but not rising to any great height; several seconds elapsed, after a sharp shock was felt, before this agitation of the water took place. The second explosion, which was to have been fired by means of Mr. Alfred Smee's new voltaic battery, did not take place on completing the circuit; but Sergeant-Major Jones, feeling the shock of the other explosion, believed it to be his own, for he completed the voltaic circuit, on first receiving the order to fire. Being ordered to complete the circuit a second time, he did so; and on keeping up the contact for about four seconds, the explosion was effected at the distance of 460 feet. After these explosions, which were witnessed by Admirals Sir Edward Codrington, and Bouverie; Major-General Sir Hercules Pakenham, and a number of officers of both services, and numerous other spectators, the divers repeatedly went down again, and lashed large pieces of timber, amongst which were the parts of a lower deck beam. A human skull, with teeth, was also brought up from the after part of the wreck, which Colonel Pasley has declared his intention to bury in Kingston churchyard, together with such other remains of skeletons as may be obtained hereafter.

Operations against the Wreck of the "Royal George," and proposed Great Explosion.

The mud accumulated in the hold of the wreck having proved troublesome to the divers, a number of small charges of 47lb. and of 260lb. of gunpowder have been fired against the wreck within the last fortnight, and the removal of the fragments has proceeded with great activity; but it now appears necessary to have recourse to another great explosion of about 2,160lb. of powder, to be placed in a wooden cylinder made in Chatham dockyard, which having been coated with a waterproof composition, and sunk in fifteen fathoms at Spithead, was declared to be perfectly water tight. Colonel Pasley has declared his intention of firing this great charge at about a quarter before two o'clock on the afternoon of Monday, the 22nd instant, when the neap tides and long slack water will favour the operation. Red flags will be hoisted on board the Success frigate hulk, and the two lumps or mooring lighters at Spithead, several hours before the explosion on the day above-mentioned. Should a violent gale of wind occasion such a swell as to prevent the operation on the 22nd, it will be postponed till the 23rd or 24th, and each day of delay will cause the explosion to take place about three quarters of an hour later than the time before mentioned.

Sketch of the Life of the late Lieut. Bell.

"John Bell was the eldest son of a hat-manufacturer of respectability and considerable property, residing in Carlisle, and was born on the 1st of March, 1747. Until he attained the age of eighteen, he assisted in the management of his father's business; indeed, from his parent having engaged in scientific pursuits, and more particularly in the vain endeavor to discover the longitude, the duties of the business almost wholly devolved on the subject of this sketch. In the year 1765, Sergeant Harding, of the Royal Artillery, who was familiarly known to the family, being at Carlisle recruiting, young Bell was induced to enter into the service of his country, and after having received the usual drilling at Woolwich, he in the following year embarked for Gibraltar, in the 3rd battalion, under Major Innes, where he remained about six years. On his return to England, he obtained a furlough, and proceeded into the north to visit his relatives, when he found that, during his absence, his father had fallen into embarrassments, from having neglected his business, and spent his property in his endeavors to obtain the prize offered by government to the discoverer of the longitude. Shortly after his return to Woolwich, his expertness in handling the Macaroni Gun elicited the applause of his sovereign, who, clapping him on the shoulder, exclaimed, "Fine young fellow—fine young fellow—make a man of you." From this time his abilities became daily more apparent, and his promotion was rapid. He first became bombadier in 1775, and was sent on a recruiting mission to Carlisle, where, being well known, and his success consequently great, he was continued for some months. He was engaged in various schemes connected with military pursuits during the succeeding seven years, and in 1782 we find him paymaster-sergeant and conductor of stores to the artillery encamped on South Sea Common. From South Sea Castle he was an eye-witness to the foundering of the Royal George, and from that time his mind became occupied in devising some plan for raising, or, should that be impracticable, for destroying the wreck. On the treaty of peace with America being ratified, he returned to Woolwich, where he was made inspector of the proof, which situation he held at the time of his death. Here he devoted his leisure hours to the scientific pursuits on which the whole energies of his mind were bent, and which his inventive genius enabled him to exhibit in a series of most extraordinary and valuable inventions. These it would be useless to attempt to describe in a mere biographical sketch, it must, therefore, suffice if some of the most remarkable be enumerated. And first, may be mentioned the "Sun Proof," by which the soundness of the interior of ordnance is scrutinised most effectually, and this proof is considered so decidedly superior to all others, that it is still believed to be used in the royal arsenal, and doubtless many a brave fellow is indebted for his lengthened existence to the security thus afforded him from the bursting of heavy artillery. A gyn, called "Bell's Gyn," (also still in use in the Royal Military Repository,) bears witness to his inventive genius, as do further, an effective petard, (a model of which may be seen at the laboratory, at Woolwich,) a method of destroying ordnance by means of a ponderous weight worked at a considerable altitude, and a variety of minor inventions and improvements. In 1791, a silver medal and a premium of five guineas were awarded to him by the Society of Arts, "For a safe crane, whereby the lives of persons descending or ascending precipices, wells, shafts of mines, &c., will be saved, although the line by which they are suspended may by accident be broken." In 1793, he received a further premium of twenty guineas from the same society, "For a gun and harpoon on a new construction for taking whales, after satisfactory trials made therewith." (Vide Trans. of this Society.) Nor must be omitted the notice of his invention for destroying sunken bodies by the operation of gunpowder, the result of his reflections on the fate of the Royal George. Having proved its practicability by blowing up a sunken rock in the Frith of Forth, he further demonstrated its utility by experiments performed at Woolwich before the Duke of Richmond and other military gentlemen. Having directed a vessel to be built upon a scale of one inch to fifty of the thickness of the Royal George's side, he caused it to be sunk in the Thames, and with 50lbs. of gunpowder, afterwards conducted into her magazine, blew her to pieces. The experiment took place at high water, and answered every expectation of the inventor. A further experiment was made, which seemed to promise success, viz., the breaking of chains or booms laid across rivers, by means of a mine of gunpowder. (Vide Gents. Mag. for 1789, pages 753 and 947.) From this it is evident that, but for his premature death, the Royal George had long ago ceased to engage the inventive faculties of scientific minds. Colonel Pasley, who is now occupied on that wreck, to his highest honour be it spoken, has in a recent letter to the Times, generously awarded to Lieut. Bell his ready assent to the claim of his being the first projector of this scheme for her demolition, although he had not been previously acquainted with it.

"But the invention which of all others entitled him to a place as well among the "sons of genius," as among the benefactors of mankind, is that of the "Apparatus for rescuing shipwrecked mariners," for which the world generally considers itself indebted to Captain Manby. Without the least desire to advance any claim derogatory to the fair fame of the gallant captain, we must be permitted to adduce such proofs as are afforded (and these, we consider, are incontrovertible) that the real merit of the invention belongs solely, and in every material respect, to the departed genius whose memory we now seek to perpetuate. His untimely death afforded Captain Manby the opportunity of bringing it more decidedly before the public, and of obtaining the high emolument of which it was considered deserving; let him, therefore, permit the empty honour to gild the escutcheon of the undoubted proto-inventor. It is scarcely necessary to describe the apparatus, since there can be but few persons who are unacquainted with its nature and uses. The models of the whole apparatus, originally deposited by Lieut. Bell, may be inspected in the rooms of the Society for the Encouragement of Arts and Sciences. Several successful experiments were made at Woolwich before the Duke of Richmond, then Master-General of the Ordnance, and other distinguished and scientific individuals, when Lieut. Bell, by discharging a shell from a mortar, threw on shore a rope, by which he drew himself to the land with perfect ease, of which exploit a living witness now resides at Woolwich, namely, Mr. Laycock, who at that time served in the Royal Artillery. His success was so perfectly satisfactory, that, in 1792, a premium of fifty guineas was awarded to him for the invention by the Society of Arts. It would seem that Captain Manby's claim to the originating of this contrivance rested solely upon the subtle distinction of his throwing a line across the ship from a mortar on shore, whilst Lieut. Bell's has been thought to suggest the throwing it from the vessel *only*. The following extract from Lieut. Bell's observations, transmitted to the Society of Arts, and published in their transactions, proves that his plan comprehended both these methods:—"there is every reason to conclude that this contrivance would be very useful at all ports of difficult access, both at home and abroad, where ships are liable to strike ground before they enter the harbour, as Shields Bar, and other similar situations, when a line might be *thrown over the ship*, which might probably be the means of saving both lives and property; and, moreover, if a ship were driven on shore near such a place, the apparatus might easily be removed to afford assistance; and the whole performance is so exceedingly simple, that any person once seeing it done, would not want any further instruction. (Vide Trans. of the Society of Arts, vol. 25, page 135.)

"In addition to the above convincing proofs of the right of Lieut. Bell to the honour of this invention, two extracts are subjoined—First, from Lieut. General Farrington's letter of 19th January, 1808, transmitting report of the committee of field officers of artillery at Woolwich on the Manby apparatus, in which the lieut.-general states, "that this invention was brought forward by the late Lieut. Bell, of the Royal Artillery, nearly fourteen years since; his idea was to project the rope from the ship to the shore, which is assuredly the method most to be depended upon, as the vessel in that case carries the means with it, and need not rely on any fortuitous assistance from the shore." And, secondly, an extract from the letter of Colonel Ramsay, dated October, 1808, containing a report of field officers of artillery on the Manby apparatus, in which the colonel states, "that Lieut. Bell was presented with a premium from the Society of Arts for a similar application of ordnance to come from the ship to the shore, and for having also suggested its utility in *projecting the rope from the land to vessels* in danger of being wrecked." (Vide report of the committee of the House of Commons on the application of Captain Manby for remuneration for the apparatus.)

"If any proof be required in addition to the above of the true person entitled to the merit of this valuable invention, it might be found in the fact of the House of Commons having, in the year 1815, voted the sum of £500 to Mrs. E. Whitfield, as a reward for the ingenuity of her father, in discovering the apparatus for rescuing persons in danger of losing their lives by shipwrecks.

"Little more remains to be said; his reputation as a man of sound practical science was now fully established, and in 1793 he was presented by the Duke of Richmond, who had frequently witnessed his experiments and unreservedly expressed his high opinion of his abilities, with a commission as second lieutenant in the Royal Artillery. Somewhere about this time he was despatched on a secret expedition, which had for its object the destruction of the Dutch fleet in the Texel, but which was abandoned. In January, 1794, he was promoted to the rank of first lieutenant in the Invalid Battalion in the same regiment, and in this station continued till his death. Lieut. Bell died suddenly at Queenborough, on the 1st June, 1798, in the height of his successful career, whilst engaged in improving and fitting out the fire-ships there, after having devoted his best energies to the service of his country, to the interests of science, and to the cause of humanity, without having reaped those pecuniary benefits which he might have reasonably anticipated as the reward of untiring exertions, terminating in a series of valuable discoveries."—*United Service Journal.*

Photogenic Art—Engraving.

It was stated last week, that Dr. Berres, of Vienna, had discovered a method of fixing the impressions produced by Daguerréotyphy, by means of which these productions can be employed instead of engraved plates, and copies therefrom printed, as in the course of the ordinary copper plates, &c. We now give a copy of the doctor's account of his discovery:—

"It was announced in the Vienna Gazette of the 18th of April last, that I had succeeded in discovering a method by which I was enabled both permanently to fix the pictures produced by the method of Daguerré, and to render them available to all the purposes of etchings upon copper, steel, &c. from which copies might be struck off to any extent, as in the case of ordinary engraved productions; and it was stated in the same newspaper, that I proposed bringing my discovery immediately before the public.

"As a member of this distinguished society, I consider it my duty, first, to make known to this learned body a discovery which creates so much hope, and which promises so great a benefit to the arts and sciences. The well known expenses and difficulties attendant on the publication of an extensive work, requiring engravings as illustrations, led me, in the first instance, to hope that I might be enabled to render the discovery of Daguerré available by improvements, to represent and fix the objects necessary to my work; and the first view of a heliographed picture aroused in me the desire also to represent, in the same manner, microscopic objects, although attempts, with the strongest lamplights, to produce engravings or etchings had been unsuccessful, and the idea abandoned as hopeless, until revived by a sight of the hydro-oxygen gas microscope of M. Schuh, of Berlin, an instrument which, in its power and clearness, has never before been equalled or even approached. On the 27th February last, I had the honour of laying before the learned body the results of the united investigations of my distinguished colleague, Professor de Ettingshausen, and myself upon this subject, and the perfectly successful experiments of pictures prepared through the process of photography upon microscopic objects. Many specimens of the results of our researches and successful attempts to employ photography for scientific and useful purposes are now placed before you for examination. Through this new method, the Daguerréotype is rendered more extensively available for scientific uses. Every object which is discernible to the eye with clearness, can, in the future, through the means of the iodined silver plates, be minutely etched, and, true to naturé, (for she is herself the artist!) be copied with the minutest exactness. But the beautiful representations which we are able to produce through the means of the Daguerréotype are liable to so many injuries, and are so delicate, fragile, and evanescent, that they never can be rendered available for illustrating works of science and other useful purposes.

"In a Petersburgh newspaper, of March last, I first saw an account of some attempts to bring the Daguerréotype process into general use. In the meantime, M. Daguerré had declared, before the institute of Paris, the complete failure of all his attempts, by means of etching, to obtain the impression even of a single copy.

"The experiments at St. Petersburgh, and the hope of eventual success, urged me to attempt to make some use of the Daguerréotype pictures; and I began, at the commencement of this month, my series of experiments. Without recapitulating all these, in which I was assisted with cordial zeal by M. Francis Kratochwila (a gentleman in the employ of government), and by M. Schuh, who placed at my disposal an immense number of Daguerréotype plates, and before I come to an explanation of the process by which I render these Daguerréotype pictures permanent and capable of further use, I consider it necessary to lay before this learned body the following observations:—

"1st, With the copper plates, as used at present in the Daguerréotype process, we can effect only the permanently fixing, never the etching and printing of copies therefrom.

"2nd, For the heliographic etchings, it is necessary that the picture be produced with the required intensity, upon pure chemical silver plates.

"3rd, The etching of the Daguerréotype picture is produced through the influence of nitric acid, to be explained hereafter.

"4th, For the permanently fixing of the Daguerréotype impression, a galvanic power is necessary.

"5th, For the changing of the Daguerréotype picture into a deep metal etching, so as to be used as a means of printing, the chemical process of etching is of itself sufficient.

"My newly discovered method of managing the Daguerréotype pictures may be divided into two processes:

"1st, The permanently fixing the design.

"2nd, The changing of the design, when once permanently fixed, into an etching upon the plate.

"The method of permanently fixing the Daguerréotype picture with a transparent metal coating consists in the following process:—

"I take the pictures produced in the usual manner, by the Daguerréotype process, hold them for some minutes over a moderately-warmed nitric acid vapour, or steam, and then lay them in nitric acid of 13° to 14° Reaumur, in which a considerable quantity of copper or silver, or both together, has been previously dissolved. Shortly after being placed therein, a precipitate of metal is formed, and can now be changed to what degree of intensity I desire. I then take the heliographic picture coated with metal, place it in water, clean it, dry it, polish it with chalk or magnesia and a dry cloth or soft leather. After this process the coating will become clean, clear, and transparent, so that the picture can again be easily seen. The greatest care and attention are required in preparing the Daguerréotype impressions intended to be printed from. The picture must be carefully freed from iodine, and prepared upon a plate of the most chemically pure silver.

"That the production of this picture should be certain of succeeding, according to the experiments of M. Kratochwila, it is necessary to unite a silver with a copper plate; while, upon other occasions, without being able to explain the reasons, deep etchings or impressions are produced, without the assistance of the copper plate, upon pure silver plate.

"The plate will now, upon the spot where acid ought not to have dropped, be varnished; then held for one or two minutes over a weak warm vapour or steam, of 25° to 30° (Reaumur), of nitric acid, and then a solution of gum arabic, of the consistence of honey, must be poured over it, and it must be placed in an horizontal position, with the impression uppermost, for some minutes. Then place the plate, by means of a kind of double pincette (whose ends are protected by a coating of asphalt or hard wood), in nitric acid, at 12° or 13° (Reaumur). Let the coating of gum slowly melt off or disappear, and commence now to add, though carefully and gradually, at a distance from the picture, a solution of nitric acid, of from 25° to 33°, for the purpose of deepening or increasing the etching power of the solution. After the acid has arrived at 16° to 17° (Reaumur), and gives off a peculiar biting vapour, which powerfully affects the sense of smelling, the metal becomes softened, and then, generally, the process commences changing the shadow upon the plate into a deep engraving or etching. This is the decisive moment, and upon it must be bestowed the greatest attention. The best method of proving if the acid be strong enough is to apply a drop of the acid in which the plate now lies to another plate; if the acid make no impression, it is, of course, necessary to continue adding nitric acid; if, however, it corrode too deeply, then it is necessary to add water, the acid being too strong. 'The greatest attention must be bestowed upon this process. If the acid has been too potent, a fermentation or white froth will cover the whole picture, and thus not alone the surface of the picture, but also the whole surface of the plate, will quickly be corroded. When, by a proper strength of the etching powers of the acid, a soft and expressive outline of the picture shall be produced, then may we hope to finish this undertaking favorably. We have now only to guard against an ill-measured division of the acid, and the avoidance of a precipitate. To attain this end, I frequently lift the plate out of the fluid, taking care that the etching power shall be directed to whatever part of the plate it may have worked the least, and seek to avoid the bubbles and precipitate by a gentle movement of the acid.

"In this manner the process can be continued to the proper points of strength and clearness of etching required upon the plates from which it is proposed to print. I believe that a man of talent, who might be interested with this art of etching, and who had acquired a certain degree of dexterity in preparing for it, would very soon arrive at the greatest clearness and perfection; and, from my experience, I consider that he would soon be able to simplify the whole process. I have tried very often to omit the steaming and the gum arabic; but the result was not satisfactory, or the picture very soon after was entirely destroyed, so that I was compelled again to have recourse to them.

"The task which I have undertaken is now fully performed, by placing in the hands of this learned body my method of etching and printing from the Daguerréotype plates, which information, being united to the knowledge and mathematical experience we already possess, and published to the world, may open a road to extensive improvement in the arts and sciences. By thus laying open my statement to the scientific world, I hope to prove my devotion to the arts and sciences, which can end only with my life."

Discovery of the Mariners' Compass.

"Much interest must for ever attach to the discovery of this instrument, and yet there are few subjects concerning which is less known. For a period, the honour of the invention was ascribed to Gioia, a pilot, or ship captain, born at Pasitano, a small village situated near Malphi, or Amalfi, about the end of the thirteenth century. His claims, however, have been disputed. According to some, he did not invent but improve it; and according to others, he did neither. Much learning and labor have been bestowed upon the subject of the discovery. It has been maintained by one class, that even the Phœnicians were the inventors; by another, that the Greeks and Romans had a knowledge of it. Such notions, however, have been completely refuted. One passage nevertheless, of a very remarkable character, occurs in the work of Cardinal de Vitry, bishop of Ptolemais, in Syria. He went to Palestine during the fourth crusade, about the year 1204; he returned afterwards to Europe, and subsequently went back to the Holy Land, where he wrote his work entitled "Historia Orientalis," as nearly as can be determined, between the years 1215 and 1220. In chap. xci of that work he has this singular passage:—"the iron needle, after contact with the loadstone, constantly turns to the north star, which, as the axis of the firmament, remains immoveable, whilst the others revolve; and hence it is essentially necessary to those navigating on the ocean." These words are as explicit as they are extraordinary; they state a fact, and announce a use. The thing, therefore, which essentially constitutes the compass, must have been known long before the birth of Gioia. In addition to this fact, there is another equally fatal to his claims as the original discoverer: it is now settled beyond a doubt, that the Chinese were acquainted with the compass long before the Europeans. It is certain that there are allusions to the magnetic needle in the traditionary period of Chinese history, about 2,600 years before Christ; and a still more credible account of it is found in the reign of Ching-wang, of the Chow dynasty, before Christ 1,114. All this, however, may be granted, without in the least impairing the just claims of Gioia to the gratitude of mankind. The truth appears to be this: the position of Gioia, in relation to the compass, was precisely that of Watt in relation to the steam engine—the element existed, he augmented its utility. The compass used by the mariners in the Mediterranean, during the twelfth and thirteenth centures, was a very uncertain and unsatisfactory apparatus. It consisted only of a magnetic needle floating in a vase or basin by means of two straws or a bit of cork supporting it on the surface of the water. The compass used by the Arabians in the thirteenth century was an instrument of exactly the same description. Now the inconvenience and inefficiency of such an apparatus are obvious; the agitation of the ocean, and the tossing of the vessel, might render it useless in a moment. But Gioia placed the magnetized needle on a pivot, which permits it to turn to all sides with facility. Afterwards, it was attached to a card, divided into thirty-two points, called Rose des Vents; and then the box containing it was suspended in such a manner, that however the vessel might be tossed, it would always remain horizontal. The result of an investigation participated by men of various nations, and possessing the highest degree of competency, may thus be stated. The discovery of the directive virtue of the magnet was made anterior to the time of Gioia. Before that period, navigators, both in the Mediterranean and Indian seas, employed the magnetic needle; but Gioia, by his invaluable improvement in the principle of suspension, is fully entitled to the honor of being considered the real inventor, in Europe, of the compass as it now exists."—*Campbell's Maritime Discovery.*

ANNALS OF ELECTRICITY &c

VOL. 5. PLATE 1.

Fig. 3.

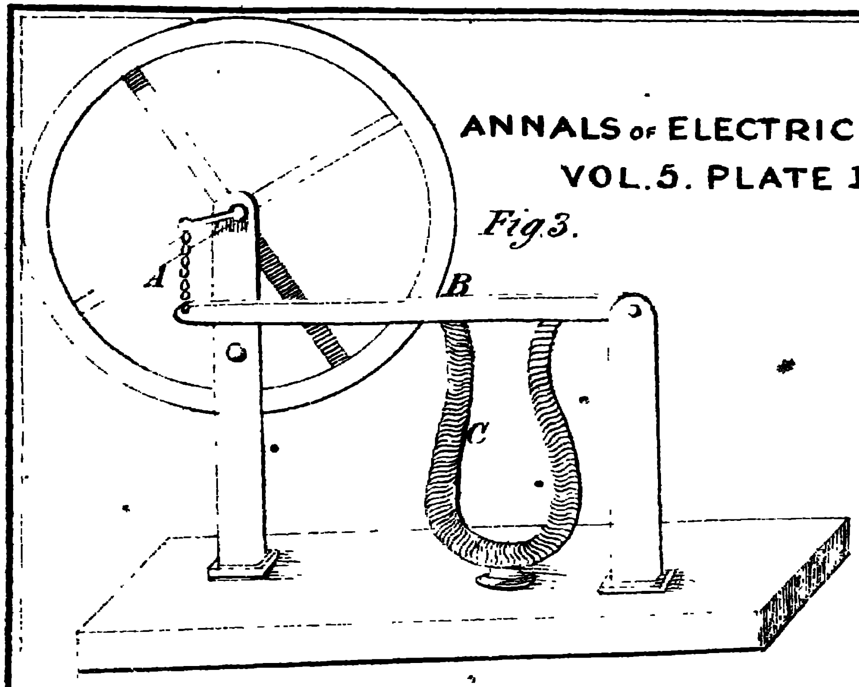


Fig 2

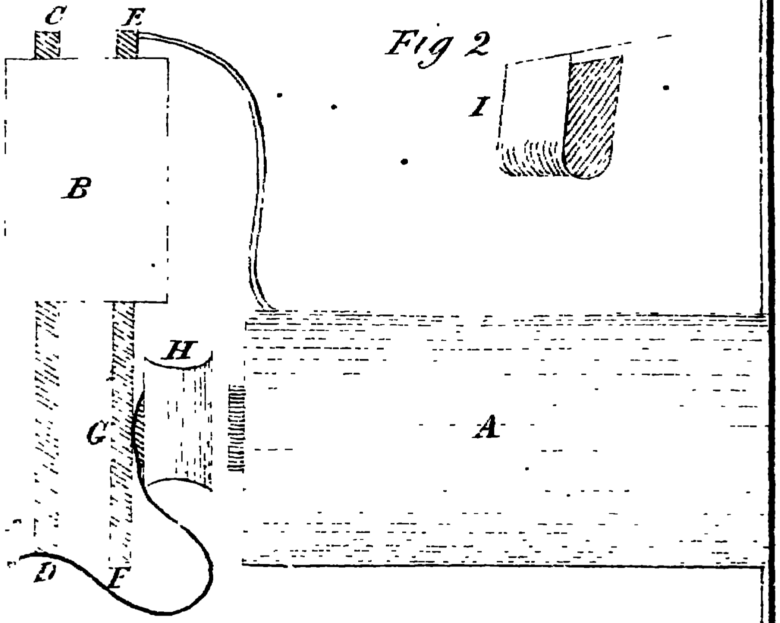


Fig 1.

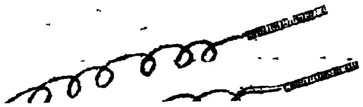


Fig. 4.

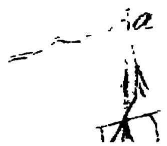


Fig 5

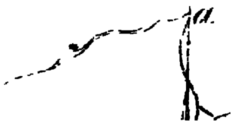
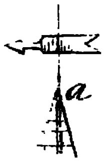


Fig 6.



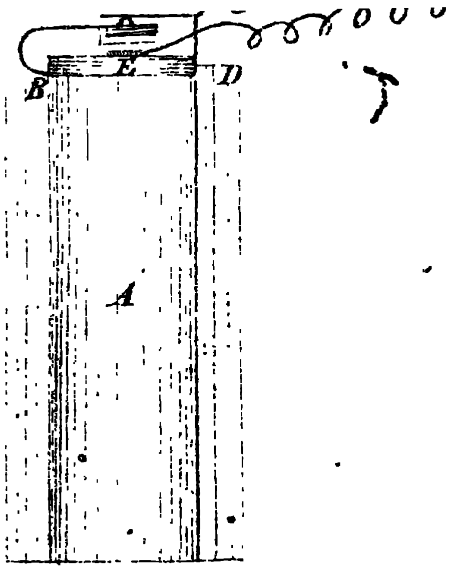
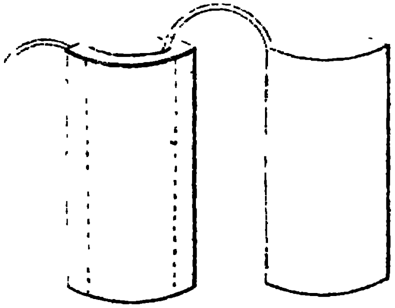
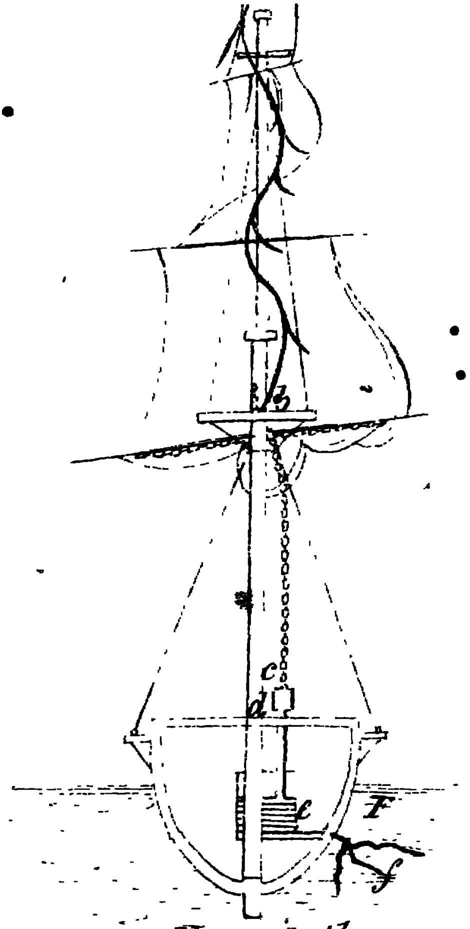
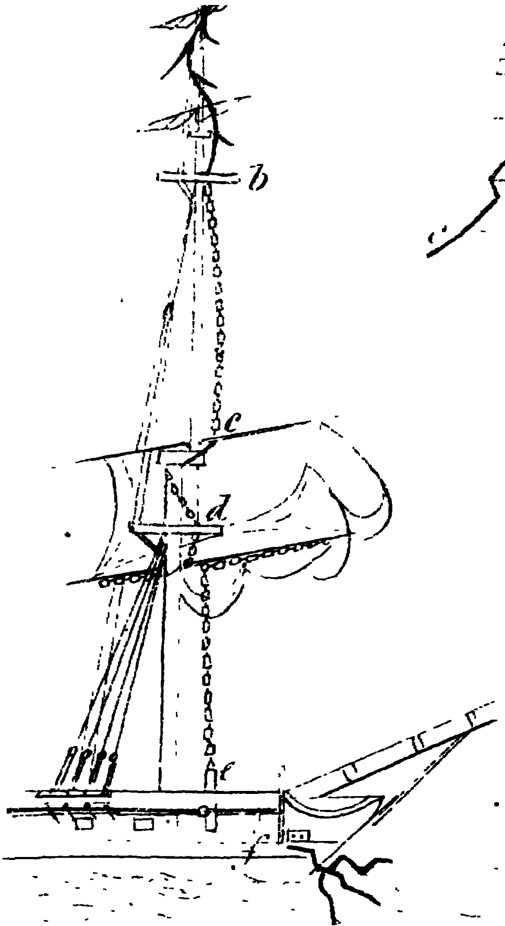


Fig 7.

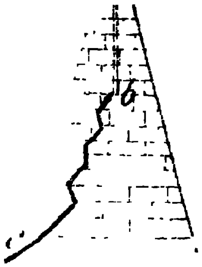




Hyacinth



Athol



ANNALS OF ELECTRICITY &c
VOL. 5. PLATE 1.

Fig 3.

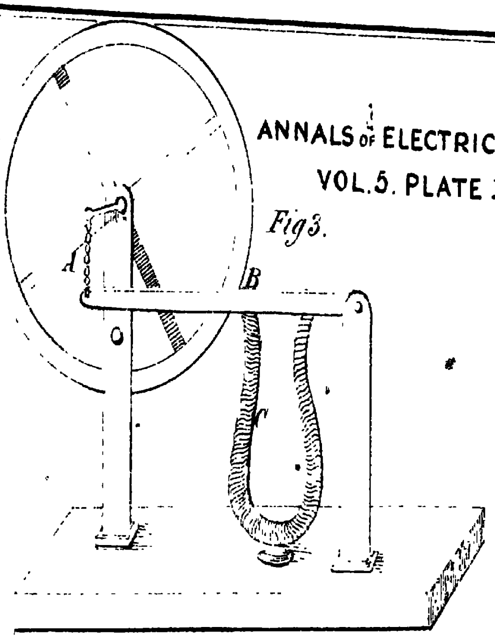


Fig 2

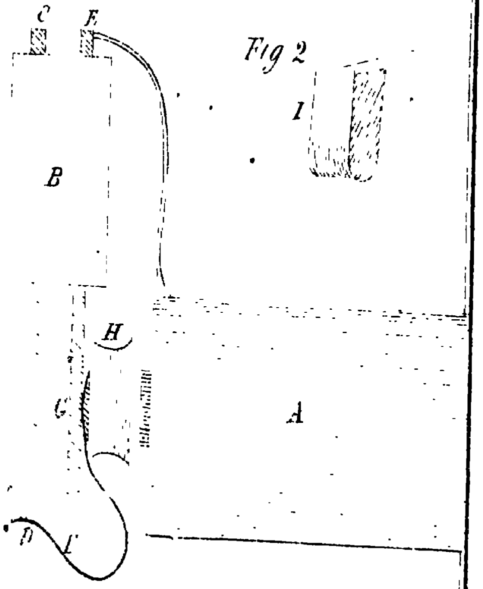


Fig 1.

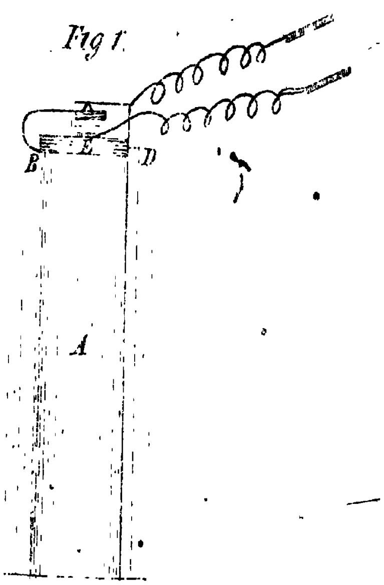
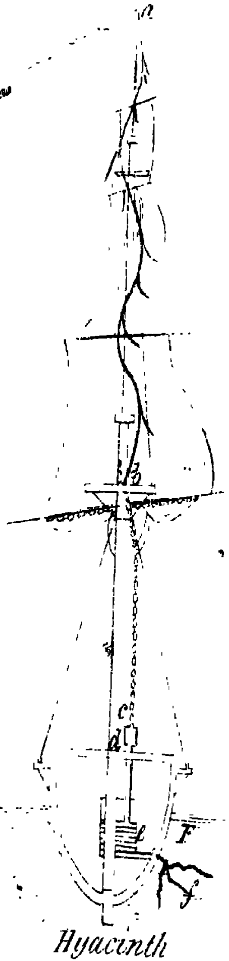
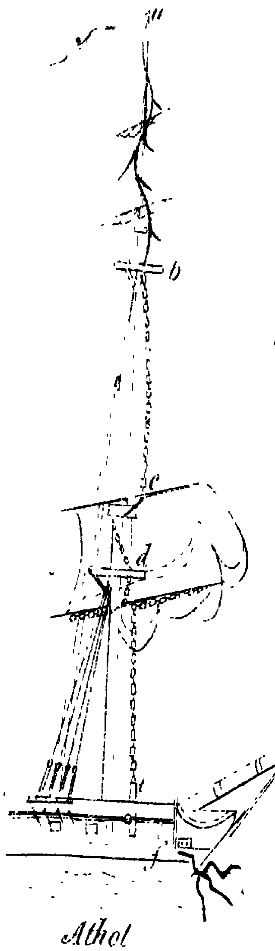


Fig. 4



Hyacinth

Fig. 5



Athol

Fig 6.

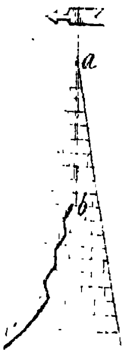
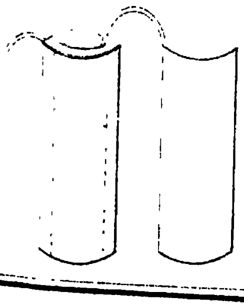


Fig 7.



THE ANNALS
OF
ELECTRICITY, MAGNETISM,
AND CHEMISTRY;
AND
Guardian of Experimental Science.

AUGUST, 1840.

XII.—*Experimental Researches in Electricity.—Twelfth Series.* By Michael Faraday, Esq. D.C.L. F.R.S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Florence, Copenhagen, Berlin, &c. &c.

Received January 11,—Read February 8, 1838.

§ 18. *On Induction (continued).* ¶ vii. *Conduction, or conductive discharge.* ¶ viii. *Electrolytic discharge.* ¶ ix. *Disruptive discharge—Insulation—Spark—Brush—Difference of discharge at the positive and negative surfaces of conductors.*

1318. I proceed now, according to my promise, to examine, by the great facts of electrical science, that theory of induction which I have ventured to put forth (1165. 1295. &c.) The principle of induction is so universal that it pervades all electrical phenomena; but the general case which I purpose at present to go into, consists of insulation traced into and terminating with discharge, with the accompanying effects. This case includes the various *modes* of discharge, and also the condition and characters of a current; the elements of magnetic action being amongst the latter. I shall necessarily have occasion to speak theoretically, and even hypothetically; and though these papers profess to be experimental researches, I hope that, considering the facts and investigations contained in the last series in support of the particular view advanced, I shall not be considered as taking too much liberty on the present occasion, or as departing too far from the character which they ought to have, especially as I shall use every opportunity which presents itself of returning to that strong test of truth, experiment.

1319. Induction has as yet been considered in these papers only in cases of insulation;—opposed to insulation is *discharge.* The action or effect which may be expressed by the general term *discharge*, may take place, as far as we are aware at present, in several modes. Thus, that which is simply called *conduction* involves no chemical action, and apparently no displacement of the particles concerned. A second mode may be called *electrolytic discharge*; in it chemical action does occur, and particles must, to a certain degree, be displaced. A third mode, namely, that by sparks or brushes may, because of its violent displacement of the particles of the *dielectric* in its course, be called the *disruptive discharge*; and a fourth may, perhaps, be conveniently distinguished for a time by the words *convection*, or *carrying discharge*, being that in which discharge is effected either by the carrying power of solid particles, or those of gases and liquids. Hereafter, perhaps, all these modes may appear as the result of one common principle, but at present they require to be considered apart; and I will now speak of the *first* mode, for amongst all the forms of discharge that which we express by the term conduction appears the most simple and the most directly in contrast with insulation.

¶ vii. *Conduction, or conductive discharge.*

1320. Though assumed to be essentially different, yet neither Cavendish nor Poisson attempt to explain by, or even state in, their theories, what the essential difference between insulation and conduction is. Nor have I anything, perhaps, to offer in this respect, *except* that, according to my view of induction, both it and conduction depend upon the same molecular action of the dielectrics concerned; are only extreme degrees of *one common condition* or effect; and in any sufficient mathematical theory of electricity must be taken as cases of the same kind. Hence the importance of the endeavour to shew the connexion between them under my theory of the electrical relations of contiguous particles.

1321. Though the action of the insulating dielectric in the charged Leyden jar, and that of the wire in discharging it, may seem very different, they may be associated by numerous intermediate links, which carry us on from one to the other, leaving, I think, no necessary connexion unsupplied. We may observe some of these in succession for information respecting the whole case.

1322. Spermaceti has been examined and found to be a dielectric, through which induction can take place (1240. 1246), its specific inductive capacity being about or above 1·8 (1279,) and the inductive action has been considered in it, as in all other substances, an action of contiguous particles.

1323. But spermaceti is also a *conductor*, though in so low a degree that we can trace the process of conduction, as it were, step by step through the mass (1247.); and even when the electric force has travelled through it to a certain distance, we can, by removing the coercitive (which is at the same time the inductive) force, cause it to return upon its path and reappear in its first place (1245. 1246.) Here induction appears to be a necessary preliminary to conduction. It, of itself, brings the contiguous particles of the dielectric into a certain condition, which, if retained by them, constitutes *insulation*, but if lowered by the communication of power from one particle to another, constitutes *conduction*.

1324. If *glass* or *shell-lac* be the substances under consideration, the same capabilities of suffering either induction or conduction through them appear (1233. 1239. 1247.), but not in the same degree. The conduction almost disappears (1239. 1242.); the induction therefore is sustained, i. e. the polarized state into which the inductive force has brought the contiguous particles is retained, there being little discharge action between them, and therefore the *insulation* continues. But, what discharge there is, appears to be consequent upon that condition of the particles into which the induction throws them; and thus it is that ordinary insulation and conduction are closely associated together, or rather are extreme cases of one common condition.

1325. In ice or water we have a better conductor than spermaceti, and the phenomena of induction and insulation therefore quickly disappear, because conduction quickly follows upon the assumption of the inductive state. But let a plate of cold ice have metallic coatings on its sides, and connect one of these with a good electrical machine in work, and the other with the ground, and it then becomes easy to observe the phenomena of induction through the ice, by the electrical tension which can be obtained and continued on both the coatings (419. 426.) For although that portion of power which at one moment gave the inductive condition to the particles is at the next lowered by the consequent discharge due to the conductive act, it is succeeded by another portion of force from the machine to restore the inductive state. If the ice be converted into water, the same succession of actions can be just as easily proved, provided the water be distilled, and, (if the machine be not powerful enough) a voltaic battery be employed.

1326. All these considerations impress my mind strongly with the conviction, that insulation and ordinary conduction cannot be properly separated when we are examining into their nature; that is, into the general law or laws under which their phenomena are produced. They appear to me to consist in an action of contiguous particles, dependent on the forces developed in electrical excitement; these forces bring the particles into a state of tension or polarity, which constitutes both *induction* and *insulation*; and being in this state, the continuous particles have a power or capability of communicating their forces one to the other, by which they are lowered, and discharge occurs. Every body appears to discharge (444); but the possession of this capability in a *greater or smaller degree* in different bodies, makes them better or worse conductors, worse or better insulators; and both *induction* and *conduction* appear to be the same in their principle and action (1320.), except that in the latter an effect common to both is raised to the highest degree, whereas in the former it occurs in the best cases, in only an almost insensible quantity.

1327. That in our attempts to penetrate into the nature of electrical action, and to deduce laws more general than those we are at present acquainted with, we should endeavor to bring apparently opposite effects to stand side by side in harmonious arrangement, is an opinion of long standing, and sanctioned by the ablest philosophers. I hope, therefore, I may be excused the attempt to look at the highest cases of conduction as analagous to, or even the same in kind with, those of induction and insulation.

1328. If we consider the slight penetration of sulphur (1241. 1242.) or shell-lac (1234.) by electricity, or the feebler insulation sustained by spermaceti (1279. 1240.), as essential consequences and indications of their *conducting* power, then may we look on the resistance of metallic wires to the passage of electricity through them as *insulating* power. Of the numerous well known cases fitted to shew this resistance in what are called the perfect conductors, the experiments of Professor Wheatstone best serve my present purpose, since they were carried to such an extent as to shew that *time* entered as an element into the conditions of conduction* even in metals. When discharge was made through a copper wire 2640 feet in length, and 1-15th of an inch in diameter, so that the luminous sparks at each end of the wire, and at the middle, could be observed in the same place, the latter was found to be sensibly behind the two former in time, they being by the conditions of the experiment, simultaneous. Hence a proof of retardation; and what reason can be given why this retardation should not be of the same kind as that in spermaceti, or in lac, or sulphur? But as, in them, retardation is insulation, and insulation is induction, why should we refuse the same relation to the same exhibitions of force in the metals.

---

* Philosophical Transactions, 1834. p. 583.

1329. We learn from the experiment, that if *time* be allowed retardation is gradually overcome; and the same thing obtains for the spermaceti, the lac, and glass; give but time in proportion to the retardation, and the latter is at last vanquished. But if that be the case, and all the results are alike in kind, the only difference being in the length of time, why should we refuse to metals the previous inductive action, which is admitted to occur in the other bodies? The diminution of *time* is no negation of the action; nor is the lower degree of tension requisite to cause the forces to traverse the metal, as compared to that necessary in the cases of water, spermaceti, or lac. These differences would only point to the conclusion, that in metals the particles under induction can transfer their forces when at a lower degree of tension or polarity, and with greater facility than in the instances of the other bodies.

1330. Let us look at Mr. Wheatstone's beautiful experiment in another point of view. If, leaving the arrangement at the middle and two ends of the long copper wire unaltered, we remove the two intervening portions and replace them by wires of iron or platina, we shall have a much greater retardation of the middle spark than before. If, removing the iron, we were to substitute for it only five or six feet of water in a cylinder of the same diameter as the metal, we should have still greater retardation. If from water we passed to spermaceti, either directly or by gradual steps through other bodies, (even though we might vastly enlarge the bulk, for the purpose of evading the occurrence of a spark elsewhere (1331.) than at the three proper intervals,) we should have still greater retardation, until at last we might arrive, by degrees so small as to be inseparable from each other, at actual and permanent insulation.. What, then, is to separate the principle of these two extremes, perfect conduction and perfect insulation, from each other; since the moment we leave in the smallest degree perfection at either extremity, we involve the element of perfection at the opposite end? Especially too, as we have not in nature the case of perfection either at one extremity or the other, either of insulation or conduction.

1331. Again, to return to this beautiful experiment in the various forms which may be given to it: the forces are not all in the wire (after they have left the Leyden jar) during the whole time (1328.) occupied by the discharge; they are disposed in part through the surrounding dielectric under the well-known form of induction; and if that dielectric be air, induction takes place from the wire through the air to surrounding conductors, until the ends of the wire are electrically related through its length and discharge has occurred, i. e. for the *time* during which the middle spark is retarded beyond the others. This is well shewn by the old experiment, in which a long wire is so bent that two parts (Plate 2, fig. 1. *a. b.*) near its extremities shall approach within a short distance, as a quarter of an inch, of each other in the air. If the discharge of a Leyden jar, charged to a sufficient degree, be sent through such a wire, by far the largest portion of the electricity will pass as a spark across the air at the interval, and not by the metal. Does not the middle part of the wire, therefore, act here as an insulating medium, though it be of metal? and is not the spark through the air an indication of the tension (simultaneous with *induction*) of the electricity in the ends of this single wire? Why should not the wire and the air both be regarded at dielectrics; and the action at its commencement, and whilst there is tension, as an inductive action? If it acts through the contorted lines of the wire, so it also does in curved lines through air (1219. 1224.), and other insulating dielectrics (1228.); and we can apparently go so far in the analogy, whilst limiting the case to the inductive action only, as to shew that amongst insulating dielectrics some lead away the lines of force from others (1229.), as the wire will do from worse conductors, though in it the principal effect is no doubt due to the ready discharge between the particles whilst in a low state of tension. The retardation is for the time insulation; and it seems to me we may just as fairly compare the air at the interval *a*, *b*, (fig. 1.) and the wire in the circuit, as two bodies of the same kind and acting upon the same principles, as far as the first inductive phenomena are concerned, notwithstanding the different forms of discharge which ultimately follow,* as we may compare, according to Coulomb's investigations,† *different lengths* of different insulating bodies required to produce the same amount of insulating effect.

---

* These will be examined hereafter (1318, &c.)

† Memoires de l' Academie, 1785, p. 612, or Ency. Britann. First Supp. vol. 1, p. 611.

1332 This comparison is still more striking when we take into consideration the experiment of Mr. Harris, in which he stretched a fine wire across a glass globe, the air within being rarefied.* On sending a charge through the joint arrangement of metal and rare air, as much, if not more, electricity passed by the latter as by the former. In the air, rarefied as it was, there can be no doubt the discharge was preceded by induction (1284.); and to my mind all the circumstances indicate that the same was the case with the metal; that, in fact, both substances are dielectrics, exhibiting the same effects in consequence of the action of the same causes, the only variation being one of degree in the different substances employed.

1333. Judging on these principles, velocity of discharge through the *same wire* may be varied greatly by attending to the cirumstances which cause variations of discharge through spermaceti or sulphur. Thus, for instance, it must vary with the tension or intensity of the first urging force (1234. 1240.), which tension is charge and induction. So if the two ends of the wire, in Professor Wheatstone's experiment, were immediately connected with two large insulated metallic surfaces exposed to the air, so that the primary act of induction, after making the contact for discharge, might be in part removed from the internal portion of the wire at the first instant, and disposed for the moment on its surface jointly with the air and surrounding conductors, then I venture to anticipate that the middle spark would be more retarded than before; and if these two plates were the inner and outer coating of a large jar or a Leyden battery, then the retardation of that spark would be still greater.

1334. Cavendish was perhaps the first to shew distinctly that discharge was not always by one channel,† but, if several are present, by many at once. We may make these different channels of different bodies, and by proportioning their thicknesses and lengths, may include such substances as air, lac, spermaceti, water, protoxide of iron, iron and silver, and by *one* discharge make each convey its proportion of the electric force. Perhaps the air ought to be excepted, as its discharge by conduction is questionable at present; but the others may all be limited in their mode of discharge to pure conduction. Yet several of them suffer previous induction, precisely like the induction through the air, it being a necessary preliminary to their discharging action. How can we therefore separate any one of these bodies from the others, as to the *principles and mode* of insulating and conducting, except by mere degree? All seem to me to be dielectrics acting alike, and under the same common laws.

---

* Philosophical Transactions, 1834, p. 242.

† Philosophical Transactions, 1776, p. 197.

1335. I might draw another argument in favor of the general sameness, in nature and action, of good and bad conductors (and all the bodies I refer to are conductors more or less), from the perfect equipoise in action of very different bodies when opposed to each other in magneto-electric inductive action, as formerly described (213.), but am anxious to be as brief as is consistent with the clear examination of the probable truth of my views.

1336. With regard to the possession by the gases of any conducting power of the simple kind now under consideration, the question is a very difficult one to determine at present. Experiments seem to indicate that they do insulate certain low degrees of tension perfectly, and that the effects which may have appeared to be occasioned by *conduction* have been the result of the carrying power of the charged particles, either of the air or of dust, in it. It is equally certain, however, that with higher degrees of tension or charge they discharge to one another, and that is conduction. If they possess the power of insulating a certain low degree of tension continuously and perfectly, such a result may be due to their peculiar physical state, and the condition of separation under which their particles are placed. But in that, or in any case, we must not forget the fine experiments of Cagniard de la Tour,* in which he has shewn that liquids and their vapors can be made to pass gradually into each other, to the entire removal of any marked distinction of the two states. Thus, hot dry steam and cold water pass by insensible gradations into each other; yet the one is amongst the gases as an insulator, and the other a comparatively good conductor. As to conducting power, therefore, the transition from metals even up to gases is gradual; substances make but one series in this respect, and the various cases must come under one condition and law (444.) The specific differences of bodies as to conducting power only serve to strengthen the general argument that conduction, like insulation, is a result of induction, and is an action of contiguous particles.

1337. I might go on now to consider induction and its concomitant, *conduction*, through mixed dielectrics, as, for instance, when a charged body, instead of acting across air to a distant uninsulated conductor, acts jointly through it and an interposed insulated conductor. In such a case, the air and the conducting body are the mixed dielectrics; and the latter assumes a polarized condition as a mass, like that which my theory assumes *each particle* of the air to possess at the same time. But I fear to be tedious in the present condition of the subject, and hasten to the consideration of other matter.

---

* Annales de Chimie, xxi. pp. 127. 178. or Quarterly Journal of Science, xv. 145.

1338. To sum up, in some degree, what has been said, I look upon the first effect of an excited body upon neighbouring matters to be the production of a polarized state of their particles, which constitutes *induction*; and this arises from its action upon the particles in immediate contact with it, which again act upon those contiguous to them, and thus the forces are transferred to a distance. If the induction remain undiminished, then perfect insulation is the consequence; and the higher the polarized condition which the particles can acquire or maintain, the higher is the intensity which may be given to the acting forces. If, on the contrary, the contiguous particles, upon acquiring the polarized state, have the power to communicate their forces, then conduction occurs, and the tension is lowered, conduction being a distinct act of discharge between neighbouring particles. The lower the state of tension at which this discharge between the particles of a body takes place, the better conductor is that body. In this view, insulators may be said to be bodies whose particles can retain the polarized state: whilst conductors are those whose particles cannot be permanently polarized. If I be right in my view of induction, then I consider the reduction of these two effects (which have been so long held distinct) to an action of contiguous particles obedient to one common law, as a very important result; and, on the other hand, the identity of character which the two acquire when viewed by the theory (1326.), is additional presumptive proof in favor of the correctness of the latter.

1339. That heat has great influence over simple conduction is well known (445.), its effect being, in some cases, almost an entire change of the characters of the body (432. 1340.) Harris has, however, shewn that it in no respect affects gaseous bodies, or at least air;* and Davy has taught us that, as a class, metals have their conducting power *diminished* by it.†

1340. I formerly described a substance, sulphuret of silver, whose conducting power was increased by heat (433. 437. 438.); and I have since then met with another as strongly affected in the same way: this is fluoride of lead. When a piece of that substance, which had been fused and cooled, was introduced into the circuit of a voltaic battery, it stopped the current. Being heated, it acquired conducting powers before it was visibly red hot in daylight; and even sparks could be taken against it whilst still solid. The current alone then raised its temperature (as in the case of sulphuret of silver) until it fused, after which it seemed to conduct as well as the metallic vessel containing it; for whether the wire used to complete the circuit touched the fused fluoride only, or was in contact with the platina on which it was supported, no sensible difference in the current was observed. During all the time there was scarcely a trace of decomposing action on the fluoride, and what did occur seemed referable to the air and moisture of the atmosphere, and not to electrolytic action.

---

* Philosophical Transactions, 1834, p. 230.

† Ibid. 1821, p. 431.

1341. I have now very little doubt that periodide of mercury (414. 448. 691.) is a case of the same kind, and also corrosive sublimate (692). I am also inclined to think, since making the above experiments, that the anomalous action of the protoxide of antimony, formerly observed and described (693. 801.), may be referred in part to the same cause.

1342. I have no intention at present of going into the particular relation of heat and electricity, but we may hope hereafter to discover by experiment the law which probably holds together all the above effects with those of the *evolution* and the *disappearance* of heat by the current, and the striking and beautiful results of thermo-electricity, in one common bond.

¶ viii. *Electrolytic Discharge.*

1343. I have already expressed in a former paper (1164.) the view by which I hope to associate ordinary induction and electrolyzation. Under that view, the discharge of electric forces by electrolyzation is rather an effect superadded, in a certain class of bodies, to those already described as constituting induction and insulation, than one independent of, and distinct from, these phenomena.

1344. Electrolytes, as respects their insulating and conducting forces, belong to the general category of bodies (1320. 1334.); and if they are in the solid state (as nearly all can assume that state), they retain their place, presenting then no new phenomenon (426, &c.); or if one occur being in so small a proportion as to be almost unimportant. When liquefied, they also belong to the same list whilst the electric intensity is below a certain degree; but at a given intensity (910. 912. 1007.), fixed for each, and very low in all known cases, they play a new part, causing discharge in proportion (783.) to the developement of certain chemical effects of combination and decomposition; and at this point, move out from the the general class of insulators and conductors, to form a distinct one by themselves. The former phenomena have been considered (1320. 1338.); it is the latter which have now to be revised, and used as a test of the proposed theory of induction.

1345. The theory assumes, that the particles of the dielectric (now an electrolyte) are in the first instance brought, by ordinary inductive action, into a polarized state, and raised to a certain degree of tension or intensity before discharge commences; the inductive state being, in fact, a *necessary preliminary* to discharge. By taking advantage of those circumstances which bear upon the point, it is not difficult to increase the tension indicative of this state of induction, and so make the state itself more evident. Thus, if distilled water be employed,and a long narrow portion of it placed between the electrodes of a powerful voltaic battery, we have at once indications of the intensity which can be sustained at these electrodes by the inductive action through the water as a dielectric, for sparks may be obtained, gold leaves diverged, and Leyden bottles charged at their wires. The water is in the condition of the spermaceti (1322. 1323.), a bad conductor and a bad insulator; but what it does insulate is by virtue of inductive action, and that induction is the preparation for, and precursor of, discharge (1338.)

1346. The induction and tension which appear at the limits of the portion of water in the direction of the current, are only the sums of the induction and tension of the contiguous particles between those limits; and the limitation of the inductive tension, to a certain degree shews (time entering in each case as an important element of the result), that when the particles have acquired a certain relative state, *discharge*, or a transfer of forces equivalent to ordinary conduction, takes place.

1347. In the inductive condition assumed by water before discharge comes on, the particles polarized are the particles of the *water*, that being the dielectric used; but the discharge between particle and particle is not, as before, a mere interchange of their powers or forces at the polar parts, but an actual separation of them into their two elementary particles, the oxygen travelling in one direction, and carrying with it its amount of the force it had acquired during the polarization, and the hydrogen doing the same thing in the other direction, until they each meet the next approaching particle, which is in the same electrical state with that they have left, and by association of their forces with it, produce what constitutes discharge. This part of the action may be regarded as a carrying one (1319.), performed by the constituent particles of the dielectric. The latter is always a compound body (664. 823.); and by those who have considered the subject and are acquainted with the philosophical view of transfer which was first put forth by Grotthuss,* its particles may easily be compared to a series of metallic conductors under inductive action, which, whilst in that state, are divisible into these elementary movable halves.

1348. Electrolytic discharge depends, of necessity, upon the non-conduction of the dielectric as a whole, and there are two steps or acts in the process: first a polarization of the molecules of the substance, and then a lowering of the forces by the separation, advance in opposite directions, and recombination of the elements of the molecules, they being, as it were, the halves of the originally polarized conductors or particles.

1349. These views of the decomposition of electrolytes and the consequent effect of discharge, which, as to the particular case, are the same with those of Grotthuss (481.) and Davy (482.), though they differ from those of Biot (487.) De la Rive (490.), and others, seem to me to be fully in accordance not merely with the theory I have given of induction generally (1165.), but with all the known *facts* of common induction, conduction, and electrolytic discharge; and in that respect help to confirm, in my mind, the truth of the theory set forth. The new mode of discharge which electrolyzation presents must surely be an evidence of the *action of contiguous particles;* and as this appears to depend directly upon a previous inductive state, which is the same with common induction, it greatly strengthens the argument which refers induction in all cases to an action of contiguous particles also (1295, &c.).

1350. As an illustration of the condition of the polarized particles in a dielectric under induction, I may describe an experiment. Put into a glass vessel some clear rectified oil of turpentine, and introduce two wires passing through glass tubes where they are at the surface of the fluid, and terminating either in balls or points. Cut some very clean dry white silk into small particles, and put these also into the liquid; then electrify one of the wires by an ordinary machine and discharge by the other. The silk will immediately gather from all parts of the liquid, and form a band of particles reaching from wire to wire, and if touched by a glass rod will shew considerable tenacity; yet the moment the supply of electricity ceases, the band will fall away and disappear by the dispersion of its parts. The *conduction* by the silk is in this case very small; and after the best examination I could give to the effects, the impression on my mind is, that the adhesion of the whole is due to the polarity which each filament acquires, exactly as the particles of iron between the poles of a horse-shoe magnet are held together in one mass by a similar disposition of forces. The particles of silk therefore represent to me the condition of the molecules of the dielectric itself, which I assume to be polar, just as that of the silk is. In all cases of conductive discharge the contiguous polarized particles of the body are able to effect a neutralization of their forces with greater or less facility, as the silk does also in a very slight degree. Further we are not able to carry the parallel, except in imagination; but if we could divide each particle of silk into two halves, and let each half travel until it met and united with the next half in an opposite state, it would then exert its carrying power (1347.), and so far represent electrolytic discharge.

---

* Annals de Chimie, lxviii. 60 and lxiii. 20.

1351. Admitting that electrolytic discharge is a consequence of previous induction, then how evidently do its numerous cases point to induction in curved lines (1216.), and to the divergence or lateral action of the lines of inductive force (1231.), and so strengthen that part of the general argument in the former paper! If two balls of platina, forming the electrodes of a voltaic battery, are put into a large vessel of dilute sulphuric acid, the whole of the surfaces are covered with the respective gases in beautifully regulated proportions, and the mind has no difficulty in conceiving the direction of the curved lines of discharge, and even the intensity of force of the different lines by the quantity of gas evolved upon the different parts of the surface. Hence the general effects of diffusion; the appearance of the anions or cathions round the edges on the further side of the electrodes when in the form of plates; the manner in which the current or discharge will follow all the forms of electrolyte, however contorted. Hence the effects which Nobili has so well examined and described* in his papers on the distribution of currents in conducting masses. All these effects indicate the direction of the currents or discharges which occur in and through the dielectrics, and these are in every case *preceded* by equivalent inductive actions of the contiguous particles.

---

* Bibliotheque Universelle, 1835, lix. 263, 416.

1352. Hence also the advantage, when the exciting forces are weak or require assistance, of enlarging the mass of the electrolyte; of increasing the size of the electrodes; of making the coppers surround the zincs:—all is in harmony with the view of induction which I am endeavoring to examine; I do not perceive as yet one fact against it.

1353. There are many points of *electrolytic discharge* which ultimately will require to be very closely considered, though I can but slightly touch upon them. It is not that, as far as I have investigated them, they present any contradiction to the view taken (for I have carefully, though unsuccessfully, sought for such cases), but simply want of time as yet to pursue the inquiry, which prevents me from entering upon them here.

1354. One point is, that different electrolytes or dielectrics require different initial intensities for their decomposition (912). This may depend upon the degree of polarization which the particles require before electrolytic discharge commences. It is in direct relation to the chemical affinity of the substances concerned; and will probably be found to have a relation or analogy to the specific inductive capacity of different bodies (1252. 1296.). It thus promises to assist in causing the great truths of those extensive sciences, which are occupied in considering the forces of the particles of matter, to fall into much closer order and arrangement than they have heretofore presented.

1355. Another point is, the facilitation of electrolytic conducting power or discharge by the addition of substances to the dielectric employed. This effect is strikingly shewn where water is the body whose qualities are improved, but, as yet, no general law governing all the phenomena has been detected. Thus some acids, as the sulphuric, phosphoric, oxalic, and nitric, increase the power of water enormously; whilst others, as the tartaric and citric acids, give but little power; and others, again, as the acetic and boracic acids, do rot produce a change sensible to the voltameter (739.). Ammonia produces no effect, but its carbonate does. The caustic alkalies and their carbonates produce a fair effect. Sulphate of soda, nitre (753.), and many soluble salts produce much effect. Percyanide of mercury and corrosive sublimate produce no effect; nor does iodine, gum, or sugar, the test being a voltameter. In many cases the added substance is acted on either directly or indirectly, and then the phenomena are more complicated; such substances are muriatic acid (758.), the soluble protochlorides, (766.), and iodides (769.), nitric acid (752.), &c. In other cases the substance added is not, when alone, subject to, or, a conductor of the powers of the voltaic battery, and yet both gives and receives power when associated with water. M. de la Rive has pointed this result out in sulphurous acid,* iodine and bromine†; the chloride of arsenic produces the same effect. A far more striking case, however, is presented by that very influential body sulphuric acid (681.), and probably phosphoric acid also is in the same peculiar relation.

1356. It would seem in the cases of those bodies which suffer no change themselves, as sulphuric acid (and perhaps in all), that they affect water in its conducting power only as an electrolyte; for whether little or much improved, the decomposition is proportionate to the quantity of electricity passing (727. 730.), and the transfer is therefore due to electrolytic discharge. This is in accordance with the fact already stated as regards water (984.) that the conducting power is not improved for electricity of force below the electrolytic intensity of the substance acting as the dielectric; but both facts (and some others) are against the opinion which I formerly gave, that the power of salts, &c, might depend upon their assumption of the liquid state by solution in the water employed (410.). It occurs to me that the effect may perhaps be related to, and have its explanation in differences of specific inductive capacities.

1357. I have described in the last paper, cases, where shell-lac was rendered a conductor by absorption for ammonia (1294.). The same effect happens with muriatic acid; yet both these substances, when gaseous, are non-conductors; and the ammonia, also when in strong solution (748.). Mr. Harris has mentioned instances‡ in which the conducting power of metals is seriously altered by a very little alloy. These may have no relation to the former cases, but nevertheless should not be overlooked in the general investigation which the whole question requires.

1358. Nothing is perhaps more striking in that class of dielectrics which we call electrolytes, than the extraordinary and almost complete suspension of their peculiar mode of effecting discharge when they are rendered *solid* (380, &c.), even though the intensity of the induction acting through them may be increased a hundred fold or more (419.). It not only establishes a very general relation between the physical properties of these bodies and electricity acting by induction through them, but draws both their physical and chemical relations so near together, as to make us hope we shall shortly arrive at the full comprehension of the influence they mutually possess over each other.

---

* Quarterly Journal, xxvii. 407. or Bibliotheque Universelle, xl. 205. Kemp says sulphurous acid is a very good conductor, Quarterly Journal, 1831, p. 613.

† Quarterly Journal, xxiv. 465, or Annales de Chimie, xxxv. 161.

‡ Philosophical Transactions, 1827, p 22.

¶ ix *Disruptive discharge and insulation.*

1359. The next form of discharge has been distinguished by the adjective *disruptive* (1319.), as it in every case displaces more or less the particles amongst and across which it suddenly breaks. I include under it, discharge in the form of sparks, brushes and glow (1405.), but exclude the cases of currents of air, fluids &c, which, though frequently accompanying the former, are essentially distinct in their nature.

1360. The conditions requisite for the production of an electric spark in its simplest form are well known. An insulating dielectric must be interposed between two conducting surfaces in opposite states of electricity, and then if the actions be continually increased in strength, or otherwise favored, either by exalting the electric state of the two conductors, or bringing them nearer to each other, or diminishing the density of the dielectric, a *spark* at last appears, and the two forces are for the time annihilated, for *discharge* has occurred.

1361. The conductors (which may be considered as the termini of the inductive action) are in ordinary cases most generally metals, whilst the dielectrics usually employed are common air and glass. In my view of induction, however, every dialectric becomes of importance, for as the results are considered essentially dependent on these bodies, it was to be expected that differences of action, never before suspected, would be evident upon close examination, and so at once give fresh confirmation of the theory, and open new doors of discovery into the extensive and varied fields of our science. This hope was especially entertained with respect to the gases, because of their high degree of insulation, their uniformity in physical condition, and great difference in chemical properties.

1362. All the effects prior to the discharge are inductive; and the degree of tension which it is necessary to attain before the spark passes is, therefore, in the examination I am now making of the new view of induction, a very important point. It is the limit of the influence which the dielectric exerts in resisting discharge; it is a measure, consequently, of the conservative power of the dielectric, which in its turn may be considered as becoming a measure, and therefore a representative of the intensity of the electric forces in activity.

1363. Many philosophers have examined the circumstances of this limiting action in air, but, as far as I know, none have come near Mr. Harris as to the accuracy with, and the extent to, which he has carried on his investigations.* Some of his results I must very briefly notice, premising that they are all obtained with the use of air as the *dielectric* between the conducting surfaces.

1364. First as to the *distance* between the two balls used, or in other words, the *thickness* of the dielectric across which the induction was sustained. The quantity of electricity, measured by a unit jar or otherwise on the same principle with the unit jar, in the charged or inductive ball, necessary to produce spark discharge, was found to vary exactly with the distance between the balls, or between the discharging points, and that under very varied and exact forms of experiment.†

1365. Then with respect to variation in the *pressure or density* of the air. The quantities of electricity required to produce discharge across a *constant* interval varied exactly with variations of the density; the quantity of electricity and density of the air being in the same simple ratio. Or, if the quantity was retained the same, whilst the interval and the density of the air were varied, then these were found in the inverse simple ratio of each other, the same quantity passing across twice the distance with air rarefied to one half.‡

1366. It must be remembered that these effects take place without any variation of the inductive force by condensation or rarefaction of the air. That force remains the same in air,§ and in all gases (1284. 1292), whatever their rarefaction may be.

1367. Variation of the *temperature* of the air produced no variation of the quantity of electricity required to cause discharge across a given interval.||

Such are the general results, which I have occasion for at present, obtained by Mr. Harris, and they appear to me to be unexceptionable.

1368. In the theory of induction founded upon a molecular action of the dielectric, we have to look at the state of that body principally for the cause and determination of the above effects. Whilst the induction continues, it is assumed that the particles of the dielectric are in a certain polarized state, the tension of this state rising higher in each particle as the induction is raised to a higher degree, either by approximation of the inducing surfaces, variations of form, increase of the original force, or other means; until at last, the tension of the particles having reached the utmost degree which they can sustain without subversion of the whole arrangement, discharge immediately after takes place.

---

* Philosophical Transactions, 1834, p. 225. † Ibid. ‡ Ibid. p. 229.
§ Ibid. pp. 237, 244. || Ibid. p. 230.

1369. The theory does not assume, however, that *all* the particles of the dielectric subject to the inductive action are affected to the same amount, or acquire the same tension. What has been called the lateral action of the lines of inductive force (1231. 1297.), and the diverging and occasionally curved form of these lines is against such a notion. The idea is, that any section taken through the dielectric across the lines of inductive force, and including *all of them*, would be equal, in the sum of the forces, to the sum of the forces in any other section; and that, therefore, the whole amount of tension for each such section would be the same.

1370. Discharge probably occurs, not when all the particles have attained to a certain degree of tension, but when that particle which is most affected has been exalted to the subverting or turning point (1410.). For though *all* the particles in the line of induction resist charge, and are associated in their actions so as to give a sum of resisting force, yet when any one is brought up to the overturning point, *all* must give way in the case of a spark between ball and ball. The breaking down of that one must of necessity cause the whole barrier to be overturned, for it was at its utmost degree of resistance when it possessed the aiding power of that one particle, in addition to the power of the rest, and the power of that one is now lost. Hence *tension* or *intensity*[*] may, according to the theory, be considered as represented by the particular condition of the particles, or the amount in them of forced variation from their normal state (1298. 1368.)

1371. The whole effect produced by a charged conductor on a distant conductor, insulated or not, is by my theory assumed to be due to an action propagated from particle to particle of the intervening and insulating dielectric, the particles being considered as thrown for the time being into a forced condition, from which they endeavor to return to their normal or natural state. The theory, therefore, seems to supply an easy explanation of the influence of *distance* in affecting induction (1303. 1364.). As the distance is diminished induction increases; for there are then fewer particles in the line of inductive force to oppose their resistance to the assumption of the forced or polarized state, and *vice versâ*. Again, as the distance diminishes, discharge across happens with a lower charge of electricity; for if, as in Harris's experiments (1364.), the interval be diminished to one half, then half the electricity required to discharge across the first interval is sufficient to strike across the second; and it is evident, also, that at that time there are only half the number of interposed molecules uniting their forces to resist the discharge.

---

[*] See Harris on proposed particular meaning of these terms, Philosophical Transactions, 1834, p. 222.

1372. The effect of enlarging the conducting surfaces which are opposed to each other in the act of induction, is, if the electricity be limited in its supply, to lower the intensity of action; and this follows as a very natural consequence from the increased area of the dielectric across which the induction is effected. For by diffusing the inductive action, which at first was exerted through one square inch of sectional area of the dielectric, over two or three square inches of such area, twice or three times the number of molecules of the dielectric are brought into a polarized condition, and employed in sustaining the inductive action, and consequently the tension belonging to the smaller number on which the limited force was originally accumulated, must fall in a proportionate degree.

1373. For the same reason diminishing these opposing surfaces must increase the intensity up to the condition even of their becoming points. But in this case, the tension of the particles of the dielectric next the points is higher than that of particles midway, because of the lateral action and consequent bulging, as it were, of the lines of inductive force at the middle distance (1369.).

1374. The more exalted effects of induction on a point *p*, or any small surface, as the rounded end of a rod, opposed to a large surface, as that of a ball or plate, than when it is opposed to another point or end at the same distance, falls into harmonious relation (1302.). For in the latter case, the small surface *p* is effected only by those particles which are brought into the inductive condition by the equally small surface of the opposed conductor, whereas when that is a ball or plate the lines of inductive force from the latter are concentrated, as it were, upon the end *p*. Now though the molecules of the dielectric against the large surface may have a much lower state of tension than those against the similar smaller surface, yet they are also far more numerous, and, as the lines of inductive force converge towards a point, are able to communicate to the particles contained in any cross section (1369.) nearer the small surface an amount of tension equal to their own, and consequently much higher for each individual particle; so that, at the surface of the smaller conductor, the tension of a particle rises much, and if that conductor were to terminate in a point, the tension would rise to an infinite degree, except that it is limited, as before (1368.), by discharge. The nature of the discharge from small surfaces and points under induction will be resumed hereafter (1425. &c).

1375. *Rarefaction* of the air does not alter the *intensity* of inductive action (1284. 1287.); nor is there any reason, as far as I can perceive, why it should. If the quantity of electricity and the distance remain the same, and the air be rarefied one half, then, though one half of the particles of the dielectric are removed, the other half assume a double degree of tension in their polarity, and therefore the inductive forces are balanced, and the result remains unaltered as long as the induction and insulation are sustained. But the case of *discharge* is very different; for as there are only half the number of dielectric particles in the rarefied atmosphere, so these are brought up to the discharging intensity by half the former quantity of electricity; discharge, therefore, ensues, and such a consequence of the theory is in perfect accordance with Mr. Harris's results (1365.).

1376. The *increase* of electricity required to cause discharge over the same distance, when the pressure of the air or its density is increased, flows in a similar manner, and on the same principle, from the molecular theory.

1377. Here I think my view of induction has a decided advantage over others, especially over that which refers the retention of electricity on the surface of conductors in air to the *pressure of the atmosphere.* The latter, is the view which, being adopted by Poisson and Biot,* is also, I believe, that generally received; and it relates two such dissimilar things, as the ponderous air and the subtil and even hypothetical fluid or fluids of electricity, by gross mechanical relations; by the bonds of mere static pressure. My theory, on the contrary, sets out at once by connecting the electric forces with the particles of matter; it derives all its proofs, and even its origin in the first instance, from experiment; and then, without any further assumption, seems to offer at once a full explanation of these and many other singular, peculiar, and, I think, heretofore unrelated effects.

1378. An important assisting experimental argument may here be adduced, derived from the difference of specific inductive capacity of different dielectrics (1269. 1274. 1278.). Consider an insulated sphere electrified positively and placed in the centre of another and larger sphere uninsulated, a uniform dielectric, as air, intervening. The case is really that of my apparatus (1187.), and also, in effect, that of any ball electrified in a room and removed to some distance from irregularly formed conductors. Whilst things remain in this state the electricity is distributed (so to speak) uniformly over the surface of the electrified sphere. But introduce such a dielectric as sulphur or lac, into the space between the two conductors on one side only, or opposite one part of the inner sphere, and immediately the electricity on the latter is diffused unequally (1229. 1270. 1309.), although the form of the conducting surfaces, their distances, and the *pressure* of the atmosphere remain perfectly unchanged.

---

* Ency. Britann. Supplement, vol. iv. Article Electricity, pp. 76, 81, &c.

1379. Fusinieri took a different view from that of Poisson, Biot, and others, of the reason why rarefaction of air caused easy diffusion of electricity. He considered the effect as due to the removal of the *obstacle* which the air presented to the expansion of the substances, from which the electricity passed.* But platina balls shew the phenomena in vacuo as well as volatile metals and other substances; besides which, when the rarefaction is very considerable, the electricity passes with scarcely any resistance, and the production of no sensible heat; so that I think Fusinieri's view of the matter is likely to gain but few assents.

1380. I have no need to remark upon the discharging or collecting power of flame or hot air. I believe, with Harris, that the mere heat does nothing (1367.), the rarefaction only being influential. The effect of rarefaction has been already considered generally (1375.); and that caused by the heat of a burning light, with the pointed form of the wick, and the carrying power of the carbonaceous particles which for the time are associated with it, are fully sufficient to account for all the effects.

1381. We have now arrived at the important question, how will the inductive tension requisite for insulation and disruptive discharge be sustained in gases, which, having the same physical state and also the *same pressure* and the *same temperature* as *air*, differ from it in specific gravity, in chemical qualities, and it may be in peculiar relations, which not being as yet recognised, are purely electrical (1361.)?

1382. Into this question I can enter now only as far as is essential for the present argument, namely, that insulation and inductive tension do not depend merely upon the charged conductors employed, but also, and essentially, upon the interposed dielectric, in consequence of the molecular action of its particles.

---

* Bib. Univ. 1831. xlviii. 375.

1383. A glass vessel *a* (fig. 13.)* was ground at the top and bottom so as to be closed by two ground brass plates, *b* and *c*; *b* carried a stuffing box, with a sliding rod *d* terminated by a brass ball *s* below, and a ring above. The lower plate was connected with a foot, stop-cock, and socket, *e*, *f* and *g*; and also with a brass ball *l*, which by means of a stem attached to it and entering the socket *g*, could be fixed at various heights. The metallic parts of this apparatus were not varnished, but the glass was well covered with a coat of shell-lac previously dissolved in alcohol. On exhausting the vessel at the air-pump, it could be filled with any other gas than air, and, in such cases, the gas so passed in was dried whilst entering by fused chloride of calcium.

1384. The other part of the apparatus consisted of two insulating pillars, *h* and *i*, to which were fixed two brass balls, and through these passed two sliding rods, *k* and *m*, terminated at each end by brass balls; *n* is the end of an insulated conductor, which could be rendered either positive or negative from an electrical machine; *o* and *p* are wires connecting it with the two parts previously described, and *q* is a wire which, connecting the two opposite sides of the collateral arrangements, also communicates with a good discharging train *r* (292.).

1385. It is evident that the discharge from the machine electricity may pass either between *s* and *l*, or S and L. The regulation adopted in the first experiments was to keep *s* and *l* with their distance *unchanged*, but to introduce first one gas and then another into the vessel *a*, and then balance the discharge at the one place against that at the other; for by making the interval at *u* sufficiently small, all the discharge would pass there, or making it sufficiently large it would all occur at the interval *v* in the receiver. On principle it seemed evident, that in this way the varying interval *u* might be taken as a measure, or rather indication of the resistance to discharge through the gas at the constant interval *v*. The following are the constant dimensions.

Ball *s*	0·93 of an inch.
Ball S	0·96 of an inch.
Ball *l*	2·02 of an inch.
Ball L	1·95 of an inch.
Interval *v* . . .	0·62 of an inch.

1386. On proceeding to experiment it was found that when air or any gas was in the receiver *a*, the interval *u* was not a fixed one; it might be altered through a certain range of distance, and yet sparks pass either there or at *v* in the receiver. The extremes were therefore noted, i.e., the greatest distance short of that at which the discharge *always* took place at *v* in the gas, and the least distance short of that at which it *always* took place at *u* in the air. Thus, with air in the receiver, the extremes at *u* were 0·56 and 0·79 of an inch, the range of 0·23 being one at which sparks passed occasionally either at one interval or the other.

---

* The drawing is to a scale of 1·6.

1387. The small balls *s* and S could be rendered either positive or negative from the machine, and as gases were expected and were found to differ from each other in relation to this change, the results obtained under these differences of charge were also noted.

1388. The following is a table of results; the gas named is that in the vessel *a*. The smallest, greatest, and mean interval at *u* in air is expressed in parts of an inch, the interval *v* being constantly 0·62 of an inch.

	Smallest.	Greatest.	Mean.
Air, *s* and S, pos.	0·60	0·79	0·695
Air, *s* and S, neg.	0·59	0·68	0·635
Oxygen, *s* and S, pos.	0·41	0·60	0·505
Oxygen, *s* and S, neg.	0·50	0·52	0·510
Nitrogen, *s* and S, pos.	0·55	0·68	0·615
Nitrogen, *s* and S, neg.	0·59	0·70	0·645
Hydrogen, *s* and S pos.	0·30	0·44	0·370
Hydrogen, *s* and S neg.	0·25	0·30	0·275
Carbonic acid, *s* and S pos.	0·56	0·72	0·640
Carbonic acid, *s* and S neg.	0·58	0·60	0·590
Olefiant gas, *s* and S pos.	0·64	0·86	0·750
Olefiant gas, *s* and S neg.	0·69	0·77	0·730
Coal gas, *s* and S pos.	0·37	0·61	0·490
Coal gas, *s* and S neg.	0·47	0·58	0·525
Muriatic acid gas, *s* and S pos.	0·89	1·32	1·105
Muriatic acid gas, *s* and S neg.	0·67	0·75	0·720

1389. The above results were all obtained at one time. On other occasions other experiments were made, which gave generally the same results as to order, though not as to numbers. Thus:

Hydrogen, *s* and S pos.	0·23	0·57	0·400
Carbonic acid, *s* and S pos.	0·51	1·05	0·780
Olefiant gas, *s* and S pos.	0·66	1·27	0·965

I did not notice the difference of the barometer on the days of experiment.

1390. One would have expected only two distances, one for each interval, for which the discharge might happen either at one or the other; and that the least alteration of either would immediately cause one to predominate constantly over the other. But that under common circumstances is not the case. With air in the receiver, the variation amounted to 0·2 of an inch nearly on the smaller interval of 0·6, and with muriatic acid gas, the variation was above 0·4 on the smaller interval of 0·9. Why is it that when a fixed interval (the one in the receiver) will pass a spark that cannot go across 0·6 of air at one time, it will immediately after, and apparently under exactly similar circumstances, not pass a spark that can go across 0·8 of air?

1391. It is probable that part of this variation will be traced to particles of dust in the air drawn into and about the circuit. I believe also that part depends upon a variable charged condition of the surface of the glass vessel *a*. That the whole of the effect is not traceable to the influence of circumstances in the vessel *a*, may be deduced from the fact, that when sparks occur between balls in free air they frequently are not straight, and often pass otherwise than by the shortest distance. These variations in air itself, and at different parts of the very same balls, shew the presence and influence of circumstances which are calculated to produce effects of the kind now under consideration.

(To be Continued.)

XIII.—*Experiments relative to the propagation of Caloric in a Metallic Bar.* By M. H. Schroeder, Esq. Professor of Natural Philosophy at Soleure. Communicated by the Author.

It is well known that artificers in metal, advance generally enough the curious fact, that a metallic bar one end of which is placed in an ardent furnace, and held by the other end until the heat has begun to be sensibly felt by the hand, will suddenly cause a very considerable elevation of temperature to be felt, immediately on removing out of the fire the heated extremity of the bar, in order to expose it to the cold air, or even to plunge it in cold water. I know of no other experiments on the subject than those of M. Fischer, of Breslaw, (Proggend. Ann. 19,—507), and those of Professor Mousson, of Zurich, who communicated them last year in the section of Natural Philosophy, at the meeting of Swiss Naturalists, held at Neufchatel. These two philosophers seem not to doubt of the existence of the fact in question, but the former is or seems willing to attribute it to a variation in the conducting faculty of metals according to their temperature, whilst M. Mousson considers it as proving a developement of specific caloric, produced by a molecular compression which would be caused by the sudden cooling to which the metal was subjected.

Having several reasons to doubt whether the truth of the fact or phenomena had been rigorously established by experiments made up to the present time, I undertook to convince myself, by decisive and direct experiments, whether the phenomena had existed or not, and in the latter case, to shew that a false explanation of other phenomena on the part of these philosophers, and a common illusion on the part of the artisans, are the only reasons for advancing a position so paradoxical. Now such happens to be the result at which I have arrived in making use of the means offered to us by thermo-electric forces of measuring the changes of temperature; and I have given to the experiments which I am about to describe, another importance besides that of having served to demonstrate in a decisive manner, that the phenomena now in question has no existence whatever.

I obtained a galvanometer of a very delicate and sensitive construction used in measuring thermo-electric currents, on the model of that which Fechner has recommended as most convenient for that purpose. It consists of a large band of copper, folded once round an astatic system consisting of two magnetized needles,* which could oscillate freely on a graduated circle from degree to degree. A simple element of bismuth and antimony, communicating with the reservoirs of mercury, into which the ends of the band of copper are plunged, gives to the needle a continued deviation from 80° to 85 degrees, on applying solely the temperature of the hand to the point of the solder. I afterwards soldered even at the soldering point of the element of bismuth and antimony, one of the extremities of a bar of another metal, long enough to allow the free extremity to be exposed to the action of heat, without any direct influence being exercised on the solder of the section of bismuth and antimony, then keeping the eye invariably fixed on the needle, I awaited the time when the temperature of the bar would become permanent throughout its whole extent, which was easily observable by a constant deviation of the galvanometer. Immediately on the arrival of this moment, I removed, by an assistant, the source of heat, and plunged the red hot extremity into cold water. If the sudden cooling of the red hot extremity of the bar had power to produce in any manner whatever an augmentation of the temperature of the other extremity, it is clear that this heat would be communicated to the point of the solder, and that the needle, before making a retrograde movement, which would be the effect of the cooling having commenced, ought first to manifest an instantaneous deviation, in the contrary direction, thereby indicating an augmentation of temperature. But what was the change in the circumstances of this experiment? never have I been able to perceive the least sign of an effect approaching to this. The needle, on the contrary, remained each trial, immovable during some time, until perhaps the cooling had been communicated to the point of the solder in the ordinary manner, and then began immediately to indicate the direct effect of cooling. I have repeated these experiments, under very different circumstances with bands, with bars, with wires, with masses of different forms both of iron, of copper, of zinc, of brass, &c. giving them different lengths from two feet to half an inch; making use of sources of heat of a temperature more or less elevated; producing the sudden cooling sometimes by the action of the air, sometimes by water, and at others by mercury; and in making use of the thermo-electric element, a combination of any two metals; but in no case did the needle indicate to me an augmentation of temperature after the the operation of cooling. The result of these experiments being absolutely negative, I judge it not necessary to describe them in detail; but I conclude therefrom with certainty that the fact in question does not exist.

---

* No upper needle ought to be *above* the upper side of the coil, and the lower needle within the coil.—Edit.

This point once established, it remained for me to repeat conveniently the experiments of M. Mousson, in order to give the true explanation to the phenomena which he has observed. Now I am convinced, by a careful examination, that the explanation of them coincides with that of a well known phenomenon, which may be observed easily by any thermometer in any degree sensitive, namely, that if the instrument be suddenly withdrawn from the source of heat, at the *first* moment there will be seen a rapid elevation, after which the depression will commence. We know that this circumstance is owing to the sudden contraction of the small bowl of glass, and to its diminution of volume, which surpasses during the first moments the effect of the depression of temperature on the whole column of mercury. The phenomena which M. Mousson has observed are quite analogous to this, and it appears to me that he has deduced consequences from it which cannot be admitted or maintained.

Nevertheless the hypothesis which he has advanced, that a sudden cooling ought to cause a molecular compression, which in itself ought to become the means of developement of specific caloric, appears to me to be admissible in a particular case, that is to say, when it acts on the *interior* parts of a mass heated throughout its whole extent, at the first moment wherein the *exterior* surface is cooled with sufficient rapidity.

I hope however, to be able to demonstrate this fact by experiments. In this instance, I took a small cylinder of iron, in the upper side of which I inserted a spiral iron wire sufficiently strong for the purpose. I covered this cylinder up to half its height in a crucible, leaving the principal points bare, then I soldered a copper wire of nearly two lines diameter,.in such a manner that the copper did not touch the iron except at the point of the solder. I afterwards fixed the ends of the iron wire and the copper wire in the reservoirs of mercury, which communicated with the band of the galvanometer, and then I heated the cylinder by the heat of spirits of wine. I expected that the exterior parts of the mass (if it is true that in these circumstances they are contracted suddenly by the effect of a depression of temperature,) would exercise an instantaneous compression of the interior parts, which would give place to a developement of specific caloric, which would be indicated by the galvanometer. In order to find the most favorable circumstances to obtain exactness in the experiment, I observed at first the successive positions of the needle during the time of heating. The needle commenced by deviating to 72°; then, the heating still going forward, not only did it return with great rapidity to the point 0° of the division, but passed it, making a contrary or negative deviation nearly to 65°. The continuance of the process of heating produced then a contrary thermo-electric relation between the iron and the copper, inasmuch as the direction of the current depends on the temperature of the point of the solder.

I profited by this remark, and now put the lamp at such a distance from the mass of iron, that the needle had power to take nearly a constant position at the point 0° of the division whenever the temperature of the mass became permanent. This I easily accomplished after one or two trials. In, fact, in this position the needle had the greatest sensibility for the least change of temperature, and it sufficed, on approaching it to the flame for an instant, to make it deviate for some seconds after with a quick movement, almost like a shake towards the negative side of the point 0°, on such a manner, that it could not retain its constant position until after several oscillations. The iron was at that point of temperature approaching to a red heat. I fixed my eye on the needle, and caused the inferior portion of the mass of iron to be plunged into cold mercury. But the needle remained immovable during the first moments, then immediately afterwards indicated to me a prompt slackening of the heat or cooling from the point of the solder. This effect was very contrary to that which I had expected, because it appeared to me, that a strong molecular compression ought necessarily to take place in this case. I do not wish to conclude from this, that a developement of specific heat, caused by a due compression by contraction of those parts suddenly cooled, has not taken place in any case; but we see however, that if the fact does exist, it ought at least to be of very little importance, for that it cannot be perceived even under the most favorable circumstances, with an instrument so sensitive as the galvanometer of which I have spoken. It is very necessary then to be able to make use of and to explain phenomena so confidently asserted as those described by M. Mousson. I have named this last experiment because it appeared to me to have given a result contrary to what we should have expected.

I have besides these made some experiments relative to those which M. Fischer has published. But as these are not independent of the sensation of the hand, I think we ought not to attribute any great importance to them; I content myself then simply with saying, that my experiments have conducted me to the belief that M. F. has partaken of the same illusion, to which artificers in metal, ordinarily abandon themselves.

XIV.—*On a New Electro-Magnetic Engine.* By Thomas Wright, Esq.

Dear Sir,

I see in your last number an account of a new Electro-magnetic Engine by Mr. Clarke, in which he has dispensed with the rotary motion, and returned to the crank. I have been lately engaged in some experiments, the results of which, incline me to believe that this will be the best form of applying the electro-magnetic power. I adopt however, a very different arrangement from that of Mr. Clarke.

The magnetism induced in soft iron by electric currents has great sustentive, but small attractive power, in comparison with that of steel magnets; the great power which the former possess not being developed, until their keepers are in connexion with their poles. In Taylor's rotatory engine, which I have reason to think is the best, the magnets are passed by their keepers with such rapidity, that I should think it impossible that their power can be developed, as it requires a certain length of time for that purpose.

The great difficulty in the construction of electro-magnetic engines with the crank motion, is the shortness of the stroke, which, for any useful purposes, should not exceed a quarter of an inch. I have, however, been able to increase the length of the stroke materially, by two methods:—the first consists in keeping one end of the armature on its respective pole, as at *a* fig. 1 plate 3, and taking the stroke between B and C; thus interrupting the magnetic circuit at one place only; this plan answers better with small electro-magnets, which generally possess greater comparative intensity, than those of a larger size. A small electro-magnet, a quarter of an inch square, and seven inches long, coiled with ten yards of copper bell-wire, attracted its keeper, placed parallel with its poles, *a quarter of an inch*, (along a polished mahogany table,) when, however, the keeper was placed as in fig. 1, it attracted the other end *an inch and a quarter;* this was the result of several experiments. The second method which is better adapted for large electro-magnets, consists in keeping one *side* of the armature on the magnet as in fig. 2; this I think will be the most available method of applying the power, and I am now making a small engine on this principle, on which I hope to have your opinion in a short time.

The magnets employed in this engine are straight and composed of hoop iron, well annealed and riveted together, and are coiled to within an inch and a half from the ends, to which are riveted pieces of soft iron two inches square, as shewn at A fig. 3, plate 3; these magnets are then united in pairs, by having the pieces of soft iron attached to them hinged together at *B*, they are thus brought as close as possible to each other along their whole length.

Fig. 4, is a plan of the engine with two pairs of magnets, the end of the lower magnet of each pair being concealed by the pillar on which it rests; the armature of the upper magnet of the pair on the right hand, forms part of the beam of the engine, the hinge which attaches it to the lower armature being the fulcrum. The upper armature of the other pair is attached to the beam by a rod at A; it will thus be seen that by making communication between the battery and each pair of magnets alternately, the armatures of the magnetized pair will be strongly attracted to each other, lifting up the armature of the other pair respectively, and working the fly wheel.

B B B, fig. 4 plate, shews the apparatus on the shaft of the fly wheel, for throwing the battery power alternately on the pairs of magnets.

I am, my dear Sir,
Yours truly,
THOMAS WRIGHT.

XV.—*An Answer to* Dr. Hare's *Letter on certain Theoretical Opinions.* By M. Faraday, Esq. F. R. S.

My dear Sir,

1. Your kind remarks have caused me very carefully to revise the general principles of the view of *static induction* which I have ventured to put forth, with the very natural fear that as it did not obtain your acceptance, it might be founded in error; for it is not a mere complimentary expression when I say I have very great respect for your judgment. As the reconsideration of them has not made me aware that they differ amongst themselves or with facts, the resulting impression on my mind is, that I must have expressed my meaning imperfectly, and I have a hope that when more clearly stated my words may gain your approbation. I feel that many of the words in the language of electrical science possess much meaning; and yet their interpretation by different philosophers often varies more or less, so that they do not carry exactly the same idea to the minds of different men: this often renders it difficult, when such words force themselves into use, to express with brevity as much as, and no more than, one really wishes to say.

2. My theory of induction (as set forth in Series xi., xii., and xiii.,) makes no assertion as to the nature of electricity, or at all questions any of the theories respecting that subject (1667). It does not even include the origination of the developed or excited state of the power or powers; but taking that as it is given by experiment and observation, it concerns itself only with the arrangement of the force in its communication to a distance in that particular yet very general phenomenon called *static induction* (1668.). It is neither the nature nor the amount of the force which it decides upon, but solely its mode of distribution.

3. Bodies whether conductors or non-conductors can be *charged.* The word *charge* is equivocal: sometimes it means that state which a glass tube acquires when rubbed by silk, or

which the prime conductor of a machine requires when the latter is in action; at other times it means the state of a Leyden jar or a similar inductive arrangement when it is said to be charged. In the first case the word means only the peculiar condition of an electrified mass of matter considered by itself, and does not apparently involve the idea of induction; in the second it means the whole of the relations of two such masses charged in opposite states, and most intimately connected by inductive action.

4. Let three insulated metallic spheres, A, B, and C, be placed in a line, and not in contact; let A be electrified positively, and then C uninsulated; besides the general action of the whole system upon all surrounding matter, there will occur a case of inductive action amongst the three balls, which may be considered apart, as the type and illustration of the whole of my theory: A will be charged positively; B will acquire the negative state at the surface towards A, and the positive state at the surface furthest from it; and C will be charged negatively.

5. The ball B will be in what is often called a polarized condition, i. e. opposite parts will exhibit the opposite electrical states, and the two sums of these opposite states will be exactly equal to each other. A and C will not be in this polarized state, for they will each be, as it is said, charged (3), the one positively, the other negatively, and they will present no polarity as far as this particular act of induction (4) is concerned.

6. That one part of A is more positive than another part does not render it polar in the sense in which that word has just been used. We are considering a particular case of induction, and have to throw out of view the states of those parts not under the inductive action. Or if any embarrassment still arise from the fact that A is not uniformly charged all over, then we have merely to surround it with balls, such as B and C, on every side, so that its state shall be alike on every part of its surface (because of the uniformity of its inductive influence in all directions) and then that difficulty will be removed. A therefore is charged, but not polarly; B assumes a polar condition; and C is charged inducteously (1483), being by the prime influence of A brought into the opposite or negative electrical state through the intervention of the intermediate and polarized ball B.

7. Simple charge therefore does not imply polarity in the body charged. Inductive charge (applying that term to the sphere B and all bodies in a similar condition (5) does (1672.). The word charge as applied to a Leyden jar, or to the *whole*

of any inductive arrangement, by including *all* the effects, comprehends of course both these states.

8. As another expression of my theory, I will put the following the case. Suppose a metallic sphere C, formed of a thin shell a foot in diameter; suppose also in the centre of it another metallic sphere A only an inch in diameter; suppose the central sphere A charged positively with electricity to the amount we will say of 100; it would act by induction through the air, lac, or other insulator between it and the large sphere C; the interior of the latter would be negative, and its exterior positive, and the sum of the positive force upon the whole of the external surface would be 100. The sphere C would in fact be polarized (5) as regards its inner and outer surfaces.

9. Let us now conceive that instead of mere air, or other insulating dielectric, within C between it and A, there is a thin metallic concentric sphere B six inches in diameter. This will make no difference in the ultimate result, for the charged ball A will render the inner and outer surfaces of this sphere B negative and positive, and it again will render the inner and outer surfaces of the large sphere C negative and positive, the sum of the positive forces on the outside of C being still 100.

10. Instead of one intervening sphere let us imagine 100 or 1000 concentric with each other, and separated by insulating matter, still the same final result will occur; the central ball will act inductrically, the influence originating with it will be carried on from sphere to sphere, and positive force equal to 100 will appear on the outside of the external sphere.

11. Again, imagine that all these spheres are subdivided into myriads of particles, each being effectively insulated from its neighbours (1679.), still the same final result will occur; the inductric body A will polarize all these, and having its influence carried on by them in their newly acquired state, will exert precisely the same amount of action on the external sphere C as before, and positive force equal to 100 will appear on its outer surface.

12. Such a state of the space between the inductric and inducteous surfaces represents what I believe to be the state of an insulating dielectric under inductive influence; the particles of which by the theory are assumed to be conductors individually, but not to one another (1669.).

13. In asserting that 100 of positive force will appear on the outside of the external sphere under all these variations, I presume I am saying no more than what every electrician will admit. Were it not so, then positive and negative elec-

tricities could exist by themselves, and without relation to each other (1169. 1177.), or they could exist in proportions not equivalent to each other. There are plenty of experiments, both old and new, which prove the truth of the principle, and I need not go further into it here.

14. Suppose a plane to pass through the centre of this spherical system, and conceive that instead of the space between the central ball A and the external sphere C being occupied by a uniform distribution of the equal metallic particles, three times as many were grouped in the one half to what occurred in the other half, the insulation of the particles being always preserved: then more of the inductric influence of A would be conveyed outwards to the inner surface of the sphere C, through that half of the space where the greater number of metallic particles existed, than through the other half: still the exterior of the outer sphere C would be uniformly charged with positive electricity, the amount of which would be 100 as before.

15. The actions of the two portions of space, as they have just been supposed to be constituted (14), is as if they possessed two different *specific inductive capacities* (1296.); but I by no means intend to say, that *specific inductive capacity* depends in all cases upon the number of conducting particles of which the dielectric is formed, or upon their vicinity. The full cause of the evident difference of inductive capacity of different bodies is a problem as yet to be solved.

16. In my papers I speak of all induction as being dependent on the action of contiguous particles, i. e. I assume that insulating bodies consist of particles which are conductors individually (1669.), but do not conduct to each other provided the intensity of action to which they are subject is beneath a given amount (1326. 1674. 1675.); and that when the inductric body acts upon conductors at a distance, it does so by polarizing (1298. 1670.) all those particles which occur in the portion of dielectric between it and them. I have used the term *contiguous* (1164. 1673.), but have I hope sufficiently expressed the meaning I attach to it: first by saying at par. 1615, "the next existing particle being considered as the contiguous one;" then in a note to par. 1665, by the words, "I mean by contiguous particles those which are next to each other, not that there is no space between them;" and further by the note to par. 1164. of the octavo edition of my Researches, which is as follows: "The word contiguous is perhaps not the best that might have been used here and elsewhere, for as particles do not touch each other it is not strictly correct. I was induced to employ it because in its common ac-

acceptation it enabled me to state the theory plainly and with facility. By contiguous particles, I mean those which are next."

17. Finally, my reasons for adopting the molecular theory of induction were the phenomena of electrolyic discharge (1164. 1343.), of induction in curved lines (1166. 1215.), of specific inductive capacity (1167. 1252.), of penetration and return action (1245.), of difference of conduction and insulation (1320.) of polar forces (1665.), &c. &c., but for these reasons and any strength or value they may possess I refer to the papers themselves.

18. I will now turn to such parts of your critical remarks as may require attention. A man who advances what he thinks to be new truths, and to develope principles which profess to be more consistent with the laws of nature than those already in the field, is liable to be charged, first with self-contradiction; then with the contradiction of facts; or he may be obscure in his expression, and so justly subject to certain queries; or he may be found in non-agreement with the opinions of others. The first and second points are very important, and every one subject to such charges must be anxious to be made aware of, and also to set himself free from or acknowledge them; the third is also a fault to be removed if possible; the fourth is a matter of but small consequence in comparison with the other three; for as every man who has the courage, not to say rashness, of forming an opinion of his own, thinks it better than any from which he differs, so it is only deeper investigation, and most generally future investigators who can decide which is in the right.

19. I am afraid I shall find it rather difficult to refer to your letter. I will, however, reckon the paragraphs in order from the top of each page, considering that the first which has its *beginning* first in the page*. In referring to my own matter I will employ the usual figures for the paragraphs of the Experimental Researches, and small Roman numerals for those of this communication.

20. At par. 3, you say, you cannot reconcile my language at 1615, with that at 1165. In the latter place I have said I believe *ordinary induction* in all cases to be an action of *contiguous* particles, and in the former assuming a very hypothetical case, that of a vacuum, I have said, nothing in my theory forbids that a charged particle in the centre of a vacuum should act on the particle next to it, though that

---

* We shall change Prof. Faraday's references for the numbers which we have attached to Dr. Hare's letter, and refer thus, par. 23, &c.

should be half an inch off. With the meaning which I have carefully attached to the word contiguous (16.), I see no contradiction here in the terms used, nor any natural impossibility or improbability in such an action. Nevertheless all *ordinary* induction is to me an action of contiguous particles, being particles at insensible distances: induction across a vacuum is not an ordinary instance, and yet I do not perceive that it cannot come under the same principles of action.

21. As an illustration of my meaning, I may refer to the case, parallel with mine, as to the extreme difference of interval between the acting particles or bodies, of the modern views of the radiation and conduction of heat. In radiation the rays leave the hot particles and pass occasionally through great distances to the next particle, fitted to receive them: in conduction, where the heat passes from the hotter particles to those which are contiguous and form part of the same mass, still the passage is considered to be by a process precisely like that of radiation; and though the effects are, as is well known, extremely different in their appearance, it cannot as yet be shewn that the principle of communication is not the same in both.

22. So on this point respecting contiguous particles and induction across half an inch of vacuum, I do not see that I am in contradiction with myself or with any natural law or fact.

23. Paragraph 4 is answered by the above remarks, and by 8, 9, and 10.

24. Paragraph 5 is answered according to my theory by 8, 9, 10, 11, 12, and 13.

25. Paragraph 6 is answered, except in the matter of opinion (18.), according to my theory by 16. The conduction of heat referred to in the paragraph itself will, as it appears to me, bear no comparison with the phenomenon of electrical induction:—the first refers to the distant influence of an agent which travels by a very slow process, the second to one where distant influence is simultaneous, so to speak, with the origin of the force at the place of action:—the first refers to an agent which is represented by the idea of one imponderable fluid, the second to an agency better represented probably by the idea of two fluids, or at least by two forces:—the first involves no polar action, nor any of its consequences, the second depends essentially on such actions;—with the first, if a certain portion be originally employed in the centre of a spherical arrangement, but a small part appears ultimately at the surface; with the second, an amount of force appears instantly at the surface (8, 9, 10, 11, 12, 13, and 14.), exactly equal to the exciting or moving force, which is still at the centre.

26. Paragraph 13 involves another charge of self-contradiction, from which, therefore, I will next endeavor to set myself free. You say I "correctly allege that it is impossible to charge a portion of matter with one electric force without the other (see par. 1177.). But if all this be true. how can there be a *positively excited particle?* (see par. 1616.). Must not every particle be excited negatively if it be excited positively? Must it not have a negative as well as a positive pole?" Now I have not said exactly what you attribute to me: my words are, "it is impossible, experimentally, to charge a portion of matter with one electric force *independently* of the other: charge always implies *induction*, for it can in no instance be effected without (1177.)." I can, however, easily perceive how my words have conveyed a very different idea to your mind, and probably to others, than that I meant to express.

27. Using the word *charge* in its simplest meaning (3. 4.), I think that a body *can* be charged with one electric force without the other, that body being considered in relation to itself only. But I think that such charge cannot exist without induction (1178.), or independently of what is called the development of an equal amount of the other electric force, not in itself, but in the neighbouring consecutive particles of the surrounding dielectric, and through them of the facing particles of the uninsulated surrounding conducting bodies, which, under the circumstances, terminate as it were the particular case of induction. I have no idea, therefore, that a particle when charged must itself of necessity be polar; the spheres A B C of 4, 5, 6, 7, fully illustrate my views (1672.).

28. Paragraph 20 includes the question, "is this consistent?" implying self-contradiction, which, therefore, I proceed to notice. The question arises out of the possibility of glass being a (slow) conductor or not of electricity, a point questioned also in the two preceding paragraphs. I believe that it is. I have charged small Leyden jars, made of thin flint glass tube, with electricity, taken out the charging wires, sealed them up hermetically, and after two or three years have opened and found no charge in them. I will refer you also to Belli's curious experiments upon the successive charges of a jar and the successive return of portions of these charges.* I will also refer to the experiments with the shell lac hemisphere, especially that described in 1237. of my Researches; also the experiment in 1246. I cannot conceive how, in these cases, the air in the vicinity of the coating could gradually relinquish to it a portion of free electricity, conveyed into it by

---

* Bibliotheca Italiana, 1837, lxxxv., p. 417.

what I call convection, since in the first experiment quoted (1237.), when the return was gradual, there was *no coating;* and in the second (1246.), when there was *a coating*, the return action was most sudden and instantaneous.

29. Paragraphs 21 and 22 perhaps only require a few words of explanation. In a charged Leyden jar I have considered the two opposite forces on the inductric and inducteous surfaces as being directed towards each other through the glass of the jar, provided the jar have no projection of its inner coating, and is uninsulated on the outside (1682.). When discharge by a wire or discharger, or any other of the many arrangements used for that purpose is effected, these supply the "some other directions" spoken of (1682. 1683.).

30. The inquiry in paragraph 23, I should answer by saying, that the process is the same as that by which the polarity of the sphere B (4. 5.) would be neutralized if the spheres A and C were made to communicate by a metallic wire; or that by which the 100 or 1000 intermediate spheres (10.) or the myriads of polarized conducting particles (11.) would be discharged, if the inner sphere A, and the outer one C, were brought into communication by an insulated wire; a circumstance which would not in the least affect the condition of the power on the exterior of the globe C.

31. The obscurity in my papers, which has led to your remarks in paragraph 25, arises, as it appears to me (after my own imperfect expression), from the uncertain or double meaning of the word discharge. You say, "if discharge involves a return to the same state in vitreous particles, the same must be true in those of the metallic wire. Wherefore then are these dissipated when the discharge is sufficiently powerful?" A jar is said to be discharged when its charged state is reduced by any means, and it is found in its first indifferent condition. The word is then used simply to express the state of the apparatus; and so I have used it in the expressions criticised in paragraph 21, already referred to. The process of discharge, or the mode by which the jar is brought into the discharged state, may be subdivided, as of various kinds; and I have spoken of conductive (1320.), electrolytic (1343.), disruptive (1359.), and convective (1562.) discharge, any one of which may cause the discharge of the jar, or the discharge of the inductive arrangements described in this letter (30), the action of the particles in any one of these cases being entirely different from the mere return action of the polarized particles of the glass jar, or the polarized globe B (5.), to their first state. My view of the relation of insulators and conductors, as bodies of one class), is given at 1320. 1675. &c.

of the Researches: but I do not think the particles of the good conductors acquire an intensity of polarization any thing like that of the particles of bad conductors; on the contrary, I conceive that the contiguous polarized particles (1670.) of good conductors discharge to each other when their polarity is at a very low degree of intensity (1326. 1338. 1675.). The question of why are the metallic particles dissipated when the charge is sufficiently powerful, is one that my theory is not called upon at present to answer, since it will be acknowledged by all, that the dissipation is not necessary to discharge. That different effects ensue upon the subjection of bodies to different degrees of the same power, is common enough in experimental philosophy; thus, one degree of heat will merely make water hot, whilst a higher degree will *dissipate* it as steam, and a lower will convert it into ice.

32. The next most important point, as it appears to me, is that contained in paragraphs 16 and 17. I have said (1330.), " what then is to separate the principle of these two extremes, perfect conduction and perfect insulation, from each other, since the moment we leave in the smallest degree perfection at either extremity we involve the element of perfection at the opposite end?" and upon this you say, might not this query be made with as much reason in the case of motion and rest?—and in any case of the intermixture of opposite qualities, may it not be said, the moment we leave the element of perfection at one end, we involve the element of perfection at the opposite?—may it not be said of light and darkness, or of opaqueness and translucency? and so forth.

33. I admit that these questions are very properly put; not that I go to the full extent of them, as for instance that of motion and rest; but I do not perceive their bearing upon the question, of whether conduction and insulation are different properties, dependent upon two different modes of action of the particles of the substances respectively possessing these actions, or whether they are only differences in *degree* of one and the same mode of action? In this question, however, lies the whole gist of the matter. To explain my views, I will put a case or two. In former times a principle or force of levity was admitted, as well as of gravity, and certain variations in the weights of bodies were supposed to be caused by different combinations of substances possessing these two principles. In later times, the levity principle has been discarded; and though we still have imponderable substances, yet the phenomena causing weight have been accounted for by one force or principle only, that of gravity; the difference in gravitation of different bodies being considered due to dif-

ferences in *degree* of this *one force* resident in them all. Now no one can for a moment suppose that it is the same thing philosophically to assume either the two forces or the one force for the explanation of the phenomena in question.

34. Again, at one time there was a distinction taken between the principle of heat and that of cold: at present that theory is done away with, and the phenomena of heat and cold are referred to the same class, (as I refer those of insulation and conduction to one class), and to the influence of different degrees of the same power. But no one can say that the two theories, namely, that including but one positive principle, and that including two, are alike.

35. Again, there is the theory of one electric fluid and also that of two. One explains by the difference in degree or quantity of one fluid, what the other attributes to a variation in the quantity and relation of two fluids. Both cannot be true. That they have nearly equal hold of our assent, is only a proof of our ignorance: and it is certain whichever is the false theory, is at present holding the minds of its supporters in bondage, and is greatly retarding the progress of science.

36. I think it therefore important, if we can, to ascertain whether insulation and conduction are cases of the same class, just as it is important to know that heat and cold are phenomena of the same kind. As it is of consequence to shew that smoke ascends and a stone descends in obedience to one property of matter, so I think it is of consequence to shew that one body insulates and another conducts only in consequence of a difference in degree of one common property which they both possess; and that in both cases the effects are consistent with my theory of induction.

37. I now come to what may be considered as queries in your letter, which I ought to answer. Paragraph 8 contains one. As I concede that particles on opposite sides of a vacuum may perhaps act upon each other, you ask, "wherefore is the received theory of the mode in which the excited surface of a Leyden jar induces in the opposite surface a contrary state, objectionable?" My reasons for thinking the excited surface does not directly induce upon the opposite surface, &c., is, first, my belief that the glass consists of particles conductive in themselves, but insulated as respects each other (17); and next, that in the arrangement given 4, 9, or 10, A does not induce directly on C, but through the intermediate masses or particles of conducting matter.

38. In the next paragraph, the question is rather implied than asked—what do I mean by polarity? I had hoped that the paragraphs 1669. 1670. 1671. 1672. 1679. 1686. 1687.

1688. 1699. 1700. 1701. 1702. 1703. 1704., in the Researches would have been sufficient to convey my meaning, and I am inclined to think you had not perhaps seen them when your letter was written. They, and the observations already made (5. 26.), with the case given (4. 5.), will, I think, be sufficient as my answer. The sense of the word *polarity* is so diverse when applied to light, to a crystal, to a magnet, to the voltaic battery, and so different in all these cases to that of the word when applied to the state of conductor under induction (5.), that I thought it safer to use the phrase "species of polarity," than any other, which being more expressive would pledge me further than I wished.

39. Paragraph 11 involves a mistake of my views. I do not consider bodies which are changed by friction, or otherwise, as polarized, or as having their particles polarized (3, 4. 27.). This paragraph and the next do not require, therefore, any further remark, especially after what I have said of polarity above (38.).

40. And now, my dear sir, I think I ought to draw my reply to an end. The paragraphs which remain unanswered refer, I think, only to differences of opinion, or else, not even to differences, but opinions regarding which I have not ventured to judge. These opinions I esteem as of the utmost importance; but that is a reason which makes me the rather desirous to decline entering upon the reconsideration, inasmuch as on many of their connected points I have formed no decided notion, but am constrained by ignorance and the contrast of facts to hold my judgment as yet in suspense. It is, indeed, to me an annoying matter to find how many subjects there are in electrical science, on which, if I were asked for an opinion, I should have to say, I cannot tell,—I do not know; but, on the other hand, it is encouraging to think that these are they which if pursued industriously, experimentally, and thoughtfully, will lead to new discoveries. Such a subject, for instance, occurs in the currents produced by dynamic induction, which you say it will be admitted do not require for their production intervening ponderable atoms. For my own part, I more that half incline to think they do require these intervening particles, that is, where any particles intervene (1729. 1733. 1738.). But on this question, as on many others, I have not yet made up my mind. Allow me, therefore, here to conclude my letter; and believe me to be with the highest esteem, My dear Sir,

Your obliged and faithful Servant,

M. FARADAY.

Royal Institution, April 18, 1840.

XVI.—*Experimental and Theoretical Researches in Electricity, Magnetism, &c.* By WILLIAM STURGEON, Lecturer on Experimental Philosophy at the Honourable East India Company's Military Academy, Addiscombe.—Superintendent of the Royal Victoria Gallery of Practical Science, Manchester, &c. &c. Fifth Memoir.

Section 1.

On Voltaic Combinations.—A new Battery of Cast Iron and amalgamated Zinc.—A comparison of the Chemical powers of various Voltaic Batteries.

234. About twelve years ago, I engaged in an extensive series of experimental enquiries, respecting some of the principal conditions necessarily connected with the action of voltaic batteries; during which, I arrived at some remarkable results, which I then conceived might probably be advantageously applicable in the formation of that peculiar class of electrical apparatus. Some of these results I published in the year 1830, in a pamphlet entitled "Experimental Researches in Galvanism, Electro-magnetism, &c."* Since the time of my pamphlet making its appearance, some of those results which I described in it have become available in the hands of other experimenters, and some others have come into general use in almost every form of voltaic battery.† There are, however, discoveries which I then made and intended for the second part of that pamphlet, and as they have not yet been met with by others, nor in any way made public, only occasionally at my lectures; and as they appear to be of some importance, whether viewed as theoretical or practical data, I venture to give them a place in this memoir.

235. In the pamphlet already alluded to, I have shewn, at page 44, that "when two similar pieces of iron are placed, one in each of two strong solutions of nitric acid in water, of different degrees of strength, having a bladder partition between them, they formed an active voltaic pair. A galvanometer with a heavy needle, four inches long, supported on a pivot, was employed in these experiments, "and the needle would frequently stand at an angle of 35° particularly if the stronger portion of the acid solution be not very feeble, and these

---

* This pamphlet is published by Sherwood, Gilbert, & Piper, Paternoster Row, London.

† In the pamphlet alluded to, I pointed out and shewed by conclusive experiments the superiority of rolled zinc over cast zinc, in voltaic arrangements.

energies seem to improve with an increase of acid in that portion of the fluid."

236. At page 45, of the same work, (paragraph 49), under the head "iron and nitrous acid," I have shewn that, "the electric relations of the two pieces of polished iron when placed in two portions of this acid, very differently diluted, or the one piece in the acid solution and the other in water, are precisely of the same character as when the *nitric* is employed; but the electrical energies displayed are more energetic, &c."

237. From the facts discovered in these experiments, I was led to construct a compound battery of ten small pairs of iron plates, in wooden cells; each cell being furnished with a bladder partition. The iron which constituted what I have called "*a pair*," was, however, merely a single piece, or long strip, which, by being bent in the middle, was easily adapted to unite two troughs: one of its ends being immersed in the *strong* acid solution, and the other end in the *feeble* acid solution of the vicinal trough; and so on throughout the series. With this battery I could decompose water, ignite metals, charcoal, &c. to a certain extent as decidedly, as by any voltaic battery whatever, ; but as its chemical and calorific powers did not meet my expectation, I proceeded no farther with it. I discovered however, that iron held a more elevated rank amongst the metals when associated with amalgamated zinc, in voltaic series, than had ever been noticed by any other experimenter. Indeed, at that time amalgamated zinc had never been employed in voltaic batteries, except in a semi-liquid form by Mr. Kemp, an ingenious chemist at Edinburgh. Sir Humphrey Davy first noticed that amalgamated zinc acted better than pure zinc when associated with copper, in a single pair; but I believe that the employment of amalgamated rolled zinc originated with my own experiments:* and I formed compound batteries of cylinders of zinc and copper which worked exceedingly well with diluted sulphuric acid.

238. I discovered also that cast iron and wrought iron performed very differently in voltaic combinations with zinc, the cast iron forming the more energetic combination with that metal, especially when well amalgamated. I discovered moreover, that amalgamated iron holds a higher rank than either cast iron or wrought iron, when voltaically associated with zinc, and that, therefore, any transference of mercury that might occur from amalgamated zinc would rather be favorable

---

* Zinc may be easily amalgamated by first immersing it in dilute sulphuric acid and then in mercury. See p. 41, of my pamphlet.

to the action, than otherwise, a circumstance so diametrically opposed to that which occurs with amalgamated copper as to give a preference to iron over that metal in voltaic associations with amalgamated zinc, especially when excitation is carried on with dilute sulphuric acid. Lately I have been induced to construct larger batteries of cast iron and amalgamated zinc, than I had ever before done, which, with their performances in the display of phenomena, I will now describe.

239. The first battery of this kind, that I constructed since my appointment at this Institution, consists of ten cylindric jars of cast iron, each 8 inches high and 3½ inches diameter, with the same number of amalgamated zinc cylinders of the same height as the iron ones, and about 2 inches diameter. Each pair of these metals is connected together by means of a curved stout copper wire, one end of which being soldered to the iron, and the other to the zinc, as shewn in fig. 7, plate 1.* The zinc of one pair is placed in the iron jar of the next, and so on throughout the series: contact being prevented by discs of millboard placed in the bottoms of the iron vessels. Before any regular or exact experiments were carried on with this battery, a few trials were made with it to give an idea of its probable powers; some of which are the following:

240. *Experiment* 1.—When six pairs were arranged in series, and charged with dilute sulphuric acid, the polar wires were properly connected with an electro-gasometer, whose terminal platinum plates are 2¼ inches high, and 1¼ broad; consequently exposing a surface of upwards of 11 square inches to the acidulated water† in the instrument. The terminals gave off 2 cubic inches of the mixed gases per minute.

241. *Experiment* 2.—By adding two other pairs to the last series, and arranging the whole in a series of 8 pairs, the terminals in the electro-gasometer liberated 7½ cubic inches of the mixed gases per minute. The above results were obtained several times over, and, in some cases, after the battery had been in action for more than three quarters of an hour.

242. *Experiment* 3.—The electro-gasometer was now laid aside, and the calorific effects of the eight pairs in series were as follow:—

Charcoal gave out a small star of brilliant light.
One inch of copper wire $\frac{1}{25}$ of an inch diameter was fused.
Four inches of do. made white hot.

---

* This figure will also appear in plate 4, which will also contain several other figures illustrative of certain parts of this memoir.

† The liquid in the electro-gasometer was 6 water, and 1 sulphuric acid, by measure.

Eighteen inches of do. made red hot in broad day light.
Eight inches of watch main spring was made red hot.
Two inches of do. made white hot for several successive minutes.*

243. *Experiment* 4.—The battery had now been in action more than an hour, and its decomposing powers were again ascertained to be equal to those exhibited at first, the terminal platinum plates still liberating the mixed gases at the rate of 7½ cubic inches per minute. The voltaic series, on this occasion, was not extended beyond eight pairs, in consequence of the other two iron jars being leaky, and could not be used until the fissures were repaired.

244. *Experiment* 5.—As the exhibition gallery of this institution was shortly to be opened to the public, I was requested by some of our directors to try if this battery could be used to illustrate the explosions made by Colonel Pasley against the wreck of the Royal George. For this purpose, the series of eight pairs was furnished with two conducting wires, 200 feet in length each, making a circuit of 400 feet long. When the farthest extremities of these wires were joined by a thin platinum wire, the latter instantly became red hot, which left no doubt of the calorific powers of the battery being capable of exploding gunpowder at that distance ; but as no preparations had been made for trying its calorific effects below the surface of a body of water, nothing farther was done at that time.

245. *Experiment* 6.—On Saturday afternoon, the 30th of May, some of our directors and a few other gentlemen, met in the gallery, and it was proposed to try the iron battery again : and as the two leaky jars (243.) were now repaired, the whole ten were arranged in one voltaic series, and charged, as before, with dilute sulphuric acid. The electro-gasometer which had been used in the former experiments, (240.) having been broken by accident, another, of much larger dimensions was now employed. Its terminal metals consist of two sheets of thin platinum, exposing about 144 square inches of surface to the acidulated water in the apparatus.† When the ten pairs, in series, were properly connected with the terminals of this instrument, 15 cubic inches of the mixed gases were liberated per minute. In the course of about eight minutes' action, the rate of decomposition sank to about 13 cubic inches per

---

* In the short description of this battery given at page 67 of this volume, I have said that 10 pairs were used to produce these calorific effects, but I find by my notes that only eight pairs were used.

† This electro-gasometer is that which was used with Mr. Grove's battery, at the Royal Institution of Great Britain. See Annals of Electricity, vol. 4, p.

minute; and after a quarter of an hour's action, it became reduced to about 11 cubic inches per minute.

246. *Experiment* 7.—Preparations were now made for imitating the blowing-up of the Royal George, but as no water could be let into the basin of the canal in the exhibition room of the Institution, in consequence of the painters being at work in it, we had recourse to a very humble, and to some persons it will appear, a most ridiculous substitute; viz., a bucket of water. Our charge of gunpowder was the same as that used in the Polytecnic Institution in London, being furnished with a stock of cartridges, from Messrs. Watkins and Hill, Charing Cross, which had been made for similar illustrations in that Institution. The bucket of water being placed on the floor of the lecture room, and one of the extremities of each long conducting wire (244) being twisted to the wires of the cartridge, the other extremity of one of them was attached to one pole of the battery, situated in the passage outside of the room door. When the word *fire* was given, and the circuit completed by Mr. Brookhouse, who stood by the battery, with the other connecting wire, for that purpose, the most singular phenomenon occurred that was ever beheld by any of the party present; and certainly one which none of us had been led to expect. The explosion of the gunpowder was accompanied by a simultaneous perpendicular ascent of both bucket and water into the air, where they seemed to rest, for a moment, at an altitude of about 5½ feet above the floor, when both fell, and the greater part of the water spilled on the floor. The singularity of this antick of the bucket produced an effect on the bystanders more easy to imagine than describe: every one involuntarily burst into an immoderate fit of laughter, which became more and more excited as each person described the ludicrousness of the event; and the consternation displayed by the two servants, who were present, in finding mops, basins, and other paraphernalia, with which they were not prepared, for taking up the water from the room floor, added no little to the burlesque character of the scene. However, the two men were very active, and in a short time the most of the water was in the bucket again.

247. *Experiment* 8.—When the effect of the last *blow-up* had sufficiently abated, one of our directors proposed that the experiment should be repeated, in order to ascertain how high the bucket and water could be raised by a second explosion. The necessary preparations being made, and chairs, forms, tables, &c., being removed from the vicinity of the bucket; the glass cupboard, in which our splendid electrical machine is placed, being guarded by chairs, forms, &c., against the

effects of splinters in case of the bucket giving way to the force of the powder, and the faces of glazed pictures turned to the wall, &c., the cartridge was sunk in the water; and on the word *fire* being given the explosion again took place. The bucket jumped up to the height of about 4½ feet from the floor on to the lecture table, carrying with it only a small portion of water, the rest being scattered about in every direction. The servants, who were prepared, on this occasion, to take up the water from the floor, set to work with great alacrity in hopes to be enabled to replace the greater part of it in the bucket in a few minutes; but observing, after working a short time, that with all their efforts they were not lessening the water on the floor, one of them looked to see how much had been collected in the bucket, and immediately called out, that "the bottom was blown out!" Nothing better than this news could possibly have happened, to give increased tension to the already excited risibility of the company.

248. The cause of the bucket and its water jumping up together by the first explosion, may probably be traced to the sudden reaction of the floor against the bottom of the bucket: which rebounded with a force nearly equal to that with which the water was blown upwards, and being in the same direction they kept pace with one another.

249. *Experiment* 9.—The battery had now been charged more than an hour, and its decomposing powers were again tried with the same electro-gasometer as last used. From a mean of several trials the liberated gases amounted to more than 10 cubic inches per minute.

250. Since the appearance of my pamphlet in 1830, experimenters have turned their attention to the improvement of voltaic batteries, and several kinds have been invented, each of which has its peculiarities, and, for some processes, most of them have a great advantage over those previously in common use. It seems rather doubtful, however, from the facts hitherto in our possession, that we shall ever discover a form of battery capable of exhibiting every class of electric phenomena to the best advantage. It is true that with the command of an extensive series of movable combinations or pairs, we can arrange them in groups, or in series in a great variety of ways, and thus be enabled to modify their forces so as to become advantageously available for the display of the electro-magnetic, electro-chemical, and the electro-calorific classes of phenomena; but for the display of the purely electrical phenomena, such as the attractions and repulsions, and the charging of coated glass, the original pile of Volta still stands pre-eminent; and amongst all the forms of *battery* which

have hitherto made their appearance, that of Cruickshank's is the only one which can be advantageously employed for purposes of this kind, and for medical treatment it seems better adapted than any other.

251. The batteries severally invented by Grove, and Smee, are unquestionably about the most powerful now generally known for continued action in the electro-magnetic, electro-chemical, and electro-calorific departments; but their high price almost precludes their general employment amongst experimenters, excepting in such cases as where the funds of an institution are at command. Professor Daniell's battery is also so constructed as to retain its powers in action for a long time together, but unless of large dimensions, its chemical, magnetic, and calorific powers, are far below those of the former two batteries. Besides the first cost of Grove's and Daniell's batteries, there is a continual current expense attending their preparation and keeping in order for experiment, to which Smee's battery is not subject: for diluted sulphuric acid being the only liquid used, and having no diaphrams between the metals, the excitation is accomplished at a cheap rate, and is not complicated by appendages which are expensive in every form they have hitherto assumed, not only in the first purchase of the battery, but by the frequent renewal of those which become destroyed, and the time necessarily required for their preparation.

252. Notwithstanding the advantages obtained by the great superiority in the action of the modern forms of battery over that exhibited by those invented respectively by Cruickshank and Wollaston, but very little seems to have been done towards ascertaining their real capabilities, as to the most advantageous display of the several classes of phenomena to which they are best adapted: hence it is, that their full powers are but little, if at all known. It is thus that an important inquiry is still left untouched, which may probably reveal facts of the highest interest to this department of physical science. Moreover, as the employment of voltaic batteries has now become very extensive, not only in investigations, but in the daily illustrations at this, and many other similar institutions, and is likely to be still more extensively employed, both in military and civil engineering, it is obvious that a cheap efficient battery, with the mode of conducting it to the best advantage, are desiderata of great moment to the practical man who may have occasion to avail himself of the advantages which such an implement affords in the daily processes of his professional avocations. But an investigation such as is best adapted to reveal these important facts, would require the command of

every kind of battery that appears likely to be adapted for general purposes, to which such an implement[*] is peculiarly applicable: and although not much skill in manipulation would be absolutely essential to such an undertaking, the requisite series of experiments would be somewhat expensive, and could not be conducted without a considerable occupation of time.

253. The batteries belonging to this institution are the following, viz.:—Cruickshank's, two troughs of 50 pairs of 3 inch plates each.—Wollaston's, two troughs of 10 pairs of 4 inch plates, with double coppers each.—Daniell's, 20 copper cylindric jars, 24 inches high and 4 inches diameter, with amalgamated strips of rolled zinc, in hempen bags or diaphrams. Grove's, 50 pairs of 4 inch platinum plates, with double amalgamated zinc in porous pots for diaphrams. Besides these, we have 30 of those cast iron jars, with their amalgamated zinc cylinders already described, (239), and 20 pairs of copper and amalgamated zinc cylinders, in porcelain jars. I have availed myself of the use of these batteries, and also of one of Smee's construction of twelve pairs, which, by the kindness of Mr. Joseph Lockett, has been placed in my hands for the purpose of comparing their powers in the display of the electro-chemical, electro-magnetic, and the electro-calorific classes of phenomena, and for ascertaining which kind of battery is most likely to become more generally useful, both as regards economy and facility of manipulation.

On the Chemical Powers of Voltaic Batteries.

254. The chemical powers of our modern batteries have, hitherto, been tested in no other way than by the decomposition of acidulated water. This circumstance may probably be owing to the great facilities which are afforded by operating on this compound, and the *supposed exactness* of the results. In point of preparation and manipulation there can be no doubt of the superior facilities for the decomposition of water, over that of most other bodies; but notwithstanding the facilities thus afforded to experimenters, the decomposition of water, as a test for the powers of voltaic batteries, has led many to the most extravagant inaccuracies: and I am not aware that any experiments are on record that have been directed to an enquiry for ascertaining the best means of arriving at a maximum of decomposition by the employment of any one of the several batteries which have hitherto been constructed. The errors of a fashionable man, whatever may be the nature of his pursuits, are almost sure to lead those astray who have either no desire or no opportunity to judge for themselves,

and there is not, perhaps, amongst the numerous errors into which Dr. Faraday has fallen, one more eminently calculated to mislead the unwary experimenter, than the pretended accuracy of the indications of an instrument, the principles of which, he either neglected to reveal, or of which he had not the slightest knowledge. The *visionary voltameter* has been a favorite instrument with experimenters, only because of their credence in the assertions of its author, and some of them have thus been led into errors which would otherwise have been avoided, amongst the records of their own discoveries.

255. If we wish to arrive at a knowledge of the powers of any voltaic battery in the process of decomposing water, there are several particulars which are necessary to be attended to: some of which will vary with almost every form of battery, whilst others are common to all batteries whatever.

256. The first essential point to be determined is, which is the most influential body in facilitating decomposition when dissolved in the water to be operated on? And as that solution which facilitates decomposition the most in one case, will also facilitate it to the greatest extent in all, whatever may be the form of battery employed, the determination of this point becomes easily accomplished. A solution of sulphuric acid is now generally placed in connexion with the platinum terminals in the decomposing apparatus: and I have not found any other which facilitates decomposition to the same extent, when the water is to the acid as about 5 to 1. The mixture ought to be made some hours prior to its being placed in the apparatus, otherwise its heat will soften the cement so as to give way to the liquid pressure, and become leaky. Whatever may be the real character of the action of bodies which facilitate the decomposition of water:—whether it be a mere mechanical separation of its particles, which makes them more assailable to the electric forces;—an improvement in its electro-conduction, and thus permits the introduction and consequent flow of a greater quantity of electric fluid; or whether it admits of an improved electro-polarization by an association with the particles of the dissolved body, remains a problem, for which philosophers have not yet found a solution.

257. The second consideration is the *distance* between the platinum terminals in the decomposing apparatus, which can hardly be too small, provided they do not absolutely touch one another. This is a fact generally known, and like the former particular, applies to all batteries whatever.

258. The third thing to be determined in the decomposition of water, is the *size* of the terminal metals in the decomposing apparatus: for the extent of decomposition will vary very

considerably with terminals of different extent of surfaces. With feeble batteries, it is necessary to concentrate the electric force to a mere point before any decomposition of water can be accomplished; hence, in such cases, short thin platinum wires are preferable to terminals of larger dimensions. The decomposition of water, however, is not the best test for ascertaining this law with precision, when the intensity of the battery is very feeble. Perhaps the following experiment will answer as well as any.

259. *Experiment.*—Employ a battery of one pair only, of small dimensions, and let the liquid operated on be a strong solution of sulphate of copper. Let the terminal metals be sheets of platinum foil of 3 or 4 square inches each; and immerse them both completely in the cuperous solution. No decomposition is perceptible, even though the connexions be continued for more than an hour: but a galvanometer placed in the circuit, indicates the existence of a current. Let, now, the negative terminal be taken out and wiped dry, and then immerse only one of its corners. In a few minutes the immersed corner will be covered with precipitated copper, indicating decomposition by the force of the concentrated current at that point: but the galvanometer needle indicates a much feebler general current than when the platinum plate was wholly immersed. By immersing the corner of the platinum terminal to different depths in the solution, the exact amount of metallic surface which just allows of decomposition, may be discovered. And it will be found, in all cases, that as the immersed surface increases, the magnetic deflections increase also. Hence it becomes obvious that the powers which such feeble currents exercise on a magnetic needle are no indications of the chemical powers of the battery; unless, indeed, we look for the one as the reverse of the other. There are several interesting facts on this nice subject; but as the principal object of this memoir is to investigate the powers of the most formidable batteries known, I shall not dwell upon them till a future opportunity presents itself.

260. The fourth point to be determined to effect the maximum of decomposition of water, by voltaic electricity, is *the proper extent of the voltaic series*, or of the proper unit of *intensity* of the battery: and as the intensities of different batteries with the same extent of series, differ very much from each other, the determination of this point must be of great interest to experimenters generally.

261. Having now pointed out four grand particulars to be attended to for obtaining a maximum decomposition of water by voltaic electricity, I will next proceed to describe the

results of a few series of experiments made with the various kinds of batteries already noticed.

Table of Experiments on the Decomposition of Water, with various Series of Professor Daniell's Voltaic Battery; with the two Electro-gasometers described in (253).

No. of pairs in series.	Quantity of Gas obtained per Minute.	
	From the Large Terminals. ( )	From the Small Terminals ( )
10	9½	9
9	9+	8¼
8	7½	7¼
7	7−	6¼
6	5¾	5¼
5	5¼	5
4	3½	3½
3	2	1¼
2	Scarcely any from either.	

262. Each of the above tabulated results, is the mean of several trials; they furnish us with a knowledge of the *unit of intensity*, of this kind of battery, which is obviously that given by a series of 5 pairs. And although the decomposition by an extensive battery, would not suffer much loss by employing a series of either 6 or 7 pairs, yet any series above 7 or below 5, would be attended with a great loss in the *quantity* of decomposition in a given time.

263. Another essential feature in these results, is in the quantities of gas liberated by the different sized terminals; the larger ones invariably producing the greater quantity.

264. In another series of experiments with Mr. Daniell's battery, and the electro-gasometer with the larger plates (245), I obtained 10½ cubic inches of the mixed gases per minute, with a series of 10 pairs; and with lower series, the rate of decomposition was nearly proportional to that in the above table; thus indicating by both sets of experiments, that the proper unit of intensity is a series of 5 pairs: for by employing the ten pairs in two series of 5 pairs each, I obtained above 12 cubic inches of the gases per minute.

265. *Table of Experiments on the Decomposition of Water, by various Series of Voltaic Pairs of Cast Iron and amalgamated Zinc, as described in paragraph* (239).

No. of Iron Jars in series.	Cubic Inches obtained per Minute.	
	Large Terminals.	Small Terminals.
10	14	10
9	11	8¾
8	10	7
7	7¼	5
6	4	2¾
5	2½	2
4	1	¼
3	Scarcely any.	

266. The first thing to be observed in this table, is the superiority of action by the large terminals, over that by the smaller ones; and in a much greater degree, than by Daniell's form of battery.

267. The next thing to be observed is the rapid increase of decomposition, by an increase of the voltaic series, even up to ten pairs; by which we understand that the whole in one series, is much more powerful than in any other way we could combine them; and it is probable, that by extending the series we should discover that the proper unit of intensity, is considerably greater than that given by ten pairs.

268. The above results were by the employment of the first ten pairs, of this kind, that were constructed; but since the time the above expermiments were made, I have obtained 22 cubic inches of the mixed gases per minute with the 10 pairs in series; I have also got 20 new iron jars cast; with 10 pairs of which I have obtained 99 cubic inches of the gases in four minutes action: and I am in hopes of arriving at a still greater rate of decomposition. In all cases with the iron batteries, the decomposition has increased rapidly up to ten pairs in series, indicating that a still higher intensity is required for the most advantageous *unit of intensity.*

269. *Table of Experiments on the Decomposition of Water, by various Series of Voltaic Pairs, on the principle of Mr. Smee's Battery. The Electro-gasometer, with large Terminals, (245) was the only one employed in this series of experiments.*

No. of Pairs in Series.	Cubic Inches of Gases liberated in One Minute with large Terminals.
2	Scarcely perceptible
3	Ditto
4	½
5	1½
6	3
7	8
8	11
9	13
10	15

270. If we look to the rapid increase of decomposition from a series of 6 pairs to the series of 10 pairs, we are soon convinced that to employ a series of 10 is more advantageous than any series below that number; and it is very probable that the proper *unit* of intensity with this battery, as with the cast iron one, is considerably above that given by a series of 10 pairs. This point, however, must be determined by future experiments, as I have not, at present, more than 10 pairs at command. But the experiments detailed in the above table,

will be a sufficient guide, for the present, for any person employing no more than 10 pairs at once, because it is obvious that the decomposition of water will be accomplished to the greatest extent, by employing them in one series: which also appears to be the case with the cast iron battery.

271. *Experiments on the Decomposition of Water, by various series of Voltaic Pairs, upon the principle of Mr. Grove's Battery. The decomposing apparatus with the larger terminals was used* 245.

No of Pairs in Series.	Cubic Inches of Gas per Minute.
2	Scarcely any.
3	6
4	9
5	11
6	14
7	16
8	18
9	21
10	24

272. From the results of this series of Experiments, it is obvious that the 10 pairs in series produce more decomposition than by any other combination of them; and it is probable that a still more extensive series would be the proper *unit of intensity* for accomplishing the maximum of decomposition by this kind of battery. Mr. Grove has, I believe, constantly employed his battery in series of 5 pairs only, which series is obviously too small, and occasions a considerable loss of decomposing power.

273. Suppose, for instance, that a battery of 30 pairs were to be used, in six series of 5 pairs each, then as 5 pairs give 11 cubic inches of gas, 5 x 6=30 pairs, would give 6×11=66 cubic inches. But 30 pairs in three series of 10 pairs each would give 3× 24=72 cubic inches of gas, which is six cubic inches more than by Mr. Grove's mode of combination.

274. In order to compare the decomposing powers of these batteries, it will be necessary to ascertain their *relative* metallic surfaces exposed to the exciting media. They stand as below for each pair:—

Daniell's	=360 square inches of metallic surface.	
Smee's ...	=192 do.	do.
Sturgeon's	=162 do.	do.
Grove's ...	=104 do.	do.

275. Thus, by assuming Mr. Grove's battery as the unit of

surface, and also the standard of decomposing power, we shall have:

Metal.	Gas.	Metal.	Gas.	
104	....		24	Grove's
162 :	25 : :	104 :	14·8	Sturgeon's
192 :	15 : :	104 :	8·1	Smee's
360 :	12 : :	104 :	3·5	Daniell's

276. Hence it appears, that if the whole of the batteries exposed precisely the same extent of metallic surface to the existing liquid, that invented by Mr. Grove would have a decided preference, and Professor Daniell's battery would hold but a very low rank in point of decomposing power. But if we view them individually according to their respective sizes in which they have been employed in these experiments, then their maximum powers that I have obtained, will stand thus:

Sturgeon's ...	25	Cubic inches of gas per minute.	
Grove's	24	do.	do.
Smee's	15	do.	do.
Daniell's......	12	do.	do.

277. The next consideration is the cost of these batteries, both as relating to the first purchase, and the current expense of keeping them in action. The price given for 12 pairs of Smee's construction, Mr. Lockett informs me, was £32. Hence, the price of 10 pairs would be £26 13s.*—The price of 10 pairs of each of the other kind of batteries is, Grove's £7.—Daniell's £6.—Sturgeon's £3 10s.

278. The excitation is carried on by about the same quantity of suphuric acid in each battery; and in Smee's, and the iron batteries, no other expense is required. But in Grove's battery 1½ lbs. of the best nitric acid for 10 pairs is used in addition: and in Daniell's, about 5 lbs. of sulphate of copper, in addition to the sulphuric acid, is used for 10 pairs. In both these latter batteries, there are also diaphrams which are continually falling into decay, which is another current expense attending these batteries. The mercury employed in the amalgamation of the zinc, would be nearly the same in all the forms of battery hitherto described; but the time occupied in fitting up is very different indeed: the iron battery requiring much less time than any of the other forms. Hence as far as the decomposition of water is concerned, the iron battery has a decided advantage, both in point of power and

---

* There can be no question, of this being a very extravagant price, as I am confident that it can be had for less than half that money, either from Watkins and Hill, Clarke, Carey, Jones, Newman, or Harris.

economy: and is so simple, that it is manageable by any person: and what is another point in its favor, it works best when quite rusty: and retains its power a long time. The hydrogen is certainly an annoyance, but I have hit upon a contrivance to remove it, which I shall describe in the sequel.

(To be continued in the September Number.)

XVII.—*Description of a New Compensating Pendulum.*

By WILLIAM GWYNN JONES, A. M.

(Extracted from Silliman's Journal.)

During the latter part of the past year, while engaged in some interesting astronomical observations which required considerable accuracy, it was indispensable to procure a time-keeper whose rate would not be affected by the variations in the temperature of the weather, to which all such machines, of ordinary construction, are liable. The expensiveness of a chronometer which could be relied upon for such a purpose, rendered a resort to some more economical instrument desirable, if it could be depended upon. The gridiron pendulum as well as the mercurial one, both of which have been designed to effect this object, were found unsatisfactory; the former from the difficulty of procuring an exact adjustment of the different rods of which it is composed, so as to produce the desired counterbalancing expansion and contraction, and the mercurial pendulum proving upon experiment too sensitive to be relied upon. Under these circumstances, I contrived a simple arrangement for a pendulum, acting upon the principle of the lever, which performed with so much accuracy that I have been induced to present it to the notice of the readers of the American Journal, believing it will not prove uninteresting to those engaged in scientific investigations requiring great uniformity of action in a time-keeper. The arrangement of the parts is so simple as to be readily understood by any skilful workman, and as it is entirely free for the adoption of any one who may prefer its construction, I have prepared a description and diagram to render it intelligible.

Fig. 5 plate 3, shews the whole pendulum, the dotted lines representing similar parts to those on the opposite side, and are introduced to render the drawing more easily understood; *a* is a similar spring to that which is attached to the pendulum of an ordinary eight-day clock, and is firmly attached to the perpendicular brass bar *b*. Through *b* there is the usual opening for the guy-wire, which gives motion to the pendulum.

This bar is firmly affixed to the transverse bar *c* either by riveting or soldering. On each end of the bar *c* there is attached a brass rod *d*, *d*, and one inch from each of these there is also affixed a steel rod *e*, *e*. These four rods pass through the bar *p*, which is intended merely to preserve them in their proper position, and is attached to the two brass rods by a pin passing through both, while the steel rods are allowed to move freely through the holes. At *f*, a transverse bar or lever is affixed to *d* by a loose pin passing through them, and the same attachment is made to the steel rod *e* at *g*. This bar is four inches long, three inches of which extend from *g* to *h*, and a similar one is attached to the dotted rod *d* and extends on the opposite side. At *h* there is another loose attachment to the rod *i*, which is of steel, and which is again affixed to the bar *k*. At *k* there is a permanent bar *m*, which passes through the weight *o*, and has the usual adjusting screw *n* at the bottom.

Rationale.—Suppose that by an increased temperature of 20°, the steel rods *e*, *e*, are expanded in length $\frac{1}{16}$ of an inch. The rods *d*, *d*, being of brass, and a small fraction larger than the steel, will expand $\frac{1}{8}$ of an inch by the same increase of temperature, it being an established theory with the best French chemists, that the relative effect of the temperature upon the two metals is as 3 to 5, or nearly double the expansion in brass as in a steel rod of similar size. The outer rods then have expanded in length $\frac{1}{16}$ of an inch more than the inner rods. It will be apparent from a slight inspection of the drawing, that as the brass rod *d* and the steel one *e* are attached by a connecting pin to the transverse bar *f h*, that by *d* expanding more than *e*, that *f h* becomes a lever, *g* being the fulcrum, and as *g h* is three times as long as *f g*, consequently if *d* be expanded $\frac{1}{16}$ more than *e*, the end *h* will be elevated $\frac{3}{16}$ of an inch, and thereby raise the weight *o* $\frac{1}{8}$ of an inch more than the expansion of *d* has depressed it. This increased elevation is intended to allow that the spring *n*, the bar *b*, the rod *i*, and the bar *m*, unitedly, will expand $\frac{1}{8}$ of an inch also, and if so, it must be apparent that the whole pendulum has preserved its equilibrium and remains precisely of the same length as if no change had taken place in any of its parts.

Fig. 6 plate 3, shews a perpendicular view of the transverse bar *f h*, arranged so as to admit the corresponding bar for the other side to work freely, and at the same time preserve the four upper rods upon a line with each other, which, as the levers intrude within each other, could not be done without the recess as shewn in the section. The same letters correspond to the same parts in Figs. 5 and 6. The dotted lines in Fig. 6, are intended to shew the relative position of the lever which is attached to the dotted line *d*, Fig. 5, in regard to the other.

Baltimore, Md., 1834.

XVIII.—*Description of an Economical Apparatus for Solidifying Carbonic Acid, recently constructed at the Wesleyan University, Middletown, Conn.* By JOHN JOHNSTON, A. M., Professor of Natural Science.

The solidification of carbonic acid has of late excited considerable interest both in Europe and in this country; but the cost of the necessary apparatus has been considerable, and many probably have on this account, merely, been prevented from making any attempt to repeat the experiment. Most of our public literary institutions, in which alone in this country such apparatus is ever used, are obliged to study economy, and they are therefore often liable to be prevented from availing themselves of the benefits of new discoveries like the present, merely on account of the expense of apparatus.

It is therefore thought a description of an economical apparatus for solidifying carbonic acid may be acceptable to the public, though we do not pretend to offer anything new on the general subject.

The generator A, fig. 7 plate 3 is made of a common mercury flask, several of which I have tested and find sufficiently strong. They may be purchased in New York for a dollar a piece, or even less. The aperture at the neck may be a little enlarged, so as to make it an inch or an inch and a quarter in diameter, and the thread of the screw re-cut. A plug of cast-steel B is made of a bar two inches in diameter, and turned with a wide and smooth shoulder, so as to fit accurately upon a collar of block-tin when screwed into its place, as represented in the figure. This collar should be soldered to the iron; which is easily accomplished by filing the iron bright and tinning it in the ordinary manner, and then melting the block-tin and pouring it on, having first screwed a cork into the aperture and formed a wall of putty or clay at a sufficient distance around it. The shoulder of the plug is readily made to fit the collar accurately by screwing it a few times into its place, and then removing with a coarse file the parts of the collar upon which it touches. In this manner an accurate joint may be made without the use of a lathe; and if the plug does not correspond precisely with the axis of the flask it is just as well.

The faucets or stop-cocks are the most difficult part to construct, and occasion full half the expense. These in our apparatus are supposed to be essentially the same as are used by others for this purpose, but it may not be amiss to insert a description, since none has to my knowledge been given. There is this peculiarity about ours, however; they are inserted in the cast-steel plugs, which indeed make a part of them. D fig. 8 plate 3 is designed to represent the plug removed from the generator; at the upper end of it a hole F one inch in diameter is drilled about an inch deep, terminating in a hollow cone into which the point G fig. 9, is accurately ground. A small hole extends quite through the plug. Around the aperture F a collar of block-tin is fitted to receive the shoulder of the part E, as seen at I, and prevents any passage around the threads of the screw. Through the axis of the part E a hole three eighths of an inch in diameter is drilled, and receives the part G which is screwed in from below, the handle H being removed. The handle H should be afterwards riveted on.

Now suppose H E G to be inserted in its place in the cast-steel plug, as represented at B I, fig. 7, the plug itself being screwed into the generator. If H′ be screwed down, the aperture from the generator is firmly closed by the conical point G; and by giving H′ a single revolution in the opposite direction, the shoulder of G is brought firmly against the bottom of E, so that no escape is permitted directly upward, but only in a lateral direction through the brass tube L, which connects the generator with the receiver C. A washer of sheet lead should be placed around the shoulder of G, in order to secure a perfect metallic contact between it and the bottom of E.

The receiver C is made of the best boiler iron, which was strongly welded around a cylinder and a bottom also welded in. It is of the same height as the generator, which is about one foot, but only about two inches in diameter internally, and has a capacity of about one pint. This form enables it to resist much greater pressure than if it was of a larger diameter; and it is rather an advantage than otherwise to have it of the same length as the generator.

A cast-steel plug with stop-cock precisely similar to the one described, screws into the receiver, as the other does into the generator. The tube L screws into the plug which is inserted in the receiver, and the other end, turned to a conical point, fits accurately into a cavity in the plug B, and is held in its place by means of the stirrup screw M. Another stirrup screw N, and block of wood O, secures the receiver C in its place.

To use this apparatus the generator and receiver are separated, and the plug B being removed, two pounds of bicarbonate of soda, made into a paste with the same weight of water, are introduced into A, and twenty ounces* of strong sulphuric acid are poured into several lead vessels, made by soldering bottoms in pieces of lead tube a little shorter than the length internally of the generator, and of such a diameter that they will just pass the aperture. These being nearly filled with acid are dropped into the generator, which, after the plug B is inserted, is allowed to lie on one side for fifteen or twenty minutes, or a less time if it is several times rolled over to mix the acid with the soda. The receiver is then attached to it as seen in the figure, by means of the stirrup screws M and N; and if kept sufficiently cool by means of ice, the liquid carbonic acid formed in A will shortly be distilled over into C, the passage between them being of course previously opened by means of the stop-cocks before described.

The stop-cocks are now to be closed and the receiver, which now contains the liquid carbonic acid, separated from the generator. A small tin cup is then to be attached to the tube L, precisely as in Dr. Mitchell's apparatus,† to receive the jet of the acid from the receiver. It is essential that the *liquid* acid should escape into this cup, which is effected by having a small tube pass from the steel plug nearly to the bottom of the receiver, or by inverting the receiver before opening the stop-cock.

The best method of testing the strength of the apparatus, is by means of a hydraulic press, but it can be done as effectually by permitting it to lie, when charged, exposed to the direct rays of the sun, and excluded from currents of air, till the temperature rises to 100° or 110° F. This should be done two or three times before running any risks by venturing to handle the apparatus while charged.

It has been our object to construct an apparatus for forming the solid acid merely, but the gauges for ascertaining the pressure, &c. might of course be added as in Dr. Mitchell's apparatus.

The above apparatus, including the expense of testing three times, cost us about nineteen dollars.

---

* The quantity of acid required to saturate or neutralize the soda would be a little more than 24 oz., or 22 oz. only if the soda is in crystals, but something less than this should always be used.

† Journal of the Franklin Institute. Vol. xxii. p. 289, and Vol. xxxv. p. 346 of Silliman's Journal.

XIX.—*Organic Chemistry. Memoir on the Essence of Crystallized Peppermint.* By M. WALTER.

(Extracted from the Comptes Rendus.)

In a note I had the honor of communicating to the academy relative to the essence of crystallized mint. I endeavored to discover if it were necessary to place this among a group of peculiar bodies, of which, ordinary camphor would be the type; or if its place ought to be in that very nearly related, and at present so numerous, group of alcohols, of which ordinary alcohol is the type. The experiments I have tried decide in favor of the first opinion: in fact, the reactions which are exercised on the essence common sulphuric acid and perchlorure of phosphorus, neat and decisive reactions, of which I shall treat in detail hereafter, are adverse to the idea of considering it as a common alcohol. The group with its derivatives is more numerous than we should at first be tempted to suppose. I have tried to represent it in the following table, in which several bodies are even yet, only hypothetical, and present gaps which I hope ere long will be filled up.

$C^{10} H^{36} + H^{1} O^{2}$ essence of mint $C^{40} H^{36}$ menthène
$C^{40} H^{32} + H^{4} O^{2}$ unknown ......$C^{40} H^{32}$ essence of teribenthine
$C^{40} H^{28} + H^{1} O^{2}$ camphor ......$C^{40} H^{28}$ camphène
$C^{40} H^{24} + H^{4} O^{2}$ unknown ......$C^{40} H^{24}$ unknown
$C^{40} H^{20} + H^{4} O^{2}$ aniseed.........$C^{40} H^{20}$ anisène
$C^{40} H^{16} + H^{4} O^{2}$ unknown ......$C^{40} H^{16}$ napthaline

The essence of mint presents itself under the form of colorless prisms, of a taste and smell which belong to the essence of powdered mint. It is rather soluble in water, very much so in alcohol, spirit of wood, ether, and essence of terebenthine; its point of fusion is at 34°c., the point of ebullition 213°c., under the pressure of $0^{m}.76$. Anhydrous phosphoric and ordinary sulphuric acids, perchlorate of phosphorus, dry chloride acting sometimes in the dark and sometimes assisted by the solar rays, exercise particular reactions. My analyses agree with those of M. Dumas, and the density of the vapor which I have found for him. The following are the data of one of these analyses: 0.3225 essence of mint, 0.9055 carbonic acid, 0.372 water, which gives in centièmes 77.68 carbon, 12.83 hydrogen, 9.19 oxygen: these results agree with the rational formula $C^{40} H^{40} C^{2}$, which gives 77.27 carbon, 12.62 hydrogen, 16.11 oxygen. The density of the vapor was found 4.62; calculation gives it 5.455. An equivalent of essence contains four volumes of vapor.

Menthène.—Causing anhydrous phosphoric acid to react on the essence of mint, we obtain a particular liquid body to which I have given the name of menthène. Distilling it once or twice over anhydrous phosphoric acid is sufficient to purify it. This liquid is clear, transparent, and of an agreeable smell, its taste is cool; it is soluble in alcohol, ether, &c.; burns with a sooty flame, boils at 163°c., under a pressure of 0.76: its specific gravity is 0.851 at 21°c. Chlore and nitric acid react in a peculiar manner: brome produces it in a very characteristic deep red colorisation: subjected to analyses it has afforded me the following result: 0.372 menthène, 1.178 carbonic acid, 0.426 water, or in centièmes 87.59 carbon, 12.71 hydrogèn. This result agrees perfectly with the formula $C^{40} H^{26}$, which would give

$$C^{40}=1530=87.18$$
$$H^{26}=\ 225=12.12$$

I took the density of the vapor twice, and found it $=4.9$; the calculation, according to the formula quoted above, gives 4.8. Hence an equivalent of menthène contains 4 volumes of vapor.

Common sulphuric acid when cold exercises no sensible action on the essence of mint: the mixture only takes a red color; but if we heat it in a sea-bath it divides itself into two strata, one colorless and fluid, the other thick and deeply colored with red; the upper stratum supplied several times with cold sulphuric acid exhibits all the characters and composition of pure menthène, the other, thick, saturated with different bases, gave me nothing from which I could infer the existence of sulpho-menthic acid.

Chloro-menthène.—In order to prepare a chlorhydrate of menthène analogous to the chlorhydrates of bicarbonated hydrogen or methylène, I caused some perchlorure of phosphorus to react on essence of mint; the reaction was very lively, it disengaged abundant vapors of chlorhydric acid. By distilling the whole in a small excess of perchlorure of phosphorus, there passed in the recipient, first, protochlorure of phosphorus, then perchlorure, and finally, an oleaginous body. The mixture supplied with water, caused to appear on the surface of this latter an oleaginous body, which, washed with water and a solution of carbonate of soda, afterwards redistilled twice in perchlorure of phosphorus, washed, put in contact with chlorure of calcium melted, and placed in vacuo, was subjected to analysis.

0.24 of matter gave 0.608 carbonic acid and 0.214 water, 0.3565 of matter decomposed by incandiscent lime, furnished $0^{3d}.0$ of chloride of silver.

These reduced to centièmes, give

Carbon	70.09
Hydrogen	9.89
Chlore	20.87

They agree with the formula of chloro-menthène, which is

$C^{10}=69.91$
$H^{34}=9.77$
$Cl^{2}=20.32$

Chloro-menthène is a pale yellow liquid, its smell is aromatic, resembling that of mace flowers, the taste fresh; it boils at 204° c., and burns with a fuliginous flame edged with green: a concentrated solution of caustic potassa has no effect upon it. Hence collecting these characteristics we may conclude that menthène and chloro-menthène are two bodies of the same type, having the same relationship between them as olefying and chloro-lefying gas, or further, as acetic and chloro-acetic acid.

The action which chlore exercises on the essence of menthène gives rise to compounds of a complicated composition. Causing dry chlore to pass through essence of mint, abundant vapors of chloror-hydric acid are liberated, and we at length obtain a yellow liquid more dense than water, which, purified and dried by the ordinary methods and subjected to analysis, gave the following result: 0.338 matter, 0.7 carbonic acid, 0.22 water.—0.365 matter gave 0.557 chlorure of silver, or in centièmes,

Carbon	49.92	This composition agrees very nearly with the following formula:	$C^{40}=1530=50.4$
Hydrogen	6.29		$H^{31}=193=6.3$
Chlore	37.6		$Cl^{5}=1106=36.5$
Oxygen			$O^{2}=200=6.8$

This product exposed to the action of chlore and solar light becomes more pale, viscous, loses also 6 equivalents of hydrogen which are replaced by 6 of chlore; in short, 0.321 matter employed gave 0.411 carbonic acid, 0.112 water; 0.283 matter furnished 0.643 chlorure of silver. These data reduced to centiemes become

Carbon	34.42	Which agrees with the formula:	$C^{40}=1530=35.4$
Hydrogen	3.87		$H^{25}=156=3.6$
Chlore	56.0		$Cl^{11}=2434=56.3$
Oxygen			$O^{2}=200=4.6$

I now pass on to the reactions produced by nitric acid and chlore on menthène.

Cold nitric acid exercises no action; but on warming it, the reaction is made with extreme violence: numerous rectilant vapors and carbonic acid are liberated. At the end the reaction is made with extreme difficulty. We obtain a yellow liquid soluble in water and alcohol, which, purified and submitted to analysis, gave the following result: 0.374 matter, 0.582 carbonic acid; 0.222 water or in centièmes, 43.05 carbon; 6.5 hydrogen, 56.45 oxygen, which nearly agrees with the formula $C^{20} H^{18} O^{9}$. This acid demands a particular study.

Causing dry chlore to pass through menthène, the chlore attacks it in a very energetic manner, and changes it into a juicy liquid of a yellow color, which, purified and dried in vacuo, gave the following result: 0.311 matter, employed 0.441 carbonic acid, 0.136 water; 0.282 matter, employed 0.653 chlorure of silver, or in centièmes:

Carbon	39. 2	Which tends to the formula:	C^{40} =1530=39.18
Hydrogen ...	4. 8		H^{26} = 162= 4.17
Chlore	5.71		Cl^{10} =2213–56.67

In this reaction, the menthène has lost 10 equivalents of hydrogen which have been replaced by 10 of chlore.

All my attempts to produce with essence of mint and the different reactives of the compounds analogous to those which afford us alcohol, spirit of wood, ether, placed under the same circumstances having failed, the action of sulphuric acid, perchlorure of phosphorus, and phosphoric acid having always given me very particular and novel results, we may conclude that essence of crystalised mint cannot be regarded as an ordinary alcohol. Hence I shall be led to place it in the same group with camphor and acetone, which it very much resembles.

XX.—*Researches on the Phenomena resulting from the introduction of certain Salts in the way of the circulation.* By M. Blake.

(Extracted from the Comptes Rendus.)

Solutions of several salts, potassa, soda, ammonia, baryte, lime, and magnesia, have been, says the author, injected into the veins or arteries, and the resulting phenomena have in most cases been studied with the assistance of the hœmodynamometer. A striking difference in the physiological action of these substances, has caused them to be divided into two classes; the one containing salts which destroy the irritability of the heart as soon as any blood vitiated by their presence circulates in the partitions of this intestine; and the other containing those substances which, without diminishing the irritability of the heart, cause death by stopping the blood in the lungs, by an influence which it seems to exercise over the capillary system of these organs. These two classes of substances, distinct as to their physiological action, are so also with regard to their chemical composition.

In fact, salts which have soda for a base seem to be the only ones which exercise no action on the irritability of the heart, whilst those of all other bases, at least all that we have tried, stop the contractions of the heart when they are introduced into the blood in any considerable quantity.

Our author goes on to say that, if the presence of the salts of soda in the blood do not stop the irritability of the heart, it determines other perturbations which cause these salts to be ranked as the most rapidly fatal poisons. If a solution of one of these substances be injected into the jugular vein of a dog, the arrival of the blood to the left heart, is hindered in about six seconds although the contractions of this entrail do not cease. At the same time the blood accumulates in the right heart and in the veinous system, producing on the partitions of the veins a pressure capable of balancing a column of mercury two inches in length. This pressure re-acting on the sides of the ventricules of the brain, as on all the other parts of the veinous system, must produce on the encéphale a degree of compression quite sufficient to account for the sudden death which happens, to animals subjected to experiment, thirty or forty seconds after the injection of the poison in the veins.

After death the heart still preserves its contractibility; but so powerful is the obstacle which the capillaries of the lungs oppose to the passage of these substances over their calibers, that it has sometimes been impossible to find the slightest trace of them in the left heart. If the quantity of the salt introduced in the vein is not sufficient to completely stop the passage of the blood over the lungs, their action on the capillaries is still demonstrated by the augmentation of the bronchic secretion, of which the quantity is increased so as to cause the animal to perish of lethargy after having filled the aërial ways.

The phenomena which follow the injection of the second class salts in the veins are very different from those we have described above. The deepest method of studying their action, consists in injecting them in the veins of an animal whose thorax has previously been opened, and upon which the artificial respiration is practiced; from seven to ten seconds after the injection, we perceive the movements of the heart cease, and the irritability of this entrail so completely destroyed, that however small the dose of poison has been, the application even of the two poles of the pile, some seconds after death, is insufficient to reproduce the contractions of the heart. Death does not follow with so much rapidity as when the pulmonary circulation is stopped, for we see the sensibility and respiration continue from two to three minutes, after the pulsations of the heart have ceased.

XXI.—"*Proceedings of the American Philosophical Society.*"—*November and December*, 1839.

The committee, consisting of Dr. Bache, Dr. Patterson, and Mr. Booth, to whom the paper of Doctor Hare, read at the last meeting of the society, was referred, entitled, "Description of an Apparatus for deflagrating carburets, phosphurets, or cyanides, in vacuo, or in an atmosphere of hydrogen, between electrodes of charcoal; with an account of the results obtained by these and other means, especially the isolation of calcium, and formation of a new fulminating compound, By R. Hare, M. D., Professor of Chemistry in the University of Pennsylvania," reported in favor of publication in the Society's Transactions. The publication was ordered accordingly.

The apparatus is of a convenient construction for the purposes designated in the title of the paper. The lower electrode or cathode is a parallelopipedon of charcoal, on which the body is placed, to be subjected to the influence of one or more batteries; and tubes with valve-cocks, communicating with an air pump, a barometer-gauge, and a reservoir of hydrogen, open into the interior of a ground plate, on which a bell-glass is fitted, air tight. In the experiments of the author, an equivalent of lime was heated with one equivalent and a half of bicyanide of mercury, in a porcelain crucible, enclosed in the alembic made for this purpose, and described in a former paper. The weight of the residue was such as would result from the union of an equivalent of calcium with an equivalent of cyanogen. This was then subjected to galvanic action on the cathode of the apparatus, the anode being brought in contact with it, and the result was the production of masses on the charcoal, having a metallic appearance.

Phosphuret of calcium, exposed in the same manner in the galvanic circuit, left pulverulent matter which effervesced in water, and, when rubbed on porcelain, appeared to contain metallic spangles, which were rapidly oxidized in the air.

In one experiment, particles of charcoal, apparently fused or resembling plumbago, dropped from the anode.

After heating lime with bicyanide of mercury, the mass was dissolved in acetic acid, in which nitrate of mercury produced a copious white precipitate, that detonated under the hammer like fulminating silver.

On a New Compound of Deutochloride of Platinum, Nitric Oxide, and Hydrochloric Acid. By HENRY D. ROGERS, Professor of Geology in the University of Pennsylvania, and MARTIN H. BOYE, Graduate of the University of Copenhagen.

This substance is procured by dissolving platinum in an excess of nitromuriatic acid, and evaporating nearly to dryness; after which it is treated with aqua regia, freshly prepared, from concentrated hydrochloric and nitric acids. A little water is afterwards added, drop by drop, just sufficient to keep the chloride of platinum dissolved, when the compound will remain in the form of a gamboge yellow powder. It is then separated by decanting and filtering, and pressed between the folds of bibulous paper, and dried *in vacuo* over sulphuric acid.

The precipitate is a yellow, minutely crystalline powder, which absorbs water with great avidity. It may be preserved, without decomposition, in dry air, or *in vacuo*. It is decomposed by water, alcohol, &c., with extrication of nitric oxide, chloride of platinum remaining in solution. A concentrated solution of chloride of platinum has, however, no action on it. Heated in an atmosphere by hydrogen, it gives off a large amount of chloride of ammonium, leaving a residuum of metallic platinum.

ANALYSIS.—The salt analysed, was prepared and kept in the manner described. Heated to the temperature of 212° F., it does not part with any of its water of combination. For estimating the amount of platinum and chlorine, the salt was fused with carbonate of potassa, &c., and the platinum, thus obtained, weighed by itself, and the chlorine precipitated from the solution by nitrate of silver.

The quantity of nitric oxide was determined by introducing a portion of the salt into a graduated tube, inverted over mercury, and decomposing it by letting up the requisite proportion of water.

The mean of a series of experiments, varied in different ways, gave

Platinum,	-	41.26 per cent.
Chlorine,	-	43.89 "
Nitric oxide		4.98 "

The above results correspond to five atoms of bichloride of platinum; five atoms of hydrochloric acid, and two atoms of nitric oxide. The water was calculated from the loss, in the analysis, to be equivalent to ten atoms.

Respecting the chemical nature of this compound, it may be regarded, either as a chloride of platinum, with a muriate of nitric oxide, represented by the following formula, $(\mathrm{Pt\ Cl^2})^5 + [(\mathrm{Cl\ H})^5 + (\mathrm{NO^2})^2] + 10\ \mathrm{Aq}$, or as a double chlorosalt, a chloroplatinate of nitrogen, with a chloroplatinate of hydrogen, represented by the formula, $[(\mathrm{Pt\ Cl^2})^2 + \mathrm{N\ Cl^2}]^2 + (\mathrm{Pt\ Cl^2} + \mathrm{H\ Cl}) + 14\ \mathrm{Aq}$.

Hall of the American Philosophical Society.

PHILADELPHIA, *December*, 1839.

To the Hon. JOEL R. POINSETT, *Secretary of War, &c. &c.*

Sir:—The undersigned have been appointed a committee of the American Philosophical Society, to call your attention to, and invite, through the medium of your department, cooperation in, the extensive system of magnetic and meteorological observations about to be made under the direction of the British Government, and in connexion with their Antarctic expedition, particularly directed towards magnetic investigations.

The science of terrestial magnetism has of late years made great advances, through the instrumentality of Humbolt Hansteen, Gauss and others, and has now reached that point where a system of combined observations at widely distant points over the surface of the globe, appears to be necessary to its further progress: desultory effort has already done all that it is competent to effect. Such a series of systematic observations has now been set on foot by the British Government, directed to a better determination of the magnetic lines, for the use of navigators, and to the accurate investigation of the magnetic elements for theoretical purposes. The objects embraced are the measurement of the magnetic intensity, dip, and variation, at different stations, by a nautical expedition, and at fixed observatories, and especially the investigations of the variations of these elements at the latter points. As subsidiary to these objects, combined meteorological observations are to be made, which cannot fail to elucidate some of the most important questions in this useful science.

The magnetic changes to be investigated are of three kinds: first, those which, depending upon a cause not yet satisfactorily explained, take place slowly but regularly, causing a general displacement of the lines of equal variation and dip; secondly, those which, depending upon the position of the sun, run through their period of change in a year or day, producing different values in the magnetic elements, according to the season or to the hour of the day; and thirdly, the small disturbances which appear to be constantly taking place, and which require for their measurement continued observation with the most accurate instruments.

The striking fact was proved in 1818, by the observations of Arago, at Paris, and of M. Kupffer, at Kasan, that the large changes which take place in the position of the horizontal needle during the day, are simultaneous at these places, so distant from each other; and a confirmation of the fact as applying to even more distant stations, resulted from the system of observations established by Humboldt and others in 1830, and extended, through the influence of the Imperial Academy of Sciences of St. Petersburg, to the most remote parts of the Russian empire, and even to Pekin. In 1834, the celebrated German philosopher Gauss, invented an instrument for measuring the variation of the needle and its changes, which introduced into these determinations an accuracy similar to that attainable in astronomical measurements. This instrument was soon furnished to different observatories, and a concerted system of observations of the minute changes of variation was introduced, which is now going on at no less than twenty-three places in Europe, the smaller and larger states having vied with each other in providing the means of executing them. The stations include Altona, Augsburg, Berlin, Bonn, Brunswick, Breda, Breslau, Cassel, Copenhagen, Cracow, Dublin, Freyberg, Göttingen, Greenwich, Halle, Kasan, Leipsic, Marburg, Milan, Munich, Naples, St. Petersburg, and Upsala.

The results already obtained and published by the German Magnetic Association, have proved satisfactorily that the minute changes in the direction of the needle, as well as the larger ones, are simultaneous at the different stations, varying however in amount, and the variation appearing to decrease in passing southward; but the influence of the position of the place, whether depending upon geographical or magnetic position, not having yet been fully determined, and being probably determinable only by observations at places even more distant from each other than those now embraced in the German series.

The invention of an instrument by Gauss, for determining the changes in horizontal magnetic intensity with the same accuracy as those of the direction of the needle, will give rise to interesting developments in regard to them; and the changes of the three elements of horizontal direction, and horizontal and vertical intensity are all included by the two instruments before referred to, and a third invented by Professor Lloyd, of Dublin. It is the object of the series now projected, to embrace these three elements; to extend the number of stations with special reference to their distribution at points of the earth interesting in their magnetic relations; to keep up a constant series of simultaneous observations for three years; and thus to effect, on an extended scale, what the German Magnetic Association has so well begun. The execution of this plan, with observations of an appropriate kind, directed also to magnetic research, by a naval expedition, was recommended to the British Government by the members of the British Association, including men of science from different countries, in 1838. It subsequently received the sanction of the Royal Society of London, was adopted by the Government, and is now in course of execution. It may be considered, therefore, to have been approved by the highest scientific authorities. In pursuance of this plan, stationary observatories are to be established, and regular observations made, for the next three years, at Toronto in Upper Canada, at St. Helena, at the Cape of Good Hope, and at a station in Van Dieman's Land. The East India Company have also undertaken to furnish the means of observation at nine points in their dominions. European Governments, who have not hitherto joined in the German system, with which this will be in connexion, have also promised similar aid. It is this extended scheme, to which our attention has been specially invited by circular from the Royal Society of London, and in which the American Philosophical Society desires that our country should co-operate. It is on a broad scale, worthy of all encouragement, and the magnitude of the scheme, the objects for which it is undertaken, and the possibility of its execution, all mark the character of the period in which we live.

The Society would propose, in furtherance of this plan, that five magnetic observatories should be established in the N. E., N. W., S. E., S. W., and at some central point of the United States, furnished with the instruments and observers necessary, fully to carry out the proper plan of combined magnetic and meteorological observations. Should the proposition to make this co-operation truly national, be acceded to, the details in relation to it can easily be arranged, and the Society will, the undersigned confidently believe, feel proud, to lend any aid in their power, in planning or executing them. It may perhaps be more satisfactory however, to state briefly, beforehand, the nature of the observations to be made, and the means required for their execution.

The magnetic observations to be undertaken at the fixed observatories are, first, of the variation (declination), absolute horizontal intensity and dip; second, of the changes of the variation of the horizontal intensity, and of the vertical intensity. The regular observations for changes in these elements, are to be made every two hours every day, (with the exception of Sundays,) for the next three years, beginning as soon as the several observatories can be arranged. To these are to be added more frequent observations on one day of each month, including the four terms during the year, fixed by the German Magnetic Association. At each station, a building of stone or wood will be required, in the construction of which no iron must be employed. The instruments adopted by the British observers are the following: A magnetometer for the declination, one for the horizontal force, one for the vertical force, a dipping needle, azimuthal transit, two reading telescopes, and two chronometers. The estimated cost of each set of these, is about fourteen hundred dollars. The cost of the observatory must vary with the place at which it is erected, and the material chosen for it, but may be estimated at from one thousand to fifteen hundred dollars. One principal and three assistants will suffice for making and reducing the observations at each station, and for carrying on a supplementary series of meteorological observations. The meteorological observations proposed, are on the pressure, temperature, and moisture of the air; on the direction and force of the wind; on the quantity of rain; on the temperature of the ground at different depths; on solar and terrestrial radiation; besides a few miscellaneous and occasional observations, not necessary to be here stated. Regular observations are to be made on these points, four times every day, and every hour on one day in each month. The instruments required at each station, are a barometer, a standard thermometer, a maximum and minimum thermometer, a hygrometer, an anemometer, several extra thermometers, an actinometer, and an apparatus for atmospheric electricity. The probable cost of each set of these would not exceed two hundred and fifty dollars. The value of the results would be much increased, by providing a self-registering anemometer and rain-gauge, instead, of the common ones, which would increase the cost of each set of instruments to five hundred and seventy dollars. The whole cost of erecting the five observatories, and providing them with excellent instruments, will probably not exceed sixteen thousand dollars; and if the observatory already existing at Philadelphia, and provided with the necessary instruments, should be adopted as one of the five, and four others be erected and furnished, the expense to the United States would not exceed twelve thousand dollars.

No estimate is made of the cost of the principal and assistants for the proposed observatories. In the organization of the new British stationary observatories, these persons are taken, in part, if not altogether, from the officers, non-commissioned officers, and privates of the artillery. The acquirements of the graduates of our Military Academy, admirably fit them for directing the observatories, which might be appropriately placed at military posts; so as to provide the officers and men necessary for making the observations, without additional expense. The direction thus given to the views of the committee; the fact that you have long been enrolled as a member of the American Philosophical Society; and the interest which you have always manifested, both as an individual and in a public capacity, in all enterprises calculated to shed a lustre upon your country, have induced the Society to direct us to address ourselves particularly to you on this subject.

With the hope that your views may coincide with those of the Society, in regard to the plan now presented for your consideration, we are, very respectfully, yours,

A. D. Bache,
R. M. Patterson,
Joseph Henry, } *Committee.*
J. K. Kane,
Jos. G. Totten,

On the Congelation of Water by the Evaporation of Ether.
By Dr. Hare.

For effecting the congelation of water by the evaporation of ether, it had been usual to expose a bulb, containing water and moistened by the ether, to a current of air. Recently Dr. Hare had succeeded far more satisfactorily by exposing a quantity of water, twenty times as large as that usually employed, covered by ether in a capsule to a blast of air, proceeding from a vessel in which it had been condensed by a pressure equal to one or two atmospheres. By these means, the freezing of the water might be seen by five hundred spectators.

Having mentioned that the pure hyponitrous ether recently obtained, caused a cold of 15° by its evaporation, it would of course be inferred, as he had found to be the fact, that this last mentioned ether might be advantageously employed.

When hydric ether is employed, it should not exceed 730 in specific gravity.

Dr. Hare further said, that it would probably be remembered, that about two years since, he had published an account of a new process for freezing water by the evaporation of ether, caused by a diminution of atmospheric pressure. In the process then described, concentrated sulphuric acid was interposed between the retort holding the water and ether, and the air pump. Since that time he had rendered the process more rapid and interesting by interposing an iron mercury bottle, with two cocks between the receiver holding the acid and the pump. The ether and water were introduced into the retort. The beak of the retort, properly bent, entered the receiver, through the tubulure to which it was luted. The beak was of such a length and curvature, as to cause its orifice to be below the surface of the acid. The neck of the receiver communicated with the cavity of the bottle, that of the bottle with the pump. The apparatus being thus arranged, the bottle was exhausted, and the cock, communicating with the pump, closed. Under these circumstances, on opening a communication between the bottle and receiver, the pressure in that vessel and in the retort was so much reduced as to cause the instantaneous ebullition of the ether, so that little, if any subsequent aid, was required from the pump. But the result which gave increased interest to the process, was the inconceivable rapidity with which the acid, under these circumstances, absorbed the ethereal vapor, which it appeared to do with greater avidity as the process advanced.

In fact, the water, in the act of congealing, flew all over the inner surface of the retort, in consequence of an explosive evolution of ethereal vapor, generated amid the aqueous particles. The congelation of the water was rendered evident to the ears as well to the eyes of his class of more than three hundred students.

Dr. Hare said, it did not appear to him that sufficient attention had been paid by artists or men of science, to the great difference which existed between the effect upon glass of heating it by radiation and by conduction. When exposed to radiant heat alone, unaccompanied by flame, or a current of hot air, glass is readily penetrated by it, and is heated, within and without, with commensurate rapidity; but in the case of its exposure to an incandescent vapour or gas, the caloric could only penetrate by the process of conduction; and, consequently, from the inferior conducting power of glass, the temperature of the outer and inner portions of the mass would be so different, as by the consequent inequality of expansion to cause the fracture, which was well known, under such circumstances, to ensue.

The combustion of anthracite coal, in an open grate, in his laboratory, having four flues of about 4.12 by 2.12 inches each, in area, just above the level of the grate (the upper stratum of the fire, having nothing between it and the ceiling,) had allowed him to perform some operations with success, which formerly he would have considered impracticable. The fire having attained to that state of incandescence to which it easily arrives when well managed, he had, on opening a hole by means of an iron rod, so as to have a perpendicular perforation extending to the bottom of the fire, repeatedly fused the beaks of retorts of any capacity, not being more than three gallons, causing them to draw out, by the force of gravity, into a tapering tube; so that, on lifting the beak from the fire, and holding the body of the retort upright, the fused portion would hang down so as to form an angle with the rest of the beak, or to have any desired obliquity. By these means, in a series of retorts, the beak of the first might be made to descend through the tubulure of a second; the beak of the second through that of a third, and so on; the beak of the last retort in the row being made, when requisite, to enter a tube passing through ice and water in an inverted bell-glass.

By means of the anthracite fire, as above described, thick rods, as well as stout tubes, might, as he had found, be softened and extended, or bent into suitable forms.

The lower end of a green glass phial, such as is used usually for Cologne water, might be made to draw out into a trumpet-shaped extremity. A Florence flask might be heated, and made flat, so as to answer better for some purposes. The drawing out of tubes into a tapering form, suitable for introducing liquids through retort tubulures, was thus easily effected; and in all cases the sealing of large tubes was better commenced in this way, although the blowpipe might be necessary to close a capillary opening which could not be closed by the fire.

Dr. Hare further communicated a method of preparing pure chlorohydric acid, from the impure muriatic acid of commerce, by the action of sulphuric acid.

It is known, said Dr. Hare, that concentrated sulphuric acid, when added to liquid chlorohydric acid, expels more or less of it as a gas, in consequence of its superior affinity for water. At the present low price of the ordinary acid of commerce, Dr. Hare had found it advantageous to procure the latter in purity, by subjecting it to the former.

A tubulated glass retort, having been half filled with chlorohydric acid, sulphuric acid was allowed to drop from a glass funnel, with a cock, into a tube descending into the acid in the retort through the tubulure, to which it was luted by strips of gum-elastic. The tube terminated in a very small bore. The beak of the retort, bent in the fire, as he had just described, descended through the tubulure into the body of a small retort containing water not refrigerated. The beak of the latter descended into a larger one, half full of water, to which ice was applied. Of course the beak of the third might, in like manner, enter the body of a fourth. After an equivalent weight of sulphuric acid had been introduced, and the evolution of gas was no longer sufficiently active, heat might be applied until nearly all the chlorohydric acid should come over.

The residual diluted sulphuric acid was, with the addition of nitrate of soda or potassa, or nitric acid, as serviceable for galvanic purposes, as if it had not been thus used.

Dr. Hare further communicated a method of preparing hydrochloric acid and chlorine in the self-regulating reservoir invented by him, and spoke of some of the applications of the gases thus prepared.

Dr. Hare was under the impression that few chemists were aware of the great advantage of the self-regulating reservoirs of gas, to which he had resorted. He was enabled, by means of them, to keep hydrogen, carbonic acid, nitric oxide, chlorine, chlorohydric acid, sulphydric acid, and arseniuretted hydrogen, so as to use any of these gases at pleasure. He had kept these reservoirs in operation for months, without taking the constituent vessels apart.

By means of the reservoir of chlorohydric acid he had been encouraged to make an effort which proved successful; to form artificial camphor by the impregnation of oil of turpentine with that gas.

Subjecting an ingot of tin to a current of chlorine from his reservoir, it was rapidly converted into the bichloride, or fuming liquor of Libavius. To his surprise the ingot was fused by the heat generated. In the last mentioned reservoir, the materials were manganese, in lumps, and concentrated chlorohydric acid, diluted sulphuric acid being also introduced; as the reaction of this last mentioned acid with the manganese was more active than that of the chlorohydric acid. In fact, sulphuric acid, diluted with its weight of water and common salt, might be used without chlorohydric acid. In the reservoir for chlorohydric acid, the materials were sal ammoniac and sulphuric acid, to which some water was added, but not so much as to prevent the chlorohydric acid from assuming the gaseous state.

He had found it preferable to keep the sulphydric acid reservoir in a flue, the gas being drawn, when wanted, through a globe of water, by means of a leaden tube, at a convenient place. It would be desirable that the reservoirs of chlorine and chlorohydric acid should be similarly situated.

XXII.—MISCELLANEOUS ARTICLES.

Wreck of the "Royal George."

The great explosion announced on the 16th instant took place on the 22nd, being the same day, and very little later than the hour stated. The effect was beautiful, and the intention of firing it having been generally known, it was witnessed by a vast number of spectators, notwithstanding that it blew a stiff breeze, which deterred many from going out to Spithead. The morning was very fine, but doubts were entertained whether the high wind might not prevent the operation, until eight o'clock, when the red flag was hoisted from all the Royal George flotilla—namely, the Success frigate hulk, and the two lumps usually moored over the wreck, one of which, No. 4, was removed about 60 yards to the westward, whilst the other remained over the spot intended for the explosion. As soon as these flags were seen, it was know that the operation would be attempted, at the afternoon slack. In the mean time, Lieut. Symonds, the executive engineer, according to a plan preconcerted between him and Colonel Pasley, sent down Mr. George Hall, the diver, who placed first a charge of 47lb., and afterwards another of 260lb. of powder, on the spot originally occupied by the main hatchway on the orlop deck, which were fired successively by Professor Daniell's voltaic battery, as soon as he came up, the second charge being placed in the hole made by the first. The object of these charges, which were fired at the morning slack, was to make a deep crater or hole for the great charge proposed to be fired at the afternoon slack. Colonel Pasley came out about one o'clock, and at half-past one the great cylinder, loaded with 25½ barrels, or nearly 2,300lb. of gunpowder, with the voltaic conducting apparatus attached to it, was raised out of a launch alongside by the derrick of No. 5, lump, and lowered into the water so as to rest a little above the surface, where it remained suspended by the bull-rope of the derrick. Hall was then sent down, and made fast a down haul rope with a single sheave block to a solid piece of timber, which he found at the bottom of the crater produced by the morning's explosion. He came up, and handed over the end of this rope, which was attached to the cylinder, to which a couple of pigs of ballast were added, to make it sink more easily; after which it was lowered from No. 5 lump, and accompanied in its descent by Hall, who had a line attached to it in his hand, and who made signals to the men above, either to lower or occasionally to raise, or to move the cylinder to the eastward or westward, as required, until he guided it into its proper place, where he lashed it to the timber before-mentioned. At about a quarter past two o'clock he came up, and reported that it was properly placed. Whilst being lowered, the voltaic conducting apparatus attached to the cylinder was veered out, and the other end of it was taken on board No. 4 lump, and placed near the voltaic battery, where Lieutenant Symonds now stationed himself. No. 5 lump was then removed to the distance of 70 or 80 yards to the southward of the spot where the cylinder had been let down. All being now ready, Colonel Pasley, who remained in that lump, ordered his bugler first to sound the "preparative," and in about a minute afterwards "the fire." At that moment Lieut. Symonds completed the circuit with the voltaic battery, and an immediate explosion took place, the shock being felt and the report heard at the same instant. In a few seconds afterwards the surface rose three or four feet in a circle of moderate size, from the center of which almost immediately afterwards, a splendid column of water at least 50 feet high, and of a conical form, was thrown up, beautifully sparkling in the sun, which was hailed by the hearty cheers of all the workmen employed, as well as the numerous spectators, and soon after several large fragments of the wreck came floating up to the surface, which proved to be the lower part of the main-mast. The form of the column of water was not so regular as on former occasions, owing to the strong wind which acted upon it. When it fell down again, clear circular waves spread outwards from the same centre, making a great commotion at the surface. and causing No. 4 lump, which was nearest to the explosion, to pitch a good deal. Soon after this the mud from the bottom came up, blackening the same circle of water, which spread outwards, discoloring the surface as it extended, and at the same time stilling the swell of the sea for a space of perhaps 200 yards in diameter. A great number of small fish came up dead, as on former occasions, which were picked up by the boatmen. More than 50 yachts or large sailing boats came out, whose decks were covered with spectators, among whom were a great number of ladies. The two admirals and the general commanding the garrison, with a great number of naval and military officers, and most of the officers of the dockyard, with their families, were present, many of whom went on board the two lumps to have a better view of the operations. The deck of the Success frigate was also crowded with spectators. The Bishop of Norwich, the Astronomer Royal, and the Russian Consul-General were present.

The cylinder used on the 22nd was of wood with iron hoops, like a mooring buoy, made by Mr. Harding, the master capstan maker, in Chatham dockyard, and protected by two coats of canvass and several coats of a waterproof composition discovered by Sergeant-Major Jones, which by numerous experiments tried to compare it with other compositions, by order of Colonel Pasley, was found to be far superior to any in former use, as it combines absolute resistance to the greatest pressure of water with a certain degree of elasticity that does not allow it to crack. This skilful and zealous officer and a party of Royal sappers and miners have been most useful in all the operations, and since one of the three excellent professional divers engaged was obliged to give up on account of ill-health, Corporal Harris has supplied his place, and made himself much more useful in that capacity than could have been expected from so short an apprenticeship, for he has only worked two weeks in this department. Besides the divers, whose services are of the most essential importance, the dockyard riggers under Mr. Clewitt, of Portsmouth, and James Chapman, of Chatham yard, have given the greatest satisfaction, as well as the naval pensioners, about 40 in number, most of whom were petty officers, and who, though all middle-aged or elderly men, have been extremely zealous and efficient. The Lively sailing lighter, commanded by Mr. Harfield, goes backwards and forwards continually, and the seamen belonging to her are kept in constant employment in taking on board or landing the timbers and guns recovered from the wreck, of the former of which an immense pile has been deposited in Portsmouth dockyard. The whole is superintended by Lieut. Symonds with great skill and indefatigable activity, who carries on the work at every slack tide, except when it blows a gale approaching to a hurricane, that is, always twice, and sometimes three times, a day, according to the moon's age, and never goes on shore but on Sundays. Col. Pasley usually goes out to Spithead once a day, except when his duty requires him at Chatham, where he passes about half his time. The massy oak timbers upon which the foremast and mizenmast were stepped, the after-part of the keel, the whole sternpost and dead wood over it, and an immense mass of the starboard bow, which rested on the keel, weighing more than 30 tons, and which was knocked down about eight months ago by the last great explosion of 1839, having been got up this season, together with the heel of the mainmast having been disengaged from its step by the great explosion which we have just described—these circumstances shew that the demolition and removal of wreck have extended nearly to the bottom of the vessel, and also prove that the mud has no tendency to accumulate, but is cleared away by the action of the tides, in proportion as the upper parts of the wreck are demolished and removed. The effect of the last explosion could not be ascertained, as the tide ran too strong for Hall, the diver, who went down again after it, to quit his ladder, but it is presumed that it will have blown out the larboard side of the wreck, and that it will have broken up timbers in all directions, probably fracturing not only the step of the mainmast, but the kelson and keel below it. The starboard side opposite was shattered and thrown down by the first great explosion of 1839. Upon the whole, though opinions were very much divided at first, and perhaps generally unfavorable, there seems every reason to hope, that before the end of the season the whole of the wreck of the Royal George will have been sufficiently removed to enable vessels to anchor over the spot without incurring the risk of losing their anchors and cables. If so, probably the Government may be induced to remove the wreck of the Edgar also, from which Lieut. Symonds recovered five iron guns during Col. Pasley's last official visit to Chatham, the surface of which, after an immersion of 129 years, proved to be converted into very soft carburet of iron or plumbago to a considerable depth, on removing the mass of oysters, &c., with which they were incrusted. These guns are remarkable for being much thicker at the breech and thinner at the muzzle than guns of the same calibre of more modern construction, from which the iron guns recovered from the Royal George differ very little.

The Atmospheric Railway Carriages.

It is upwards of a year and a half since we described the experiment made by Mr. Clegg, in the borough, on the mode of propelling carriages by means of exhausting a pipe, or tube,

with which the carriages were connected, and then admitting the atmospheric air, and, as it were, forcing the carriages or train along, and thus superseding the necessity of gas or any other power for the conveyance of a train along a rail or tram-road. These experiments were very successful; indeed, so successful, that few persons had any doubt about the ultimate success of the principle of the invention. On Thursday the experiments were exhibited on a large scale, on a railroad which connects, or is intended to connect, the line of the Birmingham, the Bristol, and the Thames Junction Lines, commencing within a short distance of Shepherd's Bush, and running in a westerly direction for about three quarters of a mile. The carriage put in motion, with the persons on it, did not weigh much less than 12 tons; but it travelled with great ease at the rate of 25 miles an hour. The exhaustion pipe, or tube, laid down, which was not the propelling agent, but the means of development, was about 9 inches indiameter. The engine by which it was exhausted—namely, a pump, worked by steam—rendered it fit for the operations required in about two minntes and a half; and from the index, that is quicksilver, employed at the *termini*, it was ascertained that the operation was performed simultaneously at both ends of the line. There was no noise, no smoke, and, what is better, no danger of explosion, or of a power which could not be governed. In short, the experiment was as successful as its most warm well-wishers could expect, and shewed that the agency of steam is not a *sine quâ non* on a railroad. Without going into the minute history of railroads, it may be as well to say, that this power may be applied to any railroad at a saving of about 70 per cent.; and that it is of sufficient force to preclude the necessity of tunnelling. It is applicable to almost any gradients. The experiments were attended by a great number of the nobility, and by many scientific men. The Archbishop of Canterbury, Lords Charleville, Rodstock, and Prudhoe, the Marquis of Douro, Lord Howick, and Lord Stuart de Rothsay, &c. were on the ground, and expressed their satisfaction at the result of the experiments.—*Times.*

Frog found in Coal.

On Wednesday morning, as two colliers, George Ross and James Gardner, were in one of the rooms of the Old Muirfield Pit, at Gargieston, they found a living frog embodied in the solid seam of coal, at least twelve fathoms beneath the surface of the earth. The nich in which it had lived was perfectly smooth inside, of the exact shape of the frog, and without a crack or crevice to give admittance to air. The hind legs of the animal are at least a third longer than usual, the fore legs shorter, the toes longer and harder, and its general colour is of a bronze hue. It leaped briskly about, the moment that it was excavated from its narrow cell. How many centuries it has been shut out from light and air, and entombed in its dreary dormitory, it is impossible to say—certain it is, that, although diminutive in form, and with great brilliancy of eye, it has a most antediluvian aspect.—*Scotch paper.*

On the separation of Lime from Magnesia, and on the Assay of Gold. By Lewis Thompson, Esq. M. R. C. S.

To separate Lime from Magnesia.

Dissolve the combined earths in dilute nitric or muriatic acid, and precipitate the filtered solution by means of an excess of carbonate of soda; dry the precipitate, and place it in a coated green glass tube, so disposed that the whole can be heated to a dull red heat; when red hot pass a current of well-washed chlorine through the tube for a few minutes: the lime will be converted into chloride of calcium, but the magnesia remains unacted upon. When the whole is cool, remove the mass from the tube and boil it for a minute or two in water, filter the liquid and wash the insoluble portion (which is magnesia) with water, and precipitate the lime from the mixed liquors by carbonate of soda. The heat should not exceed a dull red, as the mass is apt to become vitrified at the part which touches the tube, and this renders it difficult to remove the contents.

To Assay Gold.

Take six grains of the gold to be assayed and place in a small crucible, with fifteen grains of silver, and from eight to twelve grains of chloride of silver, according to the supposed impurity of the gold; lastly add fifty grains of common salt (chloride of sodium) reduced to fine powder so as to prevent decrepitation; fuse the whole together for five minutes, and allow it to become cold; then take out the metallic button and beat it into a thin plate, and subject it to the action of dilute nitric acid as in the ordinary mode of parting. By this plan the tedious process of cupellation is avoided, the baser metals being wholly removed by the chlorine of silver, and their place supplied by pure silver.

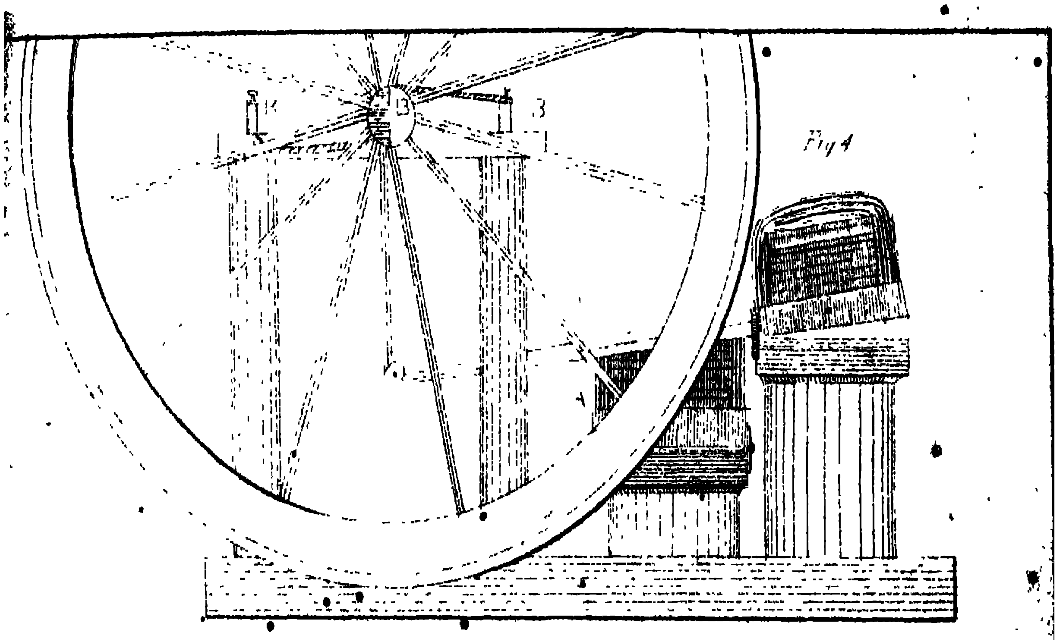


Fig 1

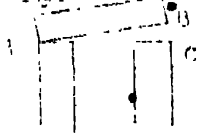


Fig 2

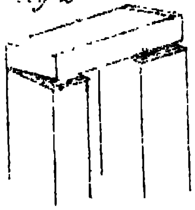


Fig 3

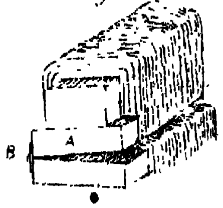


Fig 9.

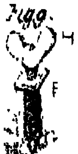


Fig 8

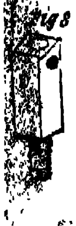


Fig 7

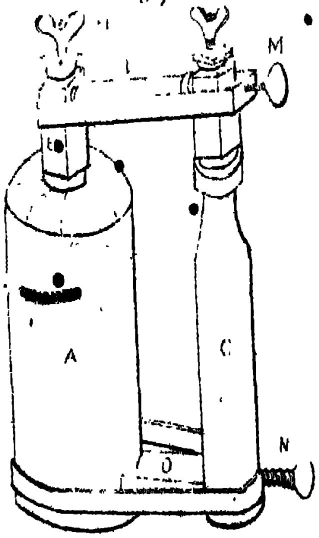


Fig. 6.

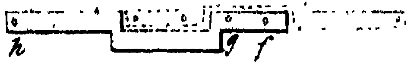
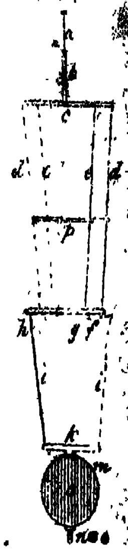


Fig 5



THE ANNALS
OF
ELECTRICITY, MAGNETISM,
AND CHEMISTRY;
AND
Guardian of Experimental Science.

NOVEMBER, 1840.

XLVII.—Experimental Researches in Electricity.—Thirteenth Series. By MICHAEL FARADAY, ESQ., D.C.L., F.R.S., Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Florence, Copenhagen, Berlin, &c. &c.

Received February 22,—Read March 15, 1838.

¶ x. *Convection, or carrying discharge (continued).* ¶ xi. *Relation of a vacuum to electrical phenomena.* §. 19. *Nature of the electrical current.*

(Continued from page 284).

1610. The latter experiments (1609.) may therefore be considered as failing to give the hoped-for proof, but I have much confidence in the former (1605. 1608.), and in the considerations (1603.) connected with them. If I have rightly viewed them, and we may be allowed to relate the currents at points and surfaces in such extremely different bodies as air and the metals, and admit that they are effects of the *same* kind, differing only in degree and in proportion to the insulating or conducting power of the dielectric used, what great additional argument we obtain in favour of that theory, which in the phenomena of insulation and conduction also, as in these, would link *the same* apparently dissimilar substances together (1336. 1561.); and how completely the general view, which refers all the phenomena to the direct action of the molecules of matter, seems to embrace the various isolated phenomena as they successively come under consideration!

1611. The connection of this convective or carrying effect, which depends upon a certain degree of insulation, with conduction; i. e. the occurrence of both in so many of the substances referred to, as, for instance, the metals, water, air, &c., would lead to many very curious theoretical generalizations, which I must not indulge in here. One point, however, I shall venture to refer to. Conduction appears to be essentially an action of contiguous particles, and the considerations just stated, together with others formerly expressed (1326. 1336, &c.), lead to the conclusion, that all bodies conduct, and by the same process, air as well as metals; the only difference being in the necessary degree of force or tension between the particles which must exist before the act of conduction or transfer from one particle to another can take place.

1612. The question then arises, what is this limiting condition which separates, as it were, conduction and insulation from each other? Does it consist in a difference between the two contiguous particles, or the contiguous poles of these particles in the nature and amount of positive and negative force, no communication or discharge occurring unless that difference rises up to a certain degree, variable for different bodies, but always the same for the same body? Or is it true that, however small the difference between two such particles, if *time* be allowed, equalization of force will take place, even with the particles of such bodies as air, sulphur or lac? In the first case, insulating power in any particular body would be proportionate to the degree of the assumed necessary difference of force; in the second, to the *time* required to equalize equal degrees of difference in different bodies. With regard to airs, one is almost led to expect a permanent difference of force, but in all other bodies, time seems to be quite sufficient to ensure, ultimately, complete conduction. The difference in the modes by which insulation may be sustained, or conduction effected, is not a mere fanciful point, but one of great importance, as being essentially connected with the molecular theory of induction, and the manner in which the particles of bodies assume and retain their polarized state.

¶ xi. *Relation of a vacuum to electrical phenomena.*

1613. It would seem strange if a theory which refers all the phenomena of insulation and conduction, i. e. all electrical phenomena, to the action of contiguous particles, were to omit to notice the assumed possible case of a *vacuum*. Admitting that a vacuum can be produced, it would be a very curious matter indeed to know what its relation to electrical phenomena would be; and as shell-lac and metal are directly opposed to each other, whether a vacuum would be opposed to them both, and allow neither of induction or conduction across it. Mr. Morgan* has said that a vacuum does not conduct. Sir H. Davy concluded from his investigations, that as perfect a vacuum as could be made† did conduct, but does not consider the prepared spaces which he used as absolute vacua. In such experiments I think I have observed the luminous discharge to be principally on the inner surface of the glass; and it does not appear at all unlikely, that, if the vacuum refused to conduct, still the surface of glass next it might carry on that action.

1614. At one time, when I thought inductive force was exerted in right lines, I hoped to illustrate this important question by making experiments on induction with metallic mirrors (used only as conducting vessels) exposed towards a very clear sky at night time, and of such concavity that nothing but the firmanent could be visible from the lowest part of the concave *n*, *fig.* 29, plate iii. Such mirrors, when electrified, as by connection with a Leyden jar, and examined by a carrier ball, readily gave electricity at the lowest part of their concavity if in a room; but I was in hopes of finding that, circumstanced as before stated, they would give little or none at the same spot, if the atmosphere above really terminated in a vacuum. I was disappointed in the conclusion, for I obtained as much electricity there as before; but on discovering the action of induction in curved lines (1231.), found a full and satisfactory explanation of the result.

1615. My theory, as far as I have ventured it, does not pretend to decide upon the consequences of a vacuum. It is not at present limited sufficiently, or rendered precise enough, either by experiments relating to spaces void of matter, or those of other kinds, to indicate what would happen in the vacuum case. I have only as yet endeavoured to establish, what all the facts seem to prove, that when electrical phenomena, as those of induction, conduction, insulation and discharge occur, they depend on, and are produced by the action of *contiguous* particles of matter, the next existing particle being considered as the contiguous one; and I have further assumed, that these particles are polarized; that each exhibits the two forces, or the force in two directions (1295. 1298.); and that they act at a distance only by acting on the *contiguous* and intermediate particles.

1616. But assuming that a perfect vacuum were to intervene in the course of the lines of inductive action (1304.), it does not follow from this theory, that the particles on opposite sides of such a vacuum could not act on each other. Suppose it possible for a positively electrified particle to be in the centre of a vacuum an inch in diameter, nothing in my present views forbids that the particle should act at the distance of half an inch on all the particles forming the inner superfices of the bounding sphere, and with a force consistent with the well-known law of the squares of the distance. But suppose the sphere of an inch were full of insulating matter, the electrified particle would not then, according to my notion, act directly on the distant particles, but on those in immediate association with it, employing *all* its power in polarizing them; producing in them negative force equal in amount to its own positive force and directed towards the latter, and positive force of equal amount directed outwards and acting in the same manner upon the layer of particles next in succession. So that ultimately, those particles in the surface of a sphere of half an inch radius, which were acted on *directly* when that sphere was a vacuum, will now be acted on *indirectly* as respects the central particle or source of action, i. e. they will be polarized in the same way, and with the same amount of force.

---

* Philosophical Transactions, 1785, p. 272. † Ibid. 1822, p. 64.

§ 19. *Nature of the electric current.*

1617. The word *current* is so expressive in common language, that when applied in the consideration of electrical phenomena we can hardly divest it sufficiently of its meaning, or prevent our minds from being prejudiced by it (283. 511.). I shall use it in its common electrical sense, namely, to express generally a certain condition and relation of electrical forces supposed to be in progression.

1618. A current is produced both by excitement and discharge; and whatsoever the variation of the two general causes may be, the effect remains the same. Thus excitement may occur in many ways, as by friction, chemical action, influence of heat, change of condition, induction, &c.; and discharge has the forms of conduction, electrolyzation, disruptive discharge, and convection; yet the current connected with these actions, when it occurs, appears in all cases to be the same. This constancy in the character of the current, notwithstanding the particular and great variations which may be made in the mode of its occurrence, is exceedingly striking and important; and its investigation and development promise to supply the most open and advantageous road to a true and intimate understanding of the nature of electrical forces.

1619. As yet the phenomena of the current have presented nothing in opposition to the view I have taken of the nature of induction as an action of contiguous particles. I have endeavoured to divest myself of prejudices and to look for contradictions, but I have not perceived any in conductive, electrolytic, convective, or disruptive discharge.

1620. Looking at the current as a *cause*, it exerts very extraordinary and diverse powers, not only in its course and on the bodies in which it exists, but collaterally, as in inductive or magnetic phenomena.

1621. *Electrolytic action.*—One of its direct actions is the exertion of pure chemical force, this being a result which has now been examined to a considerable extent. The effect is found to be *constant* and *definite* for the quantity of electric force discharged (783, &c.); and beyond that, the *intensity* required is in relation to the intensity of the affinity or forces to be overcome (904. 906. 911.). The current and its consequences are here proportionate; the one may be employed to represent the other; no part of the effect of either is lost or gained; so that the case is a strict one, and yet it is the very case which most strikingly illustrates the doctrine that induction is an action of contiguous particles (1164. 1343.).

1622. The process of electrolytic discharge appears to me to be in close analogy, and perhaps in its nature identical with another process of discharge, which at first seems very different from it, I mean *convection*. In the latter case the particles may travel for yards across a chamber; they may produce strong winds in the air, so as to move machinery; and in fluids, as oil of turpentine, may even shake the hand, and carry heavy metallic bodies about;* and yet I do not see that the force, either in kind or action, is at all different to that by which a particle of hydrogen leaves one particle of oxygen to go to another, or by which a particle of oxygen travels in the contrary direction.

1623. Travelling particles of the air can effect chemical changes just as well as the contact of a fixed platina electrode, or that of a combining electrode, or ions of a decomposing electrolyte (453.471); and in the experiment formerly described, where eight places of decomposition were rendered active by one current (469.), and where charged particles of air in motion were the only electrical means of connecting these parts of the current, it seems to me that the action of the particles of the electrolyte and of the air were essentially the same. A particle of air was rendered positive; it travelled in a certain determinate direction, and coming to an electrolyte, communicated its powers; an equal amount of positive force was accordingly acquired by another particle (the hydrogen), and the latter, so charged, travelled as the former did, and in the same direction, until it came to another particle, and transferred its power and motion, making that other particle active. Now, though the particle of air travelled over a visible and occasionally a large space, whilst the particle of the electrolyte moved over an exceedingly small one; though the air particle might be oxygen, nitrogen, or hydrogen, receiving its charge from force of high intensity, whilst the electrolytic particle of hydrogen had a natural aptness to receive the positive condition with extreme facility; though the air particle might be charged with very little electricity at a very high intensity by one process, whilst the hydrogen particle might be charged with much electricity at a very low intensity by another process; these are not differences of kind, as relates to the final discharging action of these particles, but only of degree; not essential differences which make things unlike, but such differences as give to things, similiar the electrified particle would not then, according to my notion, act directly on the distant particles, but on those in immediate association with it, employing *all* its power in polarizing them; producing in them negative force equal in amount to its own positive force and directed towards the latter, and positive force of equal amount directed outwards and acting in the same manner upon the layer of particles next in succession. So that ultimately, those particles in the surface of a sphere of half an inch radius, which were acted on *directly* when that sphere was a vacuum, will now be acted on *indirectly* as respects the central particle or source of action, i. e. they will be polarized in the same way, and with the same amount of force.

---

* If a metallic vessel three or four inches deep, containing oil of turpentine, be insulated and electrified, and a rod with a ball (an inch or more in diameter) at the end, have the ball immersed in the fluid whilst the end is held in the hand, the mechanical force generated when the ball is moved to and from the sides of the vessel will soon be evident to the experimenter.

§ 19. *Nature of the electric current.*

1617. The word *current* is so expressive in common language, that when applied in the consideration of electrical phenomena we can hardly divest it sufficiently of its meaning, or prevent our minds from being prejudiced by it (283. 511.). I shall use it in its common electrical sense, namely, to express generally a certain condition and relation of electrical forces supposed to be in progression.

1618. A current is produced both by excitement and discharge; and whatsoever the variation of the two general causes may be, the effect remains the same. Thus excitement may occur in many ways, as by friction, chemical action, influence of heat, change of condition, induction, &c.; and discharge has the forms of conduction, electrolyzation, disruptive discharge, and convection; yet the current connected with these actions, when it occurs, appears in all cases to be the same. This constancy in the character of the current, notwithstanding the particular and great variations which may be made in the mode of its occurrence, is exceedingly striking and important; and its investigation and development promise to supply the most open and advantageous road to a true and intimate understanding of the nature of electrical forces.

1619. As yet the phenomena of the current have presented nothing in opposition to the view I have taken of the nature of induction as an action of contiguous particles. I have endeavoured to divest myself of prejudices and to look for contradictions, but I have not perceived any in conductive, electrolytic, convective, or disruptive discharge.

1620. Looking at the current as a *cause*, it exerts very extraordinary and diverse powers, not only in its course and on the bodies in which it exists, but collaterally, as in inductive or magnetic phenomena.

1621. *Electrolytic action.*—One of its direct actions is the exertion of pure chemical force, this being a result which has now been examined to a considerable extent. The effect is found to be *constant* and *definite* for the quantity of electric force discharged (783, &c.); and beyond that, the *intensity* required is in relation to the intensity of the affinity or forces to be overcome (904. 906. 911.). The current and its consequences are here proportionate; the one may be employed to represent the other; no part of the effect of either is lost or gained; so that the case is a strict one, and yet it is the very case which most strikingly illustrates the doctrine that induction is an action of contiguous particles (1164. 1343.).

1622. The process of electrolytic discharge appears to me to be in close analogy, and perhaps in its nature identical with another process of discharge, which at first seems very different from it, I mean *convection*. In the latter case the particles may travel for yards across a chamber; they may produce strong winds in the air, so as to move machinery; and in fluids, as oil of turpentine, may even shake the hand, and carry heavy metallic bodies about;* and yet I do not see that the force, either in kind or action, is at all different to that by which a particle of hydrogen leaves one particle of oxygen to go to another, or by which a particle of oxygen travels in the contrary direction.

1623. Travelling particles of the air can effect chemical changes just as well as the contact of a fixed platina electrode, or that of a combining electrode, or ions of a decomposing electrolyte (453.471); and in the experiment formerly described, where eight places of decomposition were rendered active by one current (469.), and where charged particles of air in motion were the only electrical means of connecting these parts of the current, it seems to me that the action of the particles of the electrolyte and of the air were essentially the same. A particle of air was rendered positive; it travelled in a certain determinate direction, and coming to an electrolyte, communicated its powers; an equal amount of positive force was accordingly acquired by another particle (the hydrogen), and the latter, so charged, travelled as the former did, and in the same direction, until it came to another particle, and transferred its power and motion, making that other particle active. Now, though the particle of air travelled over a visible and occasionally a large space, whilst the particle of the electrolyte moved over an exceedingly small one; though the air particle might be oxygen, nitrogen, or hydrogen, receiving its charge from force of high intensity, whilst the electrolytic particle of hydrogen had a natural aptness to receive the positive condition with extreme facility; though the air particle might be charged with very little electricity at a very high intensity by one process, whilst the hydrogen particle might be charged with much electricity at a very low intensity by another process; these are not differences of kind, as relates to the final discharging action of these particles, but only of degree; not essential differences which make things unlike, but such differences as give to things, similiar in their nature, that great variety which fits them for their office in the system of the universe.

---

* If a metallic vessel three or four inches deep, containing oil of turpentine, be insulated and electrified, and a rod with a ball (an inch or more in diameter) at the end, have the ball immersed in the fluid whilst the end is held in the hand, the mechanical force generated when the ball is moved to and from the sides of the vessel will soon be evident to the experimenter.

1624. So when a particle of air, or of dust in it, electrified at a negative point, moves on through the influence of the inductive forces (1572.) to the next positive surface, and after discharge passes away, it seems to me to represent exactly that particle of oxygen which, having been rendered negative in the electrolyte, is urged by the same disposition of inductive forces, and going to the positive platina electrode, is there discharged, and then passes away, as the air or dust did before it.

1625. *Heat* is another direct effect of the *current* upon substances in which it occurs, and it becomes a very important question, as to the relation of the electric and heating forces, whether the latter is always definite in amount.[*] There are many cases, even amongst bodies which conduct without change, which stand out at present from the assumption that it is;[†] but there are also many which indicate that, when proper limitations are applied, the heat produced is definite. Harris has shown this for a given length of current in a metallic wire, using common electricity;[‡] and De la Rive has proved the same point for voltaic electricity by his beautiful application of Breguet's thermometer.[§]

1626. When the production of heat is observed in electrolytes under decomposition, the results are still more complicated. But important steps have been taken in the investigation of this branch of the subject by De la Rive[||] and others; and it is more than probable that, when the right limitations are applied, constant and definite results will here also be obtained.

1627. It is a most important part of the character of the current, and essentially connected with its very nature, that it is always the same. The two forces are everywhere in it. There is never one current of force or one fluid only. Any one part of the current may, as respects the presence of the two forces there, be considered as precisely the same with any other part; and the numerous experiments which imply their possible separation, as well as the theoretical expressions which, being used daily, assume it, are, I think, in contradiction with facts (511, &c.). It appears to me to be as impossible to assume a current of positive or a current of negative force alone, or of the two at once with any predominance of one over the other, as it is to give an absolute charge to matter (1169. 1177.).

1628. The conviction of this truth, if, as I think, it be a truth, or on the other hand the disproof of it, is of the greatest consequence. If, as a first principle, we can establish that the centres of the two forces, or elements of force, never can be separated to any sensible distance, or at all events not further than the space between two contiguous particles (1615.), or if we can establish the contrary conclusion, how much more clear is our view of what lies before us, and how much less embarrassed the ground over which we have to pass in attaining to it, than if we remain halting between two opinions! And if, with that feeling, we rigidly test every experiment which bears upon the point, as far as our prejudices will let us (1161.), instead of permitting them with a theoretical expression to pass too easily away, are we not much more likely to attain the real truth, and from that proceed with safety to what is at present unknown?

---

[*] See De la Rive's Researches, Bib. Universelle, 1829, xl. p. 40.

[†] Amongst others, Davy, Philosophical Transactions, 1821, p. 438. Pelletier's important results, Annales de Chimie, 1834, lvi. p. 371. and Becquerel's non-heating current, Bib. Universelle, 1835, lx. 218.

[‡] Philosophical Transactions, 1824, pp. 225. 228.

[§] Annales de Chimie, 1836, lxii. 177.

[||] Bib. Universelle, 1829, xl. 49; and Ritchie, Phil. Trans. 1832, p. 296.

1629. I say these things not, I hope, to advance a particular view, but to draw the strict attention of those who are able to investigate and judge of the matter, to what must be a turning point in the theory of electricity; to a separation of two roads, one only of which can be right: and I hope I may be allowed to go a little further into the facts which have driven me to the view I have just given.

1630. When a wire in the voltaic circuit is heated, the temperature frequently rises first, or most at one end. If this effect were due to any relation of positive or negative as respects the current, it would be exceedingly important. I therefore examined several such cases; but when, keeping the contacts of the wire and its position to neighbouring things unchanged, I altered the direction of the current, I found that the effect remained unaltered, showing that it depended, not upon the direction of the current, but on other circumstances. So there is here no evidence of a difference between one part of the circuit and another.

1631. The same point, i. e. uniformity in every part, may be illustrated by what may be considered as the inexhaustible nature of the current when producing particular effects; for these effects depend upon transfer only, and do not consume the power. Thus a current which will heat one inch of platina wire will heat a hundred inches (853. note). If a current be sustained in a constant state, it will decompose the fluid in one voltameter only, or in twenty others if they be placed in the circuit, in each to an amount equal to that in the single one.

1632. Again, in cases of disruptive discharge, as in the spark, there is frequently a dark part (1422.), which, by Professor Johnson, has been called the neutral point*; and this has given rise to the use of expressions implying that there are two electricities existing separately, which, passing to that spot, there combine and neutralize each other†. But if such expressions are understood as correctly indicating that positive electricity alone is moving between the positive ball and that spot, and negative electricity only between the negative ball and that spot, then what strange conditions these parts must be in; conditions, which to my mind are every way unlike that which really occurs! In such a case, one part of a current would consist of positive electricity only, and that moving in one direction; another part would consist of negative electricity only, and that moving in the other direction; and a third part would consist of an accumulation of the two electricities, not moving in either direction, but mixing up together, and being in a relation to each other utterly unlike any relation which could be supposed to exist in the two former portions of the discharge. This does not seem to me to be natural. In a current, whatever form the discharge may take, or whatever part of the circuit or current is referred to, as much positive force as is there exerted in one direction, so much negative force is there exerted in the other. If it were not so we should have bodies electrified not merely positive and negative, but on occasions in a most extraordinary manner, one being charged with five, ten, or twenty times as much of both positive and negative electricity in equal quantities as another. At present, however, there is no known fact indicating such states.

---

* Silliman's Journal, 1834, xxv. p. 57.

† Thomson on Heat and Electricity, p. 471.

1633. Even in cases of convection, or carrying discharge, the statement that the current is everywhere the same must in effect be true (1627.): for how, otherwise, could the results formerly described occur? When currents of air constituted the mode of discharge between the portions of paper moistened with iodide of potassium or sulphate of soda (465. 469.), decomposition occurred; and I have since ascertained that, whether a current of positive air issued from a spot, or one of negative air passed towards it, the effect of the evolution of iodine or of acid was the same, whilst the reversed currents produced alkali. So also in the magnetic experiments (307.) whether the discharge was effected by the introduction of a wire, or the occurrence of a spark, or the passage of convective currents either one way or the other, (depending on the electrified state of the particles) the result was the same, being in all cases dependent upon the perfect current.

1634. Hence, the section of a current compared with other sections of the same current must be a constant quantity, if the actions exerted be of the same kind; or if of different kinds, then the forms under which the effects are produced are equivalent to each other, and experimentally convertible at pleasure. It is in sections, therefore, we must look for identity of electrical force, even to the sections of sparks and carrying actions, as well as those of wires and electrolytes.

1635. In illustration of the utility and importance of establishing that which may be the true principle, I will refer to a few cases. The doctrine of unipolarity as formerly stated, and I think generally understood,* is evidently inconsistent with my view of a current (1627.); and the latter singular phenomena of poles and flames described by Erman and others[*] partake of the same inconsistency of character. If a unipolar body could exist, i. e. one that could conduct the one electricity and not the other, what very new characters we should have a right to expect in the currents of single electricities passing through them, and how greatly ought they to differ, not only from the common current which is supposed to have both electricities travelling in opposite directions in equal amount at the same time, but also from each other! The facts, which are excellent, have, however, gradually been more correctly explained by Becquerel,[†] Andrews,[‡] and others; and I understand that Professor Ohms[§] has perfected the work, in his close examination of all the phenomena; and after showing that similar phenomena can take place with good conductors, proves that with soap, &c. many of the effects are the mere consequences of the bodies evolved by electrolytic action.

---

* Erman, Annales de Chimie, 1807, lxi. p. 115. Davy's Elements, p. 168. Biot, Epcy. Brit. Supp. iv. p. 444. Becquerel, Traite, i. p. 167. De la Rive, Bib. Univ. 1837, vii. 392.

1636. I conclude, therefore, that the *facts* upon which the doctrine of unipolarity was founded are not adverse to that unity and indivisibility of character which I have stated the current to possess, any more than the phenomena of the pile itself, which might well bear comparison with those of unipolar bodies, are opposed to it. Probably the effects which have been called effects of unipolarity, and the peculiar differences of the positive and negative surface when discharging into air, gases, or other dielectrics (1480. 1525.) which have been already referred to, may have considerable relation to each other.[||]

1637. M. de la Rive has recently described a peculiar and remarkable effect of heat on a current when passing between electrodes and a fluid.[¶] It is, that if platina electrodes dip into acidulated water no change is produced in the passing current by making the positive electrode hotter or colder; whereas making the negative electrode hotter increased the deflexion of a galvanometer affected by the current, from 12° to 30° and even 45°, whilst making it colder diminished the current in the same high proportions.

1638. That one electrode should have this striking relation to heat whilst the other remained absolutely without, seem to me as incompatible with what I conceived to be the character of a current as unipolarity (1627. 1635.), and it was therefore with some anxiety that I repeated the experiment. The electrodes which I used were of platina; the electrolyte, water containing about one sixth of sulphuric acid by weight: the voltaic battery consisted of two pairs of amalgamated zinc and platina plates in dilute sulphuric acid, and the galvanometer in the circuit was one with two needles, and gave when the arrangement was complete a deflexion of 10° or 12°.

---

[*] Erman, Annales de Chimie, 1824, xxv. 278. Becquerel, Ibid. xxxvi. p. 329.

[†] Becquerel, Annales de Chimie, 1831, xlvi. p. 283.

[‡] Andrews, Philosophical Magazine, 1836, ix. 182.

[§] Schweigger's Jahrbuch der Chemie, &c. 1830. Heft 8. Not understanding German, it is with extreme regret I confess I have not access, and cannot do justice, to the many most valuable papers in experimental electricity published in that language. I take this opportunity also of stating another circumstance which occasions me great trouble, and, as I find by experience, may make me seemingly regardless of the labours of others:—it is a gradual loss of memory for some years past; and now, often when I read a memoir, I remember that I have seen it before, and would have rejoiced if at the right time I could have recollected and referred to it in the progress of my own papers.—M. F.

[||] See also Hare in Silliman's Journal, 1833, xxiv. 246.

[¶] Bibliotheque Universelle, 1837, vii. 388.

1639. Under these circumstances heating either electrode increased the current; heating both produced still more effect. When both were heated, if either were cooled, the effect on the current fell in proportion. The proportion of effect due to heating this or that electrode varied, but on the whole heating the negative seemed to favour the passage of the current somewhat more than heating the positive. Whether the application of heat were by a flame applied underneath, or one directed by a blow pipe from above, or by a hot iron or coal, the effect was the same.

1640. Having thus removed the difficulty out of the way of my views regarding a current, I did not pursue this curious experiment further. It is probable, that the difference between my results and those of M. de la Rive may depend upon the relative values of the currents used; for I employed only a weak one resulting from two pairs of plates two inches long and half an inch wide, whilst M. de la Rive used four pairs of plates of sixteen square inches in surface.

1641. Electric discharges in the atmosphere in the form of balls of fire have occasionally been described. Such phenomena appear to me to be incompatible with all that we know of electricity and its modes of discharge. As time is an element in the effect (1418. 1436.) it is possible perhaps that an electric discharge might really pass as a ball from place to place; but as every thing shows that its velocity must be almost infinite, and the time of its duration exceedingly small, it is impossible that the eye should perceive it as anything else than a line of light. That phenomena of balls of fire may appear in the atmosphere, I do not mean to deny; but that they have anything to do with the discharge of ordinary electricity, or are at all related to lightning or atmospheric electricity, is much more than doubtful.

1642. All these considerations, and many others, help to confirm the conclusion, drawn over and over again, that the current is an indivisible thing; an axis of power, in every part of which both electric forces are present in equal amount* (517. 1627.). With conduction and electrolyzation, and even discharge by spark, such a view will harmonize without hurting any of our preconceived notions; but as relates to convection, a more startling result appears, which must therefore be considered.

---

* I am glad to refer here to the results obtained by Mr. Christie with magneto-electricity, Philosophical Transactions, 1833, p. 113. note. As regards the current in a wire, they confirm everything that I am contending for.

1643. If two balls A and B be electrified in opposite states and held within each other's influence, the moment they move towards each other, a current, or those effects which are understood by the word current, will be produced. Whether A move towards B, or B move in the opposite direction towards A, a current, and in both cases having the same *direction*, will result. If A and B move from each other, then a *current* in the opposite direction, or equivalent effects, will be produced.

1644. Or, as charge exists only by induction (1178. 1299.), and a body when electrified is necessarily in relation to other bodies in the opposite state; so, if a ball be electrified positively in the middle of a room and be then moved in any direction, effects will be produced, as if a *current* in the same direction (to use the conventional mode of expression) had existed: or, if the ball be negatively electrified, and then moved, effects as if a current in a direction contrary to that of the motion had been formed, will be produced.

1645. I am saying of a single particle or of two what I have before said, in effect, of many (1633.). If the former account of currents be true, then that just stated must be a necessary result. And, though the statement may seem startling at first, it is to be considered that, according to my theory of induction, the charged conductor or particle is related to the distant conductor in the opposite state, or that which terminates the extent of the induction, by all the intermediate particles (1165. 1295.), these becoming polarized exactly as the particles of a solid electrolyte do when interposed between the two electrodes. Hence the conclusion regarding the unity and indentity of the current in the case of convection, jointly with the former cases, is not so strange as it might at first appear.

1646. There is a very remarkable phenomenon or effect of the electrolitic discharge, first pointed out, I believe, by Mr. Porrett, of the accumulation of fluid under decomposing action in the current on one side of an interposed diaphragm.* It is a mechanical result; and as the liquid passes from the positive towards the negative electrode in all the known cases, it seems to establish a relation to the polar condition of the dielectric in which the current exists (1164. 1525?). It has not as yet been sufficiently investigated by experiment; for De la Rive says,† it requires that the water should be a bad conductor, as, for instance, distilled water, the effect not happening with strong solutions; whereas, Dutrochet says‡ the contrary is the case, and that, the effect is not directly due to the electric current.

1647. Becquerel in his Traité de l'Electricité has brought together the considerations which arise for and against the opinion, that the effect generally is an electric effect.§. Though I have no decisive fact to quote at present, I cannot refrain from venturing an opinion, that the effect is analogous both to combination and convection (1623.), being a case of carrying due to the relation of the diaphragm and the fluid in contact with it, through which the electric discharge is jointly effected; and further, that the peculiar relation of positive and negative small and large surfaces already referred to (1482. 1503. 1525.), may be the direct cause of the fluid and the diaphragm travelling in contrary but determinate directions. A very valuable experiment has been made by M. Becquerel with particles of clay,[*] which will probably bear importantly on this point.

---

* Annals of Philosophy, 1816, viii. p. 75.

† Annales de Chimie, 1835, xxviii. p. 196.

‡ Annales de Chimie, 1832, xlix. p. 423.

§ Vol. iv. p. 197, 192.

1648. *As long as* the terms *current* and *electro-dynamic* are used to express those relations of the electric forces in which progression of either fluids or effects are supposed to occur (283.), *so long* will the idea of velocity be associated with them; and this will, perhaps, be more especially the case if the hypothesis of a fluid or fluids be adopted.

1649. Hence has arisen the desire of estimating this velocity either directly or by some effect dependent on it; and amongst the endeavours to do this correctly, may be mentioned especially those of Dr. Watson[†] in 1748, and of Professor Wheatstone[‡] in 1834; the electricity in the early trials being supposed to travel from end to end of the arrangement, but in the latter investigations a distinction occasionally appearing to be made between the transmission of the effect and of the supposed fluid by the motion of whose particles that effect is produced.

1650. Electrolytic action has a remarkable bearing upon this question of the velocity of the current, especially as connected with the theory of an electric fluid or fluids. In it there is an evident transfer of power with the transfer of each particle of the anion or cathion present, to the next particles of the cathion or anion; and as the amount of power is definite, we have in this way a means of localizing as it were the force, identifying it by the particle and dealing it out in successive portions, which leads, I think, to very striking results.

1651. Suppose, for instance, that water is undergoing decomposition by the powers of a voltaic battery. Each particle of hydrogen as it moves one way, or of oxygen as it moves in the other direction, will transfer a certain amount of electrical force associated with it in the form of chemical affinity (822. 852. 918.) onwards through a distance, which is equal to that through which the particle itself has moved. This transfer will be accompanied by a corresponding movement in the electrical forces throughout every part of the circuit formed (1627. 1634.), and its effects may be estimated, as, for instance, by the heating of a wire (852.) at any particular section of the current however distant. If the water be a cube of an inch in the side, the electrodes touching, each by a surface of one square inch, and being an inch apart, then, by the time that a tenth of it, or 25·25 grains, is decomposed, the particles of oxygen and hydrogen throughout the mass may be considered as having moved relatively to each other in opposite directions, to the amount of the tenth of an inch; i. e. that two particles at first in combination will after the motion be the tenth of an inch apart. Other motions which occur in the fluid will not at all interfere with this result; for they have no power of accelerating or retarding the electric discharge, and possess in fact no relation to it.

---

[*] Traite de l'Electricite, i. p. 285.

[†] Philosophical Transactions, 1748.

[‡] Ibid. 1834, p. 583.

1652. The quantity of electricity in 25·25 grains of water is, according to an estimate of the force which I formerly made (861.), equal to above 24 millions of charges of a large Leyden battery; or it would have kept any length of a platina wire 1-104 of an inch in diameter red hot for an hour and a half (853.). This result, though given only as an approximation, I have seen no reason as yet to alter, and it is confirmed generally by the experiments and results of M. Pouillet.* According to Mr. Wheatstone's experiments the influence or effects of the current would appear at a distance of 576,000 miles in a second.† We have, therefore, in this view of the matter, on the one hand, an enormous quantity of power equal to a most destructive thunder storm appearing instantly at the distance of 576,000 miles from its source, and on the other, a quiet effect, in producing which the power had taken an hour and a half to travel through the tenth of an inch: yet these are the equivalents to each other, being effects observed at the sections of one and the same current (1634.).

1653. It is time that I should call attention to the lateral or transverse forces of the *current*. The great things which have been achieved by Oersted, Arago, Ampere, Davy, De la Rive, and others, and the high degree of simplification which has been introduced into their arrangement by the theory of Ampere, have not only done their full service in advancing most rapidly this branch of knowledge, but have secured to it such attention that their is no necessity for urging on its pursuit. I refer of course to magnetic action and its relations; but though this is the only recognised lateral action of the current, there is great reason for believing that others exist and would by their discovery reward a close search for them (951.).

1654. The magnetic or transverse action of the current seems to be in a most extraordinary degree independent of those variations or modes of action which it presents directly in its course; it consequently is of the more value to us, as it gives us a higher relation of the power than any that might have varied with each mode of discharge. This discharge, whether it be by conduction through a wire with infinite velocity (1652), or by electrolyzation with its corresponding and exceeding slow motion (1651.), or by spark, and probably even by convection, produces a transverse magnetic action always the same in kind and direction.

---

* Becquerel, Traite de l'Electricite, v. p. 278.

† Philosophical Transactions, 1834, p. 589.

1655. It has been shown by several experimenters, that whilst the discharge is of the *same kind* the amount of lateral or magnetic force is very constant (366. 367. 368. 376.). But when we wish to compare discharge of different kinds, for the important purpose of ascertaining whether the same amount of current will in its *different forms* produce the same amount of transverse action, we find the data very imperfect. Davy noticed, that when the electric current was passing through an aqueous solution it affected a magnetic needle*, and Dr. Ritchie says, that the current in the electrolyte is as magnetic as that in a metallic wire†, and has made water revolve round a magnet as a wire carrying the current would revolve.

1656. Disruptive discharge produces its magnetic effects: a strong spark, passed transversely to a steele needle, will magnetise it as well as if the electricity of the spark were conducted by a metallic wire occupying the line of discharge; and Sir H. Davy has shown that the discharge of a voltaic battery in vacuo is affected and has motion given to it by approximated magnets‡.

1657. Thus the three very different modes of discharge, namely, conduction, electrolyzation, and disruptive discharge, agree in producing the important transverse phenomenon of magnetism. Whether convection or carrying discharge will produce the same phenomenon has not been determined, and the few experiments I have as yet had time to make do not enable me to answer in the affirmative.

1658. Having arrived at this point in the consideration of the current and in the endeavour to apply its phenomena as tests of the truth or fallacy of the theory of induction which I have ventured to set forth, I am now very much tempted to indulge in a few speculations respecting its lateral action and its possible connection with the transverse condition of the lines of ordinary induction (1165. 1304.). I have long sought and still seek for an effect or condition which shall be to statical electricity what magnetic force is to current electricity; for as the lines of discharge are associated with a certain transverse effect, so it appeared to me impossible but that the lines of tension or of inductive action, which of necessity precede that discharge, should also have their correspondent transverse condition or effect (951.).

1659. According to the beautiful theory of Ampere, the transverse force of a current may be represented by its attraction for a similar current and its repulsion of a contrary current. May not then the equivalent transverse force of static electricity be represented by that lateral tension or repulsion which the lines of inductive action appear to possess (1304.)? Then again, when current or discharge occurs between two bodies, previously under inductrical relations to each other, the lines of inductive force will weaken and fade away, and, as their lateral repulsive tension diminishes, will contract, and ultimately disappear in the line of discharge. May not this be an effect identical with the attractions of similar currents? i. e. may not the passage of static electricity into current electricity, and that of the lateral tension of the lines of inductive force into the lateral attraction of lines of similar discharge, have the same relation and dependencies, and run parallel to each other?

---

* Philosophical Transactions, 1821, p. 426.

† Ibid. 1832, p. 294.

‡ Philosophical Transactions, 1821, p. 427.

1660. The phenomena of induction amongst currents which I had the good fortune to discover some years ago (6. &c. 1048.)may perchance here form a connecting link in the series of effects. When a current is first formed, it tends to produce a current in the contrary direction in all the matter around it; and if that matter have conducting properties and be fitly circumstanced, such a current is produced. On the contrary, when the original current is stopped, one in the same direction tends to form all around it, and, in conducting matter properly arranged, will be excited.

1661. Now though we perceive the effects only in that portion of matter which,being in the neighbourhood, has conducting properties, yet hypothetically it is probable, that the non-conducting matter has also its relations to, and is affected by, the disturbing cause, though we have not yet discovered them. Again and again the relation of conductors and non-conductors has been shown to be one not of opposition in kind, but only of degree (1334. 1603.), and, therefore, for this, as well as for other reasons, it is probable, that what will affect a conductor will affect an insulator also; producing perhaps what may deserve the term of the electrotonic state (60. 242. 1114.).

1662. It is the feeling of the necessity of some lateral connexion between the lines of electric force (1114.); of some link in the chain of effects as yet unrecognised, that urges me to the expression of these speculations. The same feeling has led me to make many experiments on the introduction of insulating dielectrics having different inductive capacities (1270. 1277.) between magnetic poles and wires carrying currents, so as to pass across the lines of magnetic force. I have employed such bodies both at rest and in motion, without, as yet, being able to detect any influence produced by them; but I do by no means consider the experiments as sufficiently delicate, and intend, very shortly, to render them more decisive.

1663. I think the hypothetical question may at present be put thus: can such considerations as those already generally expressed (1658.) account for the transverse effects of electrical currents? are two such currents in relation to each other merely by the inductive condition of the particles of matter between them, or are they in relation by some higher quality and condition (1654.), which, acting at a distance and not by the intermediate particles, has, like the force of gravity, no relation to them?

1664. If the latter be the case, then, when electricity is acting upon and in matter, its direct and its transverse action are essentially different in their nature; for the former, if I am correct, will depend upon the contiguous particles, and the latter will not. As I have said before, this may be so, and I incline to that view at present, but I am desirous of suggesting considerations why it may not, that the question may be thoroughly sifted.

1665. The transverse power has a character of polarity impressed upon it. In the simplest forms it appears as attraction or repulsion, according as the currents are in the same or different directions: in the current and the magnet it takes up the condition of tangential forces; and in magnets and their particles produces poles. Since the experiments have been made which have persuaded me that the polar forces of electricity, as in induction and electrolytic action (1298. 1343.), show effects at a distance only by means of the polarized contiguous and intervening particles, I have been led to expect that *all polar forces* act in the same general manner; and the other kinds of phenomena which one can bring to bear upon the subject seem fitted to strengthen that expectation. Thus in crystallizations the effect is transmitted from particle to particle; and in this manner, in acetic acid or freezing water a crystal a few inches or even a couple of feet in length will form in less than a second, but progressively and by a transmission of power from particle to particle. And, as far as I remember, no case of polar action, or partaking of polar action, except the one under discussion, can be found which does not act by contiguous particles.* It is apparently of the nature of polar forces that such should be the case, for the one force either finds or developes the contrary force near to it, and has, therefore, no occasion to seek for it at a distance.

1666. But leaving these hypothetical notions respecting the nature of the lateral action out of sight, and returning to the direct effects, I think that the phenomena examined and reasoning employed in this and the two preceding papers tend to confirm the view first taken (1164), namely, that ordinary inductive action and the effects dependent upon it, are due to an action of the contiguous particles of the dielectric interposed between the charged surfaces or parts which constitute, as it were, the terminations of the effect. The great point of distinction and power (if it have any) in the theory is, the making the dielectric of essential and specific importance, instead of leaving it as it were a mere accidental circumstance or the simple representative of space, having no more influence over the phenomena than the space occupied by it. I have still certain other results and views respecting the nature of the electrical forces and excitation, which are connected with the present theory; and, unless upon further consideration they sink in my estimation, I shall very shortly put them into form as another series of these electrical researches.

Royal Institution, Feb. 4, 1838.

---

* I mean by contiguous particles those which are next to each other, not that there is *no* space between them. (See 1616.)

Mr. Loch.—How are those wires kept insulated in the tubes? First, the wires are insulated from each other by a mixture of cotton and india rubber, which is a very good insulating material; then, these prepared wires are all passed, with certain precautions, through an iron tube, which in some parts of the line is buried beneath the ground, and in other parts of the line is raised above it.

That mixture of cotton and india-rubber cuts off all communication between the wire and the tube?—Yes, and between the separate wires; it is a sufficient non-conductor.

Chairman.—You say, a guard may communicate by means of one of these posts put up, with any station? With the stations either way.

He must carry a portable apparatus? Yes.

That must be the nature of the keys, must it not? The telegraphic apparatus necessarily consists of two parts, the communicating keys and the dial on which the indicated characters are seen; that which the guard carries must contain both these parts here; the keys and the dial are in the same apparatus.

Lord Granville Somerset.—Suppose the Great Western Railway were completed between London and Bristol, do you contemplate the possibility of carrying your telegraph through the whole way, so as to signify from London to Bristol anything you wish to communicate, and *vice versâ* from Bristol to London? The experiment has not been tried, but I have every reason to believe that it can be done.

You must multiply your power considerably in that case; but if you can multiply your power sufficiently, there is no difficulty, in your opinion, in performing that? One very important circumstance I have ascertained is the little power requisite to produce this effect; it was formerly thought that to send a current to any considerable extent, very strong batteries must be employed, but in fact a very weak battery is sufficient, provided only it consists of a number of elements proportionate to the distance.

Do you see any practical difficulty in proportioning the number of your batteries to that extent, of 100, or 120 miles? I think there is none.

So far as your experiments have gone, you think you should be able to effect this telegraphic communication between Bristol and London? Yes; possibly several stations may be required, but, at any rate, the stations may be at far greater distances from each other than would be required for any ordinary system of telegraphs; my opinion is, that the intermediate stations will not be required.

You think you may communicate to the Reading station, the Reading station to another in the direction of Bristol, and that to Bristol? Yes; this means would be adopted if it should be found impracticable to effect an immediate communication between the two extreme stations.

Mr. French.—Have you any doubt you could do it with one intermediate station, dividing the distance? The experiment has not been tried; if perfect insulation of the wires can be obtained, there will be no difficulty; theoretically there is no difficulty, but we might meet with practical obstacles in so long a line.

Lord Granville Somerset.—How long has this line been laid down upon the Great Western? I think it was finished in July last. (1839.)

Do you think you have had experience enough during the last winter to ascertain that it will not fail you in consequence of any inclemency of weather, or circumstances of that nature? If the wires are properly protected, I think there is no fear whatever.

Do you conceive they can be so protected, that weather will have no effect upon them? Yes, that is my judgment, from experiment.

Mr. Loch.—Is there any appreciable loss of time in making a communication from the Paddington station to the extremity of the line to which the telegraph is now carried? From some experiments I made some years ago, published in the *Philosophical Transactions,* when I first turned my attention to the possibility of effecting telegraphic communications, I ascertained that electricity travelled through a copper wire at the rate of about 200,000 miles in a second; consequently there is no appreciable time lost in the communication of the electrical effect; the only time that would be lost would be at relay stations, if they were necessary.

Mr. Freshfield.—Suppose you want to communicate from London to Bristol, how do you signify that your intention is to communicate with Bristol and not with Drayton? There would be a separate signal appropriated to each station, which would be made before the communication begins, immediately after the alarum has been rung.

Mr. Loch.—What is the rate at which light travels? 192,000 miles in a second.

What you described in the first instance is the mode of asking the question; how is the message received? The person who is attending reads the message from the dial.

Chairman.—Have the applications you have had from foreign countries to put up this means of communication been in connection with railways, or separate from railways? All in connection with railways.

Is it of any consequence that it should be on a railway, or does a railway offer any advantage in that respect? Not the slightest advantage with regard to laying down the line, but a great one with respect to its protection from injury.

Sir John Guest.—Have you tried to pass the line through water; There would be no difficulty in doing so, but the experiment has not yet been made.

Chairman.—Could you communicate from Dover to Calais in that way? I think it perfectly practicable.

Have you any further observations to make? An electrical telegraph offers a great many advantages over an ordinary telegraph; it will work day and night, but an ordinary telegraph will act only during day; it will also work in all states of weather, an ordinary telegraph can only work in fine weather. There are a great number of days in the year in which no communication can be given by an ordinary telegraph, and, besides, a great many communications are stopped before they can be finished, on account of changes in the state of the atmosphere. No inconveniences of this kind would attend the electrical telegraph. Another advantage is, that the expense of the separate stations is by no means comparable to that of the ordinary telegraph; no look-out-men are required, and the apparatus may be worked in any room where there are persons to attend to it. There is another advantage the electric possesses over the ordinary telegraph. viz. the rapidity with which the signals may be made to follow each other. Thirty signals may be conveniently made in a minute; that number cannot be made by the ordinary telegraph. There is one thing I will take the opportunity to mention: I have been confining the attention of the committee to the telegraph now working on the Great Western Railroad, but having lately occupied myself in carrying into effect numerous improvements which have suggested themselves to me, I have, conjointly with Mr. Cooke, who has turned his attention greatly to the same subject, obtained a new patent for a telegraphic arrangement, which I think will present very great advantages over that which at present exists. It can be applied without entailing any additional expense of consequence to the line now laid down; it will only be necessary to substitute the new for the former instruments. This new apparatus requires only a single pair of wires to effect all which the present one does with five, so that three independent telegraphs may be immediately placed on the line of the Great Western; it presents in the same place all the letters of the alphabet according to any order of succession, and the apparatus is so extremely simple, that any person without any previous acquaintance with it can send a communication and read the answer. This apparatus I shall be happy to show the committee in action at King's College.

Sir John Guest.—Does not the possibility of cutting off the communication between one point and another, occur to you? The same objection may be made with regard to railroads themselves.

Suppose any person were to stop the communication from one town to another? By destroying the continuity of the rails they might stop the passage of the trains.

The common telegraphic communication could be kept up notwithstanding that? Certainly.

Chairman.—Can you state the expense which would be incurred in laying it down? I am hardly prepared to state that, because only one line has been laid down at present.

Mr. Loch.—Would it be your view ultimately, supposing the railway completed to Bristol, that there should be one line to telegraph to and another from Bristol? No; the same line will serve both purposes.

There will be no inconvenience in practice in making use of the same line? No.

Mr. H. Baring.—This sort of telegraph is not in operation on any other railroad? The Blackwell Company shortly intend to have it.

CHARLES ALEXANDER SAUNDERS, ESQ., CALLED IN AND EXAMINED.

Lord Granville Somerset.—As secretary of the Great Western Railroad Company, can you state to the committee whether they have adopted Mr. Wheatstone's magnetic telegraph? As far as West Drayton, 13 miles.

How long has it been adopted? It was finished in July last; it has been in operation about seven or eight months.

Was that laid down at the expense of the railroad? It was laid down under an agreement with Mr. Cooke, who is one of the patentees.

Was it on behalf of Mr. Wheatstone also? It was under agreement with Mr. Cooke, Mr. Cooke being the co-patentee.

Does that agreement extend to any length of time, or has this been only an experiment? The agreement contemplated the further extension of it, if the Company required it within a certain period of time, after the completion of the first 13 miles.

In fact the Company have laid down this magnetic telegraph at their own expense, under a specific agreement with Mr. Cooke, the Company taking the expense on the one hand and deriving any benefit they may derive on the other? Yes, just so.

That agreement is determinable at a certain period? It is.

Is it renewable at the option of either party? No; I think it is absolutely determinable, not renewable.

Have you any objection to state the term of years? I have no objection to state the substance of the agreement, but it is very long and very intricate; the material substance of it is this: that within a certain number of months after the telegraph shall have been laid and efficiently worked between Paddington and Drayton, the company might call upon the patentees to give them a license for the whole line, on certain terms; there are a variety of further considerations involved in the agreement, which it would be very difficult to relate.

Is this agreement binding upon the patentees, so as to enable the Great Western Company to execute this telegraph all the way from Bristol to London, for a certain number of years? It was binding upon them, but the time has now expired.

A new arrangement must be made before any permanent agreement is effected? Yes, neither party is now bound by that agreement.

Have all the advantages which were anticipated from this telegraph accrued? I think we have scarcely had it in a state to say that we have derived all the advantages which were contemplated from it, because we have, between West Drayton and Paddington, very little inducement to work the telegraph separately for that part; it had much more reference to the more distant stations, and the communications of our line with others, or to communications between places on the line where short and long trains together are running upon the same portion of railroad. As yet we have had no practical benefit of that description, but it has enabled us to ascertain that the telegraph perfectly performs all the duty that was expected of it; as far as it goes, it works perfectly true.

Provided it shall work as well when your line is completed, do you anticipate all those useful results that were anticipated before it was laid down? I do, indeed.

That is your opinion, after your experience of eight or nine months on 13 miles? Yes.

In general terms, is it a very expensive thing to lay down this magnetic telegraph? It is expensive, but that is a question of degree: I have no objection to state the expense incurred; I believe it may be laid at from £250. to £300. a mile, including the charge for station instruments.

In the discussions which have taken place, of which you may have been cognizant, upon the subject of railroad telegraphs, have the directors contemplated the conveyance of ordinary articles of intelligence between Bristol and London? I think that view was entertained by the company when they originally tried it; the object would be to facilitate all means of communication.

Do you consider that that would be the only means by which the company would be remunerated for the outlay? I think the usefulness of it to the railway itself is the chief remuneration; it is calculated undoubtedly to simplify the working of the railway, and to diminish the stock of every description, whether of engines or of carriages; to insure greater punctuality, and, in cases of accident, to repair the injury with the least delay, as well as to produce general advantages and greater security in working the railway.

You think you might have a less establishment, and less stock, in consequence of having this magnetic telegraph, than you otherwise would be obliged to keep up to conduct the line? Undoubtedly.

And that in that way the company would be remunerated? I think that would be a mode of remuneration; I do not say to what extent it would operate, as compared with the expense of the telegraph itself.

In addition to the remuneration thus derived, do you conceive it will be an effectual mode of assisting in case of accident to passengers? Certainly, it would be so.

And in some instances of preventing accidents? Yes; if a line were at any place stopped up, and a communication could be made by telegraph, it would prevent the danger of collision from a subsequent train running up to the place of danger.

Mr. Wheatstone has stated that it is intended that a guard should have a portable telegraph, capable of operating at the distance of every quarter of a mile? Yes, that is a plan proposed; it has not been carried into effect on our line at present; there are places to which the portable telegraphs may be applied, but the men have not been instructed in it yet.

Supposing that idea were carried out, would it not be the cause of great safety in case of sudden emergencies, or fear of accidents? Yes; I have no doubt security against accidents would result, and more prompt assistance in case of accident.

Suppose an engine unexpectedly became unfit for service, have you not, in the course of the last few months, occasionally sent to another station for another engine, by means of the telegraph? Yes, we have, on one or two occasions within a few months; we worked the telegraph for nearly two months, so as to communicate to Paddington the moment of the passing of the train at West Drayton and Hanwell; that was done for the purpose of trying whether the telegraph would constantly work, and whether we could rely upon it, and it answered the purpose, certainly, admirably.

Do you contemplate continuing that constant use of it? No, we do not work it in that way; but it is used in any emergency; they can transmit any intelligence between West Drayton and Paddington, which it may be material to receive.

If parties will want horses when they come to the Paddington station, you are in the habit of sending on intelligence of that? Yes, we are; I think the chief use of the telegraph, what I consider the chief advantage of it, would be upon the junction of two lines, where they are to be worked by the engines of one line; for instance, upon the line from Bristol to London, at the junction of the Cheltenham Railway, it would be a very great facility indeed if it could be ascertained at the moment at which the train comes up from Bristol which is to receive the Cheltenham traffic, that the Cheltenham train is on its progress, and either within five minutes or not within five minutes of the place; by that means there would be no useless delay to either train, and in the same manner the down train coming up would be able to send previous intelligence from a station, by which the engine from the Cheltenham train would be ready at any time to take the train on without any loss of time.

It would also, in case of any want of exactness in the arrival of a train, prevent collision, would it not? It would, and it would reduce the expense of working the line; the superintendent might be enabled, in many cases, by delaying the train only a few minutes, to save the expense of a second engine being sent for a long distance.

In case of any severe fogs in any particular district, would it not be a great advantage that the trains coming into that district should be made aware of that circumstance? Yes, I think in the case of our working short trains, which we shall probably do from Slough to London, independently of the Bristol trains, it would be very important for us to know at Paddington when a train is approaching, whether it be a Slough or a Bristol train, and for those at Slough to know that the long train is coming up, and is within a certain distance, or not within a certain distance, that they may prepare accordingly, whether to send on that train from Slough to London, or delay it for a short time.

Suppose you wish to send an extra train from one point of your line to another, without any means of communication, there must be always a certain degree of danger either of running into another train, or meeting another train? Yes, we are always obliged to allow a certain interval to elapse before another train is sent.

That is not always a certain means of preventing collision, is it? No, it is not.

By means of this telegraph, could not you guard against the danger of accident in that respect? Undoubtedly, it would tend to security in those cases.

Mr. Loch.—Would not the possession of such a means of conveyance, after the telegraph is completed as far as Bristol, give the possessor of the telegraph a great advantage in a commercial point of view over the rest of the public? It might do so, if they should choose so to avail themselves of their property.

Has it ever occurred to you what remedy the public might have under those circumstances? I do not see how they possibly could have any remedy at all; I do not see why they ought to have any remedy.

Would it be unfair, under those circumstances, that the Railway Company should give facilities to other parties to erect other telegraphs along their lines, paying the company for such facilities? I think the company would not object to other parties having a facility if they were sufficiently paid for it; but I cannot conceive if a party possesses property, why he should refuse to make it useful to himself, or why he should be called upon to make it as useful to another as to himself.

Take the railway to Portsmouth, would it be at all a matter that would be indifferent to the country, that the directors of that railroad should have the means of communicating by means of their telegraph with London, while the Government is deprived of all communication between the principal naval station and the capital in the same manner? I think the case cannot arise; Government will have the power of course, if they choose to pay for it, of putting a telegraph of their own between Portsmouth and London; and there is no telegraph which could exist, whether on the Southampton or any other railway company possessing which would prevent the Government having the use of it, if they choose to pay for it, Government might have one, of course, if they would go to the expense of making it.

What expense do you refer to? The expense of buying land and putting it down.

Would it not be a much more ready way to give the Government the power to lay down the telegraph on the railway itself? Paying for it, I do not see the slightest objection to it.

Lord Granville Somerset.—Suppose a restriction of the advantages of the railway company to that which may be called their own peculiar business, and not allowing it to transmit other intelligence? It strikes me that that would be a prohibition to the company laying it down at all.

Mr. Loch.— You think if this rule were laid down, that all the intelligence of those who telegraph should be made public with the exception of that on their own affairs, that would operate as a prohibition to their laying it down at all? I scarcely know as to any rule of its being made public; I am answering these questions very much in the dark, but it strikes me that saying "You may lay it down, but you shall not use it except in a particular way," would amount to a prohibition.

Mr. Green.—Do you see any objection to compelling the company to allow persons to send any information they please by means of your telegraph? I see none at all, under particular arrangements, inasmuch as I think that is what they would do as a matter of course; but then it must be subject to certain regulations of the company; they could not consent to its being taken out of their hands, when they are using it, and given to another; of course the transmission of general intelligence would be one source of income derived from it.

Mr. Loch.—How would this operate upon the construction of a telegraph of this sort, if the government were to have the power, paying for it, to be enabled to lay down a telegraph of their own; would that operate with the directors in preventing their laying down one of their own? I think not at all; I cannot conceive that it would be their wish to prevent government possessing one. I think if an expenditure shall have been incurred by any company in laying down one under the expectation that they will derive the benefit of it, whether in transmitting railway information or general information, being properly paid for it, if they should be obliged to permit another company to lay down another telegraph on their line, that would be a great hardship; but I am sure they would do everything they could to facilitate the views of government.

Lord Granville Somerset.—Supposing the government were to lay down a magnetic telegraph from London to Bristol, and supposing that any parties were allowed, under certain regulations, to communicate by that telegraph, would you see any objection to that? I expressly reserved that it should be used for government purposes only. It never could answer to lay down two telegraphs; it would be a great hardship to make the possessor of the soil give up his right to enable some other party to compete with him.

Mr. Loch.—Confining it to government purposes, you see no objection to allowing the government to lay an electrical telegraph? None whatever; at the same time I should state that it is a subject I have not much thought of or considered, and therefore I fear my opinion its worth nothing.

Chairman.—Why was not your telegraph put under ground? It was attempted at first to be put under ground, but the wet got so much to it, it was found better to put it above ground, to secure it from that injury. I believe one of the great difficulties the patentee had to contend with at the time has been since remedied by making the tubes more impervious to the wet.—(Mr. *Wheatstone*.) I wish to make an observation with regard to the expense of the line: the cost of the present experiment has exceeded 250*l*. per mile. We will assume that it cannot safely be reduced, though I think with more experience it might be; if we consider that the cost of laying down the whole telegraphic line from London to Bristol will be only the cost of one mile of the railroad itself, the expenditure will not appear great, considering the benefits to be obtained; this is less than one per cent. upon the original estimate of the expenditure. Now would it not be worth while to go to that expense to obtain all the advantages that will undoubtedly be obtained by the telegraph? I will make a few observations with regard to the proposed Government line. The principal expense of laying down the telegraph line is, in fact, the iron tube and the other things connected with it. The mere cost of the wires is very little, not more than 6*l.* or 7*l.* per mile each. As many wires may be put as you please in the same tube, consequently, supposing an iron tube to be laid down from hence to Portsmouth, if wires for three distinct lines were enclosed within it, the expense of each line, considered separately, would be very considerably diminished. One line might be appropriated for the rail-road purposes alone, another for general commercial intercourse, that is, for sending messages for any parties who choose to pay for the accommodation; and a third for the exclusive use of the Government. There would be no difficulty, if the Government have a telegraphic line thus associated with the others, to make the terminations in their own offices, from the admiralty in London, for instance, to any office belonging to the same department at Portsmouth, so that information may be sent without communicating with any persons but their own clerks. If this plan were adopted, it would do away with every objection which has been made with regard to the injury a private Company would do to the public, by having the exclusive means of intelligence in their own hands; and I am sure any railway company would enter willingly into an arrangement, by which Government might possess an exclusive line at a very moderate expense, much below that at which they could lay it down themselves. If the new telegraph of which I have spoken succeeds, and it has succeeded perfectly so far as experiments have yet been tried, we might place three telegraphs in connection with the six wires now used on the Great Western Railway, and these might be applied, as I have said before, to three specific purposes; one exclusively for railway purposes, another to be let to any persons who choose to avail themselves of it, and another for Government objects.

Would it be possible, by any portable instruments, for any third party to become acquainted with the messages sent on account of the government? If the government feared anything of that kind, they must use a cypher; communications by the electric telegraph would be far less public than by the present visual mode; at present every-body knows when a telegraph message is being despatched, and any person acquainted with the signals might read it.

Is it not the case, that by a little attention any person can possess themselves of any cypher? Very ingenious systems have certainly been decyphered, without any knowledge of their keys; but the task is no easy one. An extremely simple and safe mode of cypher has been devised, by means of which a person may communicate with a thousand correspondents, it being impossible for any one of them to read what is intended for another.

Mr. Freshfield.—Have you made a calculation of the probable length of time the apparatus will continue, without requiring to be renewed? That is a question I cannot answer; but it comes to this: how long can the iron tube which contains the wires be preserved; the wires themselves would remain uninjured for an indefinite period, if the tube be kept perfectly water-tight.

Do you think the wear and tear of the apparatus from London to Bristol would be less than the wear and tear of the railroad for one mile? Far less.

Mr. Loch.—Do you spell every word by the present mode? Some signals are used, but the words of a message are generally spelt.—(Mr. *Saunders.*) We have some conventional signals; the others are spelt. While we were working the telegraph, we worked it for some time intermediately through the Hanwell station, to try the effect of dividing it into different lines of telegraph; there was evidently no perceptible difference of time from Drayton to Hanwell, and from Hanwell to Paddington; for the same party having a double instrument an Hanwell, the instant he saw the signals on one he touched the keys of the other; the effect is quite instantaneous; in that way it might be sent to almost any distance.

(A description of the dial-plate of this telegraph, and of the arrangement of the magnetic needles, and their helices, will be given in our next number.)—Edit.

XLIX.—*On Electro-Magnetic Coil Machines*; By Thomas Wright, Esq.

Dear Sir,—Having been lately making some experiments with a view of determining the most efficient form which can be given to the coil in electro-magnetic coil machines, and having succeeded in producing a machine of great power in proportion to the quantity of wire employed, I proceed to lay an account of my experiments before the readers of your valuable journal.

I have long considered that both the *intensity* and quantity effects of these machines are due, rather to the *intensity* than the *quantity* of magnetism developed in the central core of iron wires. The coils which I have seen produced by the London philosophical instrument makers have been invariably short and thick, a form which I think very ill adapted to the purpose for which they were intended, as by this means a great *quantity* of magnetism is produced, but possessing very little *intensity*.

During the course of experiments which I instituted I employed 11 coils, the structure of which was as follows:—

No. 1, consisted of a core of soft iron wires 1-40[*] of inch in diameter, and four inches long, wound with 40 yards of copper wire 1-16.

No. 2. Core one foot long, half an inch in diameter, iron wires 1-60; battery helix 40 yards 1-16; superimposed helix 60 yards 1-40.

No. 3. Core two feet by half an inch, iron wires 1-40, battery helix 60 yards 1-16, superimposed helix 60 yards 1-40.

No. 4. Core one foot long, one inch in diameter, iron wires 1-40, helix 40 yards 1-16.

No. 5. Core two feet by half an inch, battery helix 40 yards 1-10, superimposed coil 60 yards 1-16.

No. 6. Core one foot and a half by half an inch, helix twenty yards 1-10.

Nos. 7 and 8. Cores each eight inches long, by a quarter of an inch, iron wires 1-60, battery helices each 25 yards 1-16, superimposed helices each 50 yards 1-60.

No. 9. Core eight inches long by half an inch, iron wires 1-20, helix 25 yards 1-16.

No. 10. Core eight inches long, half an inch square, composed of seven strips of sheet iron carefully annealed, battery helix 30 yards 1-16, superimposed helix 100 yards 1-60.

No. 11. A compound U shaped bar 20 inches long, and two inches in diameter, composed of hoop iron riveted together and wound with eight copper wires 1-16, 20 yards long, all covered together so as to form a single helix; the whole apparatus being fitted up with a revolving armature for the purpose of breaking contact with the battery.

The effects of the above coils when connected with a seven inch pair of zinc and copper plates excited *a la Mullins* were as follows:

Shock.	Spark.	Scintillation, from iron or steel.	Number of seconds required to produce a measure of Gas.
1. Scarcely perceptible	Small and bright	Very poor	18
2. From each helix strong, with both united[†] would fasten with drying hands.	Large, bright, and snapping	Very good	28
3. With both helices united excessively strong	Much larger but less bright, having a flashing appearance similar to the firing of a grain of gunpowder	Very bright from steel, not so good from iron	43
4. Effects similar to	No. 1	....	not tried
5[‡] Shock not so good as No. 2.	Very similar to No. 2		18
6. Shock just perceptible.	Large and bright	Very beautiful	10
7 & 8. Each of these coils with united helices gives a powerful shock, but when united in one length of 140 yards the shock is stronger than any I have yet experienced.	With both battery helices united in a double strand of 25 yards the spark is exceeding bright and large	brighter and larger than last	6
9. All the effects very	indifferent	....	
10. Not perceptible with battery helix, would not fasten with both helices united.	Large and bright	Very good	22

---

[*] The diameter of the wires in fractional parts of an inch.

[†] When the helices are mentioned as united, it is meant that the end of the

11. The effects from this helix with the small battery abovementioned were not so good as from Nos. 7 and 8 conjoined; when however it was connected with a series of four of Daniel's cells 3 feet high by 4 inches in diameter, the sparks and scintillations were very splendid; the decomposition with this battery were not tried, as I had not then a decomposing apparatus at hand.

All the iron used in the above coils was carefully annealed and the copper wire new *and had not been coiled at any previous time.*

From the foregoing experiments it would seem that the most efficient form that can be given to these machines, is that of an elongated helix enclosing a core of *thin* soft iron wires of not more than a quarter of an inch in diameter, and that if it should be required to increase the size of the machine, the number of coils and not their diameter or length, should be multiplied. The most advantageous length for the battery current seems to be about 20 or 25 yards, I am not, however, quite sure as to this, with regard to decompositions.

If the coils are multiplied great care must be taken that the length of wires, texture of metal, and method of coiling are *exactly similar* in each coil, otherwise the stronger currents will use the wire of the weaker ones as partial conductors, and thereby very greatly deteriorate the action of the whole of them: thus if we unite a helix of 40 yards and one of 60 in a double strand we shall not obtain near so strong a shock as from either of them singly; but if we unite two helices of equal length, the shock is sometimes better, the decomposition and deflagrations always so.

I have fitted up the coils 7 and 8 with my vibrating electrotome,* described in a late number of the "Annals": and by a particular arrangement of springs pressing against each other in various directions, I can instantly vary the quantity or intensity of the current, so as to obtain it from lengths of 25, 50, 100, or 150 yards, or from a double length of 25 yards.

The foot-board of the machine is hollow in order to admit a *flat* battery underneath.

Flat Battery.—Having noticed, (as I have no doubt many of your readers have) that in the ordinary cylindrical battery, the salt

---

battery helix is joined to the commencement of the upper one so as to form one continuous coil, the battery current being passed through the thicker helix.

† With this coil and both helices united, I was enabled to imitate the action of the Gymnotus Electricus with great success, by inserting two pieces of tinfoil in a tub of water and connecting them with the coil; if the hand was placed in the water a shock was immediately felt which was very severe when both hands were immersed.

* In my published description of this electrotome I was sorry to perceive an error in the drawing; the contact was there shown to be broken at the end of the spring, whereas it ought to have been at a point about an inch and a half nearer the middle of it; an electrotome thus constructed will vibrate a little time after being jerked by the finger, without the aid of the coil: it will be advisable to have the touching points, *which should be small*, tipped with platina; the spring should be about four inches long and press strongly on the brass bar against which it vibrates; some of the electrotomes that I have thus made are *quite musical.*

in the cupreous solution is very liable to subside, by means of which, the action, when long continued, is in a great measure confined to the lower part of the arrangement, which is evident from the copper becoming considerably thicker at the lower, than at the upper part, I was led to construct a battery in the following manner:—a piece of thin sheet copper was bent up at the edges in the form of a tray, in the inside of this was placed a similar tray of thin mil-board, cemented at the sides, and furnished with small legs of sealing wax.—Zinc and salt and water in the mill-board tray—sulphate of copper in the copper one.

This battery has many advantages:—it is contructed at an expense not greater than the cost of the materials—the action is very equable—and it can be easily slipped under the foot-board of any piece of apparatus to which it may be applied; if it is required to employ a series of them, they may be piled upon each other, the wires of the zinc plates pressing with a spring on the copper next above; and as it it is not necessary that the trays should be more than an inch deep, a large battery on this construction might be packed in a comparatively small space.

Mr. Uriah Clarke has, I am sorry to observe, accused me of copying his electro-magnetic machine; this is certainly not the case, and if necessary I could adduce proof to the contrary; I cannot say however that I see much resemblance in the two arrangements further than that they have each a reciprocating motion; I consider Mr. Clarke's much superior to mine as to the crank point, though I think he will find that great power is gained by partially continuing the armatures on the magnet. I have not proceeded with my engine, as I fancy I have hit upon a more efficient plan. I have very little expectation however of these engines being applicable to the working of machinery;—at least economically. They are very interesting toys.

I am, my dear Sir,

Very truly your's,

William Sturgeon, Esq. THOMAS WRIGHT.

L.—*On the Theory of Ætherfication, (Ætherbildung)*; By Professor Heinrich Rose.

It is known that many salts of the oxide of bismith, of the oxide of quicksilver, of the oxide of antimony, and other metallic oxides, become decomposed by water. They, usually, by that means become transformed into basic salts: but sometimes by the application of a sufficient quantity of water, the decomposition proceeds to the separation of pure oxide; as, for instance, with the nitrate of the oxide of quicksilver.

The explanations which are usually given of these decompositions is, that we admit that the water resolves the neutral salt of a metallic oxide into an acid and a basic salt, in a similar manner to that in which nitric acid transforms red super-oxide of lead into protoxide of lead, and brown super-oxide of lead. But the existence of acid salts, which, by the influence of the water on several neutral salts of metallic oxides, as has been supposed, is not satisfactorily proved: for in most cases the water takes only a part of the acid from the salt, and this dissolves some of the neutral salt, which, near the point of concentration of the acid solution by evaporation, in most cases crystallizes and separates as a neutral salt; and but very seldom as a double combination of neutral salt and acid hydrate. In many cases the quantity of salt which dissolves in the acid is exceedingly small; sometimes not any, and the whole quantity of the oxide forms an insoluble basic salt.

The easiest explanation which we can give of these decompositions by water, appears to me to be this, that it is the water which acts as a base, and separates the oxide as a basic salt; or, sometimes even in a pure condition, and combines with the acid to form a hydrate. This explanation will become the more admissable as we have already been accustomed to consider the hydrates of acids as saline compounds, in which the water represents a fixed base. It is well known what fertile inferences for the whole theory of chemistry have been drawn by these views of the nature of the action, and especially by Graham, Berzelius, and Liebig.

In fact, it is particularly the salts of such metallic oxides as are not possessed of strong basic properties which by water become decomposed. The salts of the more formidable bases do not display this phenomenon.

According to this view these decompositions are analagous to the conversion of the red oxide of lead into the brown super-oxide and the protoxide of lead by means of nitric acid, only that they are of a directly opposite kind; for the strong acid, in a combination of protoxide of lead with the peroxide, expels the electro-negative body, and combines with the basic.

The water occurs, also, as a base in other cases, and sometimes displaces other bases from their combinations. As, however, it always belongs to the weaker bases, and is at the same time volatile, such cases are not very frequent. But although itself volatile, it can expel the volatile oxide of ammonium from its combinations. If a solution of sulphate of the oxide of ammonium be boiled for a long time it becomes acid, and provided the boiling be in a retort, a fluid, containing free ammonia, distils over into the recipient. This result obviously proceeds from the water, as a base, expelling the oxide of ammonium, (which in a *free state* cannot exist, but is resolved into ammonia and water) from its combination with sulphuric acid, with which it enters into a combination. The quantity of the sulphate of the oxide of ammonia, which, in this way, becomes decomposed, is certainly very small; we must, however, consider that the oxide of ammonium belongs to the most powerful bases, and this result is principally to be attributed to its superior volatility.

If we apply the foregoing explanation of the decomposition of many salts by water, to the theory of Ætherification, much simplicity will be derived.

Berzelius and Liebig have adopted the view, that the æther may be regarded as a base, which view has found such general approbation that it is become almost universally adopted, at least in Germany.

It is known that the salts of the oxide of æthyl, (the compound æthers) by bases, become more or less easily decomposed when water is present: for those bases associate themselves with the acid of the compound, and liberate the oxide of æthyl as a hydrate (alcohol.)

The same decomposition, nevertheless, is also caused by water, which, in this case, obviously operates as a base. Some compounds of the oxide of æthyl become as easily decomposed by water, as by the operation of many other bases; as, for instance, is the case with oxalic æther, which, by water, becomes resolved into hydrate of oxalic acid and alcohol. To accomplish this transformation it is not necessary to employ a high temperature, because it takes place even at the common temperature, and, indeed, in a very short time.

The acid sulphate of oxide of æthyl, or rather the combination of the sulphates of the oxide of æthyl with hydrated sulphuric acid, (sulphovinic acid) in its solution in water, also suffers a precisely similar decomposition. Even at the common temperature, in this case, will alcohol and hydrate of sulphuric acid be gradually formed: but their formation is much quickened by boiling.

This process may also be easily explained on the supposition that the water operating as a base liberates the oxide of æthyl from its combination with the sulphuric acid, which, at the moment of separation, takes up water and forms alcohol.

The aqueous solutions of nearly all the sulphovinates become similarly decomposed, and especially when boiled. Alcohol and water evaporate, and in the solution is formed a so called acid sulphate, that is to say, a double compound of the neutral salt, which, with the hydrate of sulphuric acid, pre-existed in the sulphovinate salt.

If sulphovinic acid be heated with only a small quantity of water, no alcohol will be obtained, but principally hydrated sulphuric acid, and pure oxide of æthyl, or æther. There is not a sufficiency of water present to transform the liberated æther into alcohol.

If alcohol be mixed with hydrated sulphuric acid, sulphovinic acid becomes formed, or a double compound of neutral sulphate of oxide of æthyl with the hydrated sulphuric acid. By the formation of sulphate of oxide of æthyl two atoms of water are set free; one from the hydrated sulphuric acid and the other from the alcohol. By heating the mixture, one of these free atoms of water liberates oxide of æthyl from its combination with sulphuric acid, combines with the acid, and forms hydrated sulphuric acid.

But why does not the æther combine with water at the moment it is liberated, and thus form alcohol? The water is sufficiently plentiful, because the liberation of the æther requires only one atom of water; and at the formation of sulphovinic acid, even when anhydrous alcohol is employed, two atoms are set free.

It is known, that sulphuric acid can take up more than one atom of water to form a hydrate. We know, also, that, besides the common hydrate with one atom of water, there is a second, which can be prepared in a crystalline state, and which contains two atoms of water. This compound corresponds to a basic sulphate salt.

The disposition of the hydrate of sulphuric acid to take up more water is very great, and it is employed on this account for various purposes in our laboratories. It is this which prevents the æther, originating from the decomposition of the sulphovinic acid, from taking up the second atom of water; but if the mixture is uninterruptedly boiled for some time, the hydrated sulphuric acid loses the acquired water, which may then be distilled over in company with the æther. The æther may therefore, from a boiling mixture of the hydrate of sulphuric acid and alcohol, be distilled over at the same time with water; but they are not the products of one, but of two chemical processes, which are both active together in the boiling mixture.

At the commencement of the operation, but very little water passes over along with the æther and that alcohol contained in the mixture, which has not been converted into sulphovinic acid so, that the water remains dissolved in the distilled alcoholic æther, and does not separate: the quantity of water increases by further distillation, especially at a high temperature, when the quantity of the second hydrate of sulphuric acid has augmented.

Anhydrous alcohol is scarcely ever employed in the preparation of æther, but generally hydrated. It is evident that in the latter case the quantity of the second hydrate of sulphuric acid must be considerably increased. The experiments of Liebig, Magnus, and Marchand have shown that in the cold this second hydrate cannot form sulphovinic acid with alcohol, but does so at a higher temperature, and therefore that such a mixture on boiling can give æther by distillation. But it is known that by employing hydrated, or even anhydrous alcohol, there is always a portion of it which is not converted into sulphovinic acid, and this quantity may be distilled as alcohol from the mixture. A second portion of alcohol, which distils over in company with the æther, in the formation of æther, may, however, be produced in this way,—that æther and water are contemporaneously disengaged from the mixture, and combine to form alcohol; for it is produced only in this way when a solution of pure sulphovinic acid is boiled with much water, or compound æthers decomposed by water or by the hydrates of bases.

When, however, from the tendency of the hydrate of sulphuric acid to take up more water, æther has been evolved from a mixture of alcohol and sulphuric acid, it does not take up any water after being once separated: but water may be distilled over by heating the diluted sulphuric acid. We know that when æther is treated with water, or even dissolved in it, no alcohol is formed. When æther is once separated from a compound of oxide of æthyl, the former can in no way be converted by water into alcohol. Only when, as above observed, the æther comes in contact with water at the moment of its expulsion does it form alcohol with it. The contemporaneous disengagement of æther and water, from a boiling mixture of alcohol and the hydrate of sulphuric acid, shows therefore quite evidently that both owe their origin to two distinct processes.

Moreover, it is by no means an anomalous phænomenon that a base, which is capable of forming a hydrate, does not combine with water when brought into contact with it in a pure state; a great number of cases of this kind occur in inorganic chemistry. We need only compare æther with that numerous class of ignited oxides in which so compact a state of cohesion is produced by heat, that they not only withstand the action of water, but even entirely or partially that of acids, to find abundant proof of such analogies. The ignited oxides with these properties always belong to the weaker bases, under which æther must necessarily be classed. Æther may be assimilated to these oxides the more, as it like them combines directly with acids with difficulty.

But even among the stronger bases we find some whose relations to water resemble those of æther. When oxide of copper is precipitated in the cold by bases from solutions of salts of the oxides of copper, it appears as a hydrate of the oxide of copper; which, however, on being heated under water, loses its water, and does not take it up again when left in contact with it at a higher, or at the common temperature.

To find out at what period, in the preparation of æther by boiling a mixture of alcohol and sulphuric acid, water commences to pass over, M. Wittstock, at my request, instituted a series of experiments, which he had the kindness to communicate to me.

Two pounds of the hydrate of sulphuric acid were mixed cold with two pounds of anhydrous alcohol, the mixture was made to boil with all possible haste in a retort, the distilled products, well cooled, were gradually received, and the distillation continued until the contents of the retort boiled over.

The weight and specific gravity of the products were determined as they distilled over in succession. The results are as follows:—

First distillation: 3 drachms 50 grains; spec. gr. 0·776*; produced before the boiling of the mixture. The following products passed over after its boiling:—

Second: 3 ounces 6 drachms; spec. gr. 0·808.
Third: 3 ounces 6 drachms; spec. gr. 0·800.
Fourth: 3 ounces 6 drachms; spec. gr. 0·786.
Fifth: 3 ounces 5 drachms 50 grs.; spec. gr. 0·776.
Sixth: 4 ounces 1 drachm 50 grs.; spec gr. 0761.

---

* The specific gravities, both here as well as those to be mentioned subsequently, were all determined at 14° Reaum. (63·5° Fahr.).

Seventh: 1 ounce 7 drachms 10 grs.; spec. gr. 0·809.
Eighth: 1 ounce 2 drachms.

The first five products consisted of a single liquid; the sixth was the first in which a layer of water and of æther were perceptible. The quantity of separated water amounted to 3 drachms; the æthereal liquid had the specific gravity mentioned above. The seventh product consisted in volume of two parts water, and three parts of an æthereal fluid of the specific gravity stated; the eighth consisted almost entirely of water, above which floated a very thin layer of æther, which was coloured yellow by oil of wine. The contents of the retort boiled over on the continued application of heat.

The first five products consisted of æther mixed with alcohol, which last was contained in the retort as such, and not converted into sulphovinic acid, and evaporated from the mixture in company with the æther. The first product, which distilled over at the lowest temperature, contained, to judge from its specific gravity, much æther, and little alcohol, quite opposed to the general opinion that æther is only formed at the boiling-point of the mixture. The succeeding products gradually became, according to their specific gravity, constantly more æthereal, and contained less alcohol; but only in the sixth product was there so much water that it separated, and the quantity increased in proportion as the distillation was continued.

The first six products smelt but slightly of oil of wine; but the seventh contained a portion, and also smelt of sulphurous acid. After the first seven products had been mixed together, and the separated water removed, they had a specific gravity of 0·788.

It is well known that æther is prepared, of late, in the most advantageous manner, by allowing a small stream of alcohol to flow constantly into a mixture of alcohol and the hydrate of sulphuric acid. It has been denied that the presence of sulphovinic acid is of essential influence in the formation of æther, and asserted that it is not necessary that the formation of this acid should precede that of æther, because in the method of preparing æther alluded to, the boiling mixture must be constantly at a temperature of 140° cent., at which sulphovinic acid could not exist. But at the point where the current of cold alcohol flows into the boiling mixture, the temperature is under 140° C. The sulphovinic acid formed is decomposed it is true, in a very short time, from its soon acquiring the temperature of the boiling liquid. The preparation of æther, according to the above method, consists therefore in a constant formation, and continual decomposition of sulphovinic acid. It is a pretty generally entertained opinion that the production of æther from a mixture of alcohol and sulphuric acid, is solely effected by the boiling of the mixture, which takes place at a high temperature, about 140° C. In many works on chemistry we meet with the assertion that when a mixture of sulphuric acid and alcohol are heated at a temperature, not high enough for it to boil, no æther, but merely anhydrous alcohol, is obtained.

Were this assertion correct, it would be an important objection to the hypothesis I have advanced; for, according to that, it would be somewhat difficult to explain the circumstance why the oxide of æthyl is separated at a lower temperature, as a dydrate, and at a higher one in an anhydrous state.

But this common opinion is founded on an error, which to me is quite incomprehensible. Æther is obtained even from a mixture of the hydrate of sulphuric acid and anhydrous alcohol, when distilled in a water-bath, at a temperature which need not always amount to the boiling heat of water. It is not indeed requisite to employ anhydrous alcohol, but the hydrated, of 90 per cent. Tralles*, to obtain æther from a mixture at the above-mentioned temperature.

Mr. Wittstock, at my request, had the goodness to institute a series of experiments on this point, and communicated the result to me.

1. Fifteen ounces of anhydrous alcohol were mixed in the cold, with an equal weight of the hydrate of sulphuric acid, and the mixture distilled at a temperature at which it could not boil strongly. The products, well cooled, were successively received, and the temperature at which they passed over accurately noted.

First product: 1 dr. 10 grs., spec. gr. 0.817, passed over at from	60° to 80° R.
Second product: 3 oz. 1 dr. 10 gr., spec. gr. 0.792, passed over at from......	90° —93° „
Third product: 3 drs. 57 grs., spec. gr. 0.772, passed over at from	75° —80° „
Fourth product: 2 oz. 40 grs., spec. gr. 0.749, passed over at from	90° —95° „
Fifth product: 5 drs.	

When the mixture had reached the temperature of 90° it began to boil very slightly; the boiling, however, subsequently ceased at this temperature, but even then æther was disengaged from the mixture in bubbles, just as carbonic acid gas escapes at the common temperature from a liquid strongly saturated with it.

From these experiments it is evident that æther is formed at far lower temperatures than is usually supposed. The first product smelled indeed strongly of æther; but chiefly consisted, which is also indicated by the specific gravity, of alcohol, which had not been converted, by mixing with sulphuric acid, into sulphovinic acid; æther could not be separated from it, either by water or even by chloride of calcium. The second, third, and fourth products consisted, on the contrary, principally of æther, which could even be separated by mere washing with water. The fifth was the first that contained free water, and indeed, in volume, more than the half. The specific gravity of the æthereal liquid floating above it was not determined. This last product distilled over very slowly, although at times the temperature was raised to 100° R.

---

* That is 90 per cent. absolute alcohol by volume.

It results from these experiments that æther which is produced at lower temperatures than is requisite to boil the mixture, is at the same time purer, and contains less alcohol and water than æther which has been prepared by strong boiling. A comparison of the specific gravities with those previously mentioned, set this evidently beyond all doubt. At a low temperature the water especially escapes later, and therefore only in the last product could separated water be observed, a proof that it is not disengaged in company with the æther.

II. A second series of experiments proved this in a still more decided manner, so that there can no longer remain any doubt on the subject that æther can be evolved in abundance at the boiling-point of water.

Seventeen ounces of anhydrous alcohol of specific gravity 0.792 were mixed cold with 18 ounces of the hydrate of sulphuric acid, and the mixture subjected to distillation in a water-bath whose temperature frequently did not even attain that of boiling water. The quantities taken are in the proportion of single equivalents of each of the substances employed; they were taken in this proportion, partly because it approaches that which otherwise is employed in the preparation of æther, when equal parts by weight of alcohol and sulphuric acid are employed, and also in order to have no excess of sulphuric acid.

The results of the experiments are as follows:—

First product: 3 drachms.
Second product: 3 ounces 6 drachms; spec. gr. 0·755.
Third product: 3 drachms; spec. gr. 0·745.
Fourth product.

Even the first product consisted of nearly pure æther; for a solution of acetate of potash separated æther from the liquid to the amount of two thirds of its volume.

The fourth and last products contained free water, and consisted of nearly half of it by volume; but it distilled over so slowly in the water-bath, that several hours were necessary to obtain a few drachms of it. From the specific gravities it will be perceived that the second, and especially the third product, consisted of æther far more pure than is obtained in other modes of preparing that substance.

III. As the idea is so general, that æther is formed from a mixture of alcohol and sulphuric acid only on boiling, and as in the usual mode of distilling, hydrated, and not anhydrous alcohol is employed, a new series of experiments were performed with the former.

A pound of alcohol of 90° Tralles, such as is usually employed in the preparation of æther, was mixed in the cold with a pound of the hydrate of sulphuric acid, and the mixture subjected to distillation in a water-bath, as in the second series of experiments. The results were:

First product: 4 drs. 36 grs.; spec. gr. 0.833.
Second product: 2 oz. 4 drs. 20 grs.; spec. gr. 0.787.
Third product: 4 drs. 50 grs.; spec. gr. 0.789.
Fourth product: 5 drs. 17 grs.; spec. gr. 0.789.
Fifth product.

The first product consisted almost entirely of alcohol, as indicated by the specific gravity. The succeeding ones contained much æther, or consisted mostly of it. Free water also was evident in this case only in the fifth and last product, which consisted of one drachm of liquid, of which only one fourth was separated water. To distil this small quantity over, it was necessary to heat for more than five hours.

The æther obtained from a mixture of sulphuric acid and alcohol, at the temperature of boiling water, is far more pure, as may be anticipated, and is indicated by the specific gravities of the products, when anhydrous, instead of hydrated, alcohol is employed. The æther obtained from hydrated alcohol in this way contains more alcohol, because upon mixing hydrated alcohol with sulphuric acid, less is converted into sulphovinic acid, and more remains in a free state in the mixture, than when absolute alcohol is used. According, however, to the theory advanced in this memoir, only that portion of the alcohol can produce æther which has been converted into sulphovinic acid, and this æther distils over when heated, in company with the free alcohol.

The fact that æther is produced from a mixture of alcohol and sulphuric acid even at the boiling-point of water, is indeed highly important in the theory of the formation of æther, and by this method the æther is also obtained more pure, especially from water, and of a far lower specific gravity than when distilled at a boiling heat; but it is not convenient in the preparation of æther, in so far as at this low temperature the æther, and particularly the last products, pass over with great slowness.

One fact, however, seems not to admit of being quite satisfactorily explained by the present theory. Seeing that water acts as a base upon the oxide of æthyl, and disengages it from its combinations, it must appear surprising that stronger bases than water do not effect this separation still more perfectly. But solutions of the sulphovinate of potash and soda may be treated with an excess of potash without the oxide of æthyl being expelled; and even the salts of the alkaline earths can exist in contact with an excess of base.

But there seems to be a difference in properties between the double compound of the hydrate of sulphuric acid with the sulphate of the oxide of æthyl and the other sulphovinates. The former is far easier decomposed by water than the latter; but this fact is by no means without analogy. Water is able to decompose many salts of the oxide of antimony, and displace the latter from these combinations as a basic salt; but the combinations of the oxide of antimony with tartaric acid, and other unvolatile organic acids, are not decomposed by water.

According to the earlier method in use, æther was obtained from a mixture of equal parts, by weight, of sulphuric acid and alcohol; but there is more alcohol at the commencement than is requisite. In the progress of the distillation, however, the quantity of sulphuric acid becomes constantly predominating, in proportion as the alcohol passes over as æther; and from the great excess of the hydrate of sulphuric acid, the liberated æther is itself decomposed by the boiling, which in this case takes place at a high temperature, and is then first converted into a double compound of the sulphate of the oxide of æthyl with sulphate of ætherol (Weinöl.); and lastly changed by the boiling into olefiant gas, from the presence of too great a quantity of the hydrate of sulphuric acid, and from too high a temperature.

This change of æther into oil of wine and olefiant gas, by an excess of sulphuric acid and too high a temperature, is not the result of a mere deprivation of water, as might be concluded from a comparison of the composition of these substances with that of æther; for as soon as the slightest trace of oil of wine is evident in the formation of the æther, a corresponding trace of sulphurous acid is disengaged, the quantity of which becomes more considerable if olefiant gas is formed. The production of sulphurous acid stands therefore in definite connexion with that of the oil of wine and olefiant gas. Since the origin of these two bodies takes place only at a high temperature, especially that of the olefiant gas, these substances undoubtedly owe their origin to a similar action of sulphuric acid on æther, as this acid exerts on other bodies of organic origin at high temperatures. The sulphuric acid is coloured black by these, at the high temperature, with the evolution of sulphurous acid and separation of a carbonaceous substance; the same also takes place in the distillation of æther, when continued to the production of oil of wine and olefiant gas.

The origin of this coaly matter, which has recently been examined by Erdmann and Lose*, stands therefore in connexion with that of the sulphurous acid, oil of wine, and olefiant gas; consequently the formation of this body is the result of another process, which very likely has nothing to do with the formation of the æther.

When therefore æther is prepared from a mixture of sulphuric acid and alcohol at a very low temperature, it is perfectly free from oil of wine; and, in fact, not a trace of that substance could be observed in the first products which where obtained by the above distillations, not only in those that were performed in the water-bath, but also in those which were carried on at a gentle heat in the sand-bath. Even the last products appeared to be perfectly free from it; but if a considerable quantity of the æthereal liquid was evaporated on blotting paper, a very slight smell of it might be discovered, a trace however so insignificant, that individuals not well acquainted with the odour of oil of wine could not perceive it. Moreover, when the distillation was at an end, the residuum in the retort was, it is true, of a dark colour, but not deep so that it resembled a brownish vitriol, such as frequently occurs in commerce; the residue smelt as slightly of sulphurous acid as the distilled æther did of oil of wine. Not a trace of carbonaceous substance was separated. The process by which oil of wine is produced, commences, therefore, in the mixture prepared for the distillation of æther, even at the boiling-point of water, at least when this is long continued; but even then the formation of this body at that temperature is quite trifling in amount.

---

* Poggendorff's *Annalen*, vol. xlvii. p. 619.

When æther is distilled from a mixture of sulphuric acid and alcohol in the water-bath, we obtain, as is evident from the above results, less æther than we might expect from the quantity of alcohol employed, and the residue weighs more in proportion. In the last series of experiments described, in which æther was prepared in the water-bath, the residuum, on employing 17 ounces of absolute alcohol and 18 ounces of sulphuric acid, weighed 27 ounces, and the distilled alcohol æther $4\frac{1}{2}$ ounces; the loss consisted partly in the water distilled, the quantity of which was not determined, in volatilized æther, which in this case volatilized the more, as it was nearly pure, and also in the loss which occurs by pouring out. On employing one pound of hydrated alcohol and one pound of sulphuric acid, the residuum weighed $26\frac{1}{2}$ ounces, the products 4 ounces and some drachms; the loss consisted partly in the water which passed over, the quantity of which was not accurately determined. In both cases therefore, besides water, æther also remained with the sulphuric acid, undoubtedly as isæthionic acid, probably also in part as æthonic acid. It is very probable that the products which present themselves with æther in a distillation when long continued and at high temperature, are produced, not by the direct decomposition of the æther, but by the decomposition of the isæthionic acid, occasioned by the excess of sulphuric acid and a high temperature; such as the precipitated carbonaceous substance, the sulphurous acid, oil of wine, and lastly, the olefiant gas.

It is well known that the formation of these products is generally avoided in the preparation of æther by the new and most profitable method, in which, as æther passes over, a like quantity of alcohol is allowed to flow into the boiling mixture. The action of an excess of sulphuric acid on the alcohol, or rather on the isæthionic acid, at a high temperature, is thus prevented.

When formerly the production of æther was sought to be explained by the subtraction of the water from it, by means of sulphuric acid, it might with much justice be objected to the present explanation, that other bodies, which have, like sulphuric acid, a great affinity to water, such as the hydrate of potash, chloride of calcium, &c., are not able to transform alcohol into æther; but this objection now falls entirely to the ground, as we know that the æther is not formed by any subtraction of water, but by the decomposition of the sulphovinic acid.

If æther is regarded as a base, then all the theories on the formation of æther are not capable of satisfactorily explaining how a base is discharged from a strongly acid liquor, and by a powerful acid. It is only by the present explanation, and by the analogy which the separation of æther from sulphovinic acid bears to the decomposition of several inorganic salts by means of water, and also by the above-mentioned analogy of æther with a series of oxides which do not, or to a very slight extent, combine with acids, that this phenomenon loses its anomalous appearance.

It seems to me highly desirable in organic chemistry, to illustrate its processes always as much as possible by analogous processes in inorganic chemistry. The greatest advantages have accrued to organic chemistry by the endeavours of Berzelius, Liebig, and Dumas, who have pursued this path, frequently starting, it is true, from very different views.

It is certainly advantageous in so imperfect a science as chemistry, and especially organic chemistry, to ascribe provisionally to a common force all phænomena which stand isolated, for which no suitable analogies can be detected, and which on this account appear wonderful, and thus openly to admit that in the present state of science it is better to avoid explaining a process altogether, than to explain it by some artifice or in a constrained manner. The smaller the number of phænomena which we are compelled to refer to this class, the more perfect the science becomes.

Setting out from this point of view, I have ventured to explain a process in organic chemistry, which has long, and particularly of late years, engaged the attention of chemists, as being analogous to several processes in inorganic chemistry; and if the explanation should not give general satisfaction, the attempt to attain so important an object, will, I trust, meet with approbation.

The present theory is valid, it is true, only for the formation of æther from a mixture of alcohol and sulphuric acid; but quite a similar one may undoubtedly be advanced for the formation of æther from mixtures of phosphoric and arsenic acids with alcohol. For the present, however, I leave it undecided whether the formation of æther, by treating alcohol with fluoboracic gas, as also with the chloride of zinc and other chlorides, is to be explained by a mere subtraction of water by these substances; or in this way, that they form with alcohol, at the common temperature, combinations analogous to sulphovinic acid, which are decomposed like it, at a high temperature, by the agency of water. The latter view I regard as being the most probable.

POSTSCRIPT.

In the preceding Memoir I have compared the formation of æther from a mixture of sulphuric acid and alcohol, with the decomposition of several inorganic salts by means of water; I have endeavoured to show that it is the water which in these cases acts the part of a base, and separates the oxide of æthyl or the metallic oxide, the latter generally as basic salt.

The inorganic salts which I enumerated in this comparison as examples, were those of the oxide of bismuth, the oxide of mercury, and of antimony. These undergo the said decomposition by water even at the common temperature; æther, however, is first separated from a mixture of sulphuric acid and alcohol, or from sulphovinic acid, at a high temperature.

There are, however, among the inorganic weak bases, a considerable number which are eliminated by water, from their combinations with acids only at a high temperature; and the decomposition of the salts of these bases, by means of water, is therefore still more fit to be compared to the formation of æther.

To these bases belongs more especially the peroxide of iron, which is precipitated by water as basic salt from solutions of most of its neutral salts at a high temperature. The weaker the solution of the salt of peroxide of iron, the lower is the temperature which occasions precipitation, and the more completely is the peroxide of iron thrown down, so that with a certain dilution as M. Scheerer[*] has shown, scarcely a trace of the peroxide of iron remains in solution, but the entire quantity is separated as basic salt. As stronger bases are not precipitated by water on boiling, this property of the peroxide of iron has been employed to separate it from the oxides of cobalt, nickel, and other metals[†]. It may even be separated, by boiling the solution, from alumina, which, although it has with regard to its properties much similarity to the peroxide of iron, is evidently a stronger base; this separation of alumina from the peroxide of iron by means of water at a high temperature, is of some importance to the arts, as in the fabrication of alum the peroxide of iron contained in the mother-liquor is precipitated by mere boiling, and is thus more easy to separate from the alumina than the protoxide of iron, although the former, with sulphuric acid, and an alkali, forms an alum which has quite an analogous composition with alumina-alum; and, from being isomorphous with that alum, could crystallize with it in all proportions.

Several other bases have the same property as the peroxide of iron, which like it belong to the class of weaker bases, and also several substances which act as bases towards strong acids, and also as acids towards strong bases, and which on that account are frequently classed among the acids. Among these are the oxide of zirconium, thorina, the peroxide of cerium, peroxide of tin, titanic acid, tellurous acid, columbic acid; also in certain respects molybdic acid, tungstic acid, and vanadic acid. Several combinations of these oxides with acids are soluble in the cold in water, and are precipitated from the solution, on boiling, as oxides or basic salts.

Several of the oxides precipitated in this manner possess, after precipitation by boiling, properties which they do not evince before their solution in acids and precipitation; they are more indifferent than before, are partly of difficult solution in acids, partly insoluble, and do not combine after precipitation with them, even when these are employed in a concentrated state. Titanic acid, peroxide of tin, and many others may be classed here. This peculiarity is in a certain degree analogous to that of æther, which, when it has been once separated by boiling from a mixture containing sulphovonic acid, appears not to combine directly with acids.

LI.—*On Galvanic results, in letters addressed to Prof. Silliman, October 4, 1838, and August 6, 1839, from the vicinity of London;* by WILLIAM STURGEON, Esq.

REMARKS BY THE EDITORS.

A very economical and efficient voltaic arrangement was adopted by several members of the London Electrical Society, and the report of the construction and performance of the battery, in a series of experiments performed at Clapham Common in the autumn of 1838, is contained in the report of Mr. Charles V. Walker, published in the Transactions of the London Electrical Society, in two papers dated October 16, and November 6, 1838. In allusion to this battery, Mr. Sturgeon observes, in his letter of October 9, 1838:—

"A voltaic battery has been got up (at the expense of two of our leading men, whose names I am not at liberty to mention,) for the sole purpose of investigation. The battery consists of one hundred and sixty porcelain pint jars, each containing a copper and zinc cylinder; the latter being covered with stout brown paper, is introduced to the interior of the copper. The exciting fluids are solutions of sulphate of copper and muriate of soda; the former applied to the copper cylinders, and the latter to the zinc ones. When the jars were in series the flame was upwards of an inch long, from a charcoal point, rotated on the poles of a magnet, according to the principles of electro-magnetism. Davy *deflected* the electrical flame by magnetic influence, but I am not aware that he *rotated* it."

"Sulphuret of lead (galena) was decomposed, and metallic lead obtained. Sulphuret of antimony was decomposed, and the liberated metal kept in fusion for several minutes. The boiling antimony was *three inches long* and half an inch wide between the polar wires, and exhibited a beautiful spectacle, in a channel of those dimensions which the action had formed in the native sulphuret. When the electric flame was directed through the air between stout copper polar wires, the positive wire became red hot, but the negative wire could not be made red. The wires were made to change poles, still the same thing occurred: nay, even two inches of the positive wire, which was completely out of the circuit, was rendered hot, but no redness appeared on the negative wire.' How exceedingly curious and interesting is this last result!

"When the whole battery was formed into eight groups of twenty jars each, and properly connected with an electro-gasometer, the mixed gases were liberated from water at the rate of one cubic inch per seven seconds: and this for many successive minutes, although the battery had been in action for seven previous hours without interruption."

In his letter of August 6, 1839, Mr. Sturgeon proceeds to observe, that a good description of the apparatus and experiments will be found in the memoir above named, and of which he kindly transmitted a copy. But he remarks: "there are some particulars connected with the discovery of the *difference of temperature*, produced in the positive and negative wires, which want a clearer description than any given by Mr. Walker, or, perhaps any which that gentleman had then a means of giving; and, as I find, from the defective information which has been given of this particular discovery on the continent of Europe, that M. De la Rive and others, have failed in reproducing the curious phenomenon, it is possible that the American philosophers may also fail from a like cause, were the particulars of manipulation not made known to them. I will, therefore, for the information of all the readers of your excellent journal, give a brief historical sketch of the whole business."

"The battery consisted of a hundred and sixty white porcelain jars, each of the capacity of about two thirds of a pint, and furnished with a hollow cylinder of sheet copper, and an interior hollow cylinder of sheet zinc, the latter amalgamated, and in metallic connexion with the copper of the next pot, &c. The copper and zinc of each pot were separated from each other by a diaphragm of brown paper, (a disc, on the centre of which is placed the centre of the base of the zinc cylinder, and the periphery brought up to the upper end of the latter so as to form a bag round the zinc,) which separates the solution of sulphate of copper, which is placed *outside*, from the solution of common salt, which is placed *inside* of it. Hence the copper is washed with its sulphate solution, and the zinc with the muriate of soda solution.

"One hundred of these metals and pots were furnished by Mr. Gassiot, and the other sixty by Mr. Mason. The preparation of a battery of this kind and extent is a great labour, as you will understand from the following particulars. Mr. Walker commenced working at it between eight and nine in the morning; Mr. Mason arrived about eleven in the forenoon, and immediately set to work at it; Mr. Gassiot commenced shortly afterwards, and it was not ready for experiment till three in the afternoon, about an hour and a half after I arrived at Mr. Gassiot's house. The plan of dividing the battery into groups for the experiments on decompositions was formed by Mr. Mason, who is a very skilful and neat experimenter.

"At a previous meeting I was requested to provide a catalogue of experiments, which I did; but in consequence of the great length of time occupied in the experiments on the decomposition of water by the various forms of the battery, only a few of them were attempted. As the decompositions are very well described by Mr. Walker, it would be unnecessary to say anything more about them in this place. They were carried on with great exactness in the following manner. The graduated glass tube of the electro-gasometer being filled with acidulated water and inverted over the platinum terminals of the instrument, one of the polar wires of the battery was connected with it, and the other kept in the hand of the experimenter ready to plunge into the other mercurial cup of the instrument the moment the word "time" was given, and taken out again when a cubic inch of the gases was collected.

"With regard to the experiment in which I discovered the great difference produced in the two polar wires, it was undertaken from the views which I had long entertained concerning the non-identity of the *electric* and *calorific* matter, as you will see I have hinted at, at the close of section 1, of my first memoir to the London Electrical Society. It was late in the evening before I had any opportunity of making the experiment. The rest of the party were engaged in something else at the time, and the battery was in series of one hundred and sixty pairs. I brought the tip ends of the polar wires (copper wire one tenth of an inch diameter) into contact, end to end, then withdrew them gently and very gradually from each other, keeping the flame in full play between them till they were separated about one-fourth of an inch. In a few minutes the positive wire got red hot for half an inch, but the negative wire never became red. I repeated this several times, in order to be convinced of the fact. I next laid the wires across one another, and brought them into contact about an inch from the extremities, and separated them as before. In a short time the whole of that part of the positive wire from the point of crossing to the extremity, became very red hot, but the negative end never got even to a dull redness. It was certainly very hot, but never higher than a black heat. I next increased the length of the ends of the wires *exterior* to the circuit; and eventually heated two inches of the positive wire to bright redness; but no such heat took place on the other wire Thus satisfying myself that I was not mistaken, I called Mr. Mason to come and look at it: and after satisfying that gentleman by an experiment or two, we called Mr. Gassiot and Mr. Walker to come and witness the novel phenomenon. We now changed the places of the polar wires, making that positive which before had been negative, &c. Still the positive wire showed the same fact. You will easily understand that I experienced a great degree of pleasure at the appearance of this beautiful fact, which seemed to demonstrate the justness of the hypothesis I had so long formed. *No two bodies can be in the same place at the same time*, is an old axiom in philosophy. Hence the blacksmith is enabled to heat his iron rod or nail, by compressing the calorific matter; the blows of his hammer forcing it from the *cavities* into the *particles* of the metal. Thus, also, the electric fluid forces the calorific matter from its natural lodgings in the conductor, and drives it on even to beyond the electrical stream, to take refuge, in a compressed form, in the extremity of the positive wire. Nothing can be more simple to explain; nor do I know of an experiment that tends more to support the doctrine of *one species* of electric matter only; and that it runs through the voltaic conducting wires, *from* the positive *to* the negative pole.

"To produce the phenomenon I have been describing, requires an extensive series of pairs; certainly not less than one hundred and twenty, but two hundred would answer much better, as much depends upon the play of the fluid between the wires; and I think that the battery is quite as well when not highly charged. I have mentioned one hundred and twenty as the shortest to insure success, although it is possible that one hundred might show the fact."

The following remarks, in answer to inquiries made of Mr. Sturgeon as to his views regarding the best forms of galvanic batteries, are worth preserving, as the conclusions of so experienced an experimenter, and the more so as they coincide generally with the views of Dr. Hare, and of other distinguished men in this country.—*Eds.*

Form and size of Galvanic Batteries.

"With respect to galvanic batteries, we can never expect to find *one* which will exhibit every class of phenomena to the best advantage. The pile, with moistened card board in pure water, or a well constructed Cruikshank, charged with water, answers best for charging Leyden jars, deflections of pith balls, &c. And the more extensive the series the better. The size of the plates has also much to do in this business. A single pair of plates, charged with dilute nitrous acid, answers best for most electro-magnetic experiments. For a display of *brilliant* calorific phenomena, the burning of charcoal, deflagration of laminated metals, &c, a series of not less than a hundred pairs answers better than any smaller series. Here again, the size of the plates should never be less than four inches square. Six inch plates answer much better, and two hundred better than one hundred, &c. And these may be either of the Cruikshank form, or of any other, observing that the action with the former is of much shorter duration than with the Wollaston form, and shorter with the Wollaston than with the battery before described.

"Then again, for heating of thick wires, a series of ten or less, of *large plates*, are better than more extensive series.

"For chemical decompositions, there is, perhaps, no battery known so well adapted for them as the jars which I have described. Their sustaining power is a great recommendation. The extent of series will necessarily vary with the nature of the compound operated on. We have found that a series of twelve jars gives a sufficient intensity for the decomposition of acidulated water, (water 10, sulphuric acid 1, or even much less.) Twenty-four jars in a double series of twelve, give about twice as much gas as a single series of twelve. But twenty-four jars in a single series do not give so much gas as when they form a double series of twelve. Again, thirty-six jars in one series, do not give so much gas as when they are formed into a treble series of twelve. Hence a series of twelve of *these* jars seems to be about the best *unit of intensity* for acidulated water. Other compounds will require other *units of intensity* to produce maximum effects—and other batteries will require different extent of series to produce the same *unit of intensity* as that produced by the jars."—*Letter to Prof. Silliman.—(From Silliman's Journal.)*

BRITISH ASSOCIATION PROCEEDINGS AT GLASGOW, 1840.

GENERAL MEETING.—THURSDAY, SEPTEMBER 24.

In consequence of the absence of the Rev. Vernon Harcourt, the President of the past year, the Marquis of Northampton took the chair. He lamented the unavoidable absence of Mr. Harcourt, who had taken so active a share in the formation of the Society, and had been one of its most zealous supporters. He congratulated the Association on assembling in a city equally remarkable for its extensive commerce and great manufacturing industry, and the seat of an ancient university, which had rendered eminent service to the united cause of literature, science, and humanity. Glasgow, the native town of Watt, had taken the lead in the practical application of steam as a moving power, and the animating display of steamers on the Clyde united the triumphs of art to the most romantic scenery of nature. He felt great pleasure in introducing their new President, the Marquis of Breadalbane; and, after a brief reference to the services which the Association had rendered to science, he resigned the chair.

The Marquis of Breadalbane, on taking the chair, stated his sense of the honour conferred on him, and observed, that it was unthought of, and unlooked for on his part; and he was afraid he had no claim to it, save that of one who had a firm conviction of the vast importance and value of science, and an earnest wish to support its best interests by every means in his power. It was unnecessary, he observed, in such a meeting, composed as it was of some of the greatest ornaments of our own country, and many of the highest character in science in foreign countries, to dilate on that bond of union which it presented for promoting the great object—the investigation of truth. The British Association had conferred great and valuable benefits upon the nation, and even the world at large. He adverted to the propriety of such a meeting being held in Glasgow, a city combining in itself more perhaps than any other in the empire, the elements of national wealth—commerce and manufactures. He then called on Mr. Murchison to read—

The Address of the General Secretaries.

In entering upon the duty assigned to us, we heartily congratulate our associates on this our second assembly in Scotland. As on our first visit, we were sustained by the intellectual force of the metropolis of this kingdom, so now, by visiting the chief mart of Scottish commerce, and an ancient seat of learning, we hope to double the numbers of our northern auxiliaries.

Supported by a fresh accession of the property and intelligence of this land, we are now led on by a noble Marquis, who, disdaining not the fields we try to win, may be cited as the first Highland chieftain who, proclaiming that knowledge is power, is proud to place himself at the head of the clans of science.

If such be our chief, what is our chosen ground? Raised through the industry and genius of her sons, to a pinnacle of commercial grandeur, well can this city estimate her obligations to science! Happily as she is placed, and surrounded as she is by earth's fairest gifts, she feels how much her progress depends upon an acquaintance with the true structure of the rich deposits which form her subsoil; and, great as they are, she clearly sees that her manufactures may at a moment take a new flight by new mechanical discoveries. For she it is, you all know, who nurtured the man whose genius has changed the tide of human interests, by calling into active energy a power which (as wielded by him), in abridging time and space, has doubled the value of human life, and has established for his memory a lasting claim on the gratitude of the civilized world. The names of Watt and Glasgow are united in imperishable records!

In such a city then, surrounded by such recollections; encouraged by an ullustrious and time-honoured University, and fostered by the ancient leaders of the people, may we not augur that this meeting of the British Association shall rival the most useful of our previous assemblies, and exhibit undoubted proofs of the increasing prosperity of the British Association?

Not attempting an analysis of the general advance of science in the year that has passed since our meeting at Birmingham, we shall restrict ourselves, on the present occasion, to a brief review of what the British Association has directly effected in that interval of time, as recorded in the last published volume of our Transactions. From this straight path of our duty we shall only deviate in offering a few general remarks on subjects intimately connected with the well being and dignity of our Institution.

One of the most important—perhaps the most important service to science—which it is the peculiar duty of the Association to confer, is that which arises from its relation to the Government—the right which it claims to make known the wants of science, and to demand for them that aid which it is beyond the power of any scientific body to bestow. In the fulfilment of this important and responsible duty, the Association has continued to act upon the principle already laid down in the Address of the General Secretaries at the Meeting at Newcastle in 1838, namely, to seek the aid of Government in no case of doubtful or minor importance; and to seek it only when the resources of individuals, or of individual bodies, shall have proved unequal to the demand. The caution which it has observed in this respect has been eminently displayed in the part which it has taken with reference to the Antarctic expedition, and to the fixed Magnetical Observatories. It abstained from recommending the former to the Government until it had called for and obtained from Major Sabine, by whom the importance of such an expedition was first urged, a report in which that importance was placed beyond all doubt; and it withheld from urging the latter, although its necessity was fully felt by some of its own members, until the letter of Baron Humboldt to the Duke of Sussex gave authority and force to its recommendation.

The delay which has in consequence occurred, has been productive of signal benefit to each branch of this great twofold undertaking. Since the time alluded to, our view of the objects of investigation in terrestrial magnetism have been greatly enlarged, at the same time that they have become more distinct. Major Sabine's memoir on the Intensity of Terrestrial Magnetism, has served to point out the most interesting portion of the surface of the globe as respects the distribution of the magnetic force, and has indicated, in the clearest manner, what still remained for observation to perform: and the beautiful theory of M. Gauss, which has been partly built upon the data afforded by the same memoir, while it has assigned the most probable configuration of the magnetic lines of declination, inclination, and intensity, has done the same service with respect to all the three elements.

In another point of view, also, delay has proved of great value to both branches of the undertaking, but more especially to the fixed observatories. Our means of instrumental research have, since the time of their first projection, received great improvements, as well in their adequacy to the objects of inquiry, as in their precision; and finally, the two great lines of inquiry—the research of the distribution of terrestrial magnetism on the earth's surface, and the investigation of its variations, secular, periodic, and irregular,—have been permitted to proceed *pari passu.*

Last of all, the prudent caution, and vigilant care, which the two great scientific bodies have exhibited, both in the origin and progress of the undertaking, have naturally inspired the government with confidence; and while on the one hand science has not hesitated to demand of the country all that was requisite to give completeness to a great design, so on the other, the government of the country has not hesitated to yield, with a liberal and unsparing hand, every request the importance of which was so well guaranteed.

But while we thus enumerate the benefits which have resulted to magnetical science from the delay, it must be also acknowledged that something has been lost also, not to science, but to British glory. Although terrestrial magnetism stood forward as the prominent object of the Antarctic expedition, yet it was also destined to advance our knowledge of the "*physique du globe,*" in all its branches, and especially in that of geography. Had the project of an Antarctic expedition been acceded to when it was first proposed, viz., at the meeting of the British Association in Dublin, in 1835, there can be no reasonable doubt, that a discovery, which, by its extent, may almost be designated a Southern Continent, situated in the very region to which its efforts were to have been chiefly directed, must have fallen to its lot; and the flag of England been once more the first to wave over an unknown land. But while, as Britons, we mourn over the loss of a prize which it well became Britain and British seamen to have made their own, it is our part too as Britons, as well as men of science, to hail the great discovery—one of the very few great geographical discoveries which remained unmade;—and to congratulate those by whom it has been achieved, those whom we are proud to acknowledge as fellow-labourers, and who have proved themselves in this instance our successful rivals in an honourable and generous emulation.

The caution which has characterized the British Association in the origination of this great undertaking, has been followed up by the Royal Society in the manner in which it has planned the details, and in the vigilant care with which it has watched over the execution. Of the success which has attended this portion of the work, the strongest proof has been already given in the unhesitating adoption of the same scheme of observation by many of the continental observers, and in the wide extension which it has already received in other quarters of the globe. All that yet remains is to provide for the speedy publication of the results. The enormous mass of observations which will be gathered in, in the course of three years, by the Observatories established under British auspices, and by the Antarctic expedition, will render this part of the task one of great expense and labour. To meet the former, we must again look to the Government, and to the East India Company, who will certainly not fail to present the result of their munificence to the world in an accessible form. The latter can only be overcome by a well organized system. The planning of this system will, of course, be one of the first duties of the Royal Society; and it is important that it should be so arranged, that while every facility in the way of reduction may be given to those who shall hereafter engage in the theoretical discussion of the observations, care is taken at the same time that the data are presented entire, without mutilation or abridgment. The Council of the Royal Society will, doubtless, be greatly assisted in this duty by the eminent individual who has had in every way so large a share in the formation of these widely scattered magnetic establishments, and whose own Observatory, founded by the munificence of the Dublin University, has nearly completed twelve months' magnetic observations on that enlarged and complete system of which it set the first example.

In referring, as we have done, to those most valuable services which the Royal Society have rendered, and are continuing to render, in directing and superintending the details of this great undertaking in both its branches, it is right that, on the part of the British Association, we should express the cordial satisfaction and delight with which we witness their exertions, united with our own in this common cause; nor should we omit to recognize how much this desirable concurrence has been promoted by the influence of the noble President of the Royal Society, the Marquis of Northampton, whom, as on so many former occasions, we have the pleasure of seeing amongst us, as one of our warmest supporters and most active members.

In the volume of our Transactions now under notice, is contained the memorial presented to Lord Melbourne by the Committee of the British Association, appointed to represent to her Majesty's Government the recommendation of the Association on the subject of terrestrial magnetism. This memorial is one of many services which have been rendered to our cause, by Sir John Herschel, whose name, whose influence, and whose exertions, since our meeting two years since at Newcastle, have largely contributed to place the subject where it now stands. The devoted labour of other of our members has long been given to an object which they have had deeply at heart, viz. the advancement of the science of terrestrial magnetism; but the sacrifice which Sir John Herschel has made of time, diverted from the great work in which his ardent love of astronomy, his own personal fame, and his father's memory, are all deeply concerned, the more urgently demands from our justice a grateful mention,—because the science of magnetism had no claim on him, beyond the interest felt in every branch of science, by one to whom no part of its wide field is strange, and the regard which a national undertaking such as this deserved, from the person who occupies his distinguished station amongst the leaders of British science.

The advancement of human knowledge, which may be reckoned upon as the certain consequence of the Antarctic expedition (should Providence crown it with success), and of the arrangements connected with it, is of so extensive a nature, and of such incalculable importance, that no juster title to real and lasting glory than it may be expected to confer, has been earned by any country, at any period of time; nothing has ever been attempted by England more worthy of the place which she occupies in the scale of nations. When much which now appears of magnitude in the eyes of politicians has passed into insignificance, the fruits of this undertaking will distinguish the era which gave it birth, and, engraved on the durable records of science, will for ever reflect honour on the scientific bodies which planned and promoted it, and on the Government, which, with so much liberality, has carried it into effect.

Were the value of this association, gentlemen, to be measured only by the part which it has taken in suggesting and urging this one object, there might here be enough to satisfy the doubts of those who question its utility: to overlook such acts as these, and the power of public usefulness which they indicate, to scrutinize with microscopic view the minute defects incidental to every numerous assemblage of men, to watch with critical fastidiousness the taste of every word which might be uttered by individuals amongst us, instead of casting a master's eye over the work which has been done, and is doing, at our meetings, is no mark of superior discernment and comprehensive wisdom, but is evidence rather of confinement to narrow views, and an indulgence of vain and ignoble passions.

But to proceed with our useful efforts. One of the principal objects of our annual volumes is the publication in the most authentic form of the results of special researches, undertaken by the request, and prosecuted in many instances at the cost, of the Association. It is a trite remark, that if a man of talent has but fair play, he will soon secure to himself his due place in public estimation. We fully admit the truth of this in many instances, and above all where the points of research are connected with commerce and the useful arts; but many also are the subtile threads of knowledge, which, destined at some future day to be woven into the great web in which all the sciences are knit together, are yet not appreciable to the vulgar eye, and, if simply submitted to public judgment, would too often meet with silent neglect. Numberless, we say, are the subjects (and if your Association exceeds a centenary, still more numerous will they be) with which the retired and skilful man may wish to grapple, and still be deterred by his want of opportunity or of means. Then is it that, adopting the well-balanced recommendations of the men in whose capacity and rectitude you confide, you step forward with your aid, and bring about these recondite researches, the result of which in the volume under our notice we now proceed to consider.

The first of these inquiries to which we advert, you called for at the hands of Prof. Owen, upon "British Fossil Reptiles," one of the branches of Natural History, on a correct knowledge of which the developement of Geology is intimately dependent.

The merits of the author selected for this inquiry are now widely recognized, and he has, with justice, been approved as the worthy successor of John Hunter, that illustrious Scotchman who laid the foundation of comparative anatomy in the British isles. That this science is now taking a fresh spring, would, we are persuaded, be the opinion of Cuvier himself, could that eminent man view the progress which our young countryman is making towards the completion of the temple of which the French naturalist was the great architect. It is therefore a pleasing reflection, that when we solicited Professor Owen to work out this subject, we did not follow in the wake of Europe's praise, but led the way (as this Association ought always to do), in drawing forth the man of genius and of worth; and the value of our choice has been since stamped by the approval of the French Institute.

If Englishmen* first perceived something of the natural affinities of Palæosaurians, it was reserved for Cuvier to complete all such preliminary labour. The publication of his splendid chapters on the Osteology of the crocodile and other reptiles, drew new attention and more intelligent scrutiny to these remains; and it ought to be a subject of honest pride to us to reflect that the most interesting fruits of the researches of that great anatomist were early gathered by the English palæontologists, Clift and Home. One of our leaders, whose report on Geology ornaments the volumes of this

---

* Stukeley.

Association, formed the genius *Plesiosaurus*, on an enlarged view of the relation subsisting between the ancient and modern forms of reptile life; while shortly after Buckland established the genus *Megalosaurus*, and Mantell, *Iguanodon* and *Hylæosaurus*, worthy rivals of the *Geo Sauri* and *Moso Sauri* of Cuvier. The other Englishmen who have best toiled in the field, are De la Beche, Hawkins, and Sir Philip Egerton.

Yet although this report is on *British* reptiles, we are fully alive to the great progress which this department has made, and is making, on the Continent, through the labours of Count Munster, Jäger, and Hermann Von Meyer. The last-mentioned naturalist has been for some time preparing a series of exquisite drawings of very many forms unknown to us in England, most of which have been detected in the Muschelkalk, a formation not hitherto discovered in the British isles. Yet despite of all that had been accomplished in our own country or elsewhere, Professor Owen has thrown a new light of classification on this subject, founded on many newly-discovered peculiarities of osseous structure, and has vastly augmented our acquaintance with new forms, by describing sixteen species of *Plesiosauri*, three of which only had been recognizably described by other writers; and ten species of *Ichthyosauri*, five of which are new to science. Such results were not to be obtained without much labour; and previous to drawing up his report, Professor Owen had visited the principal depositaries of *Enaliosauri* described by foreign writers, as well as most of the public and private collections of Britain. This, the first part of Mr. Owen's report, concludes with a general review of the geological relations and extent of the strata through which he has traced the remains of British Enaliosauri. The materials which he has collected for the second and concluding portion of his report, on the terrestrial and crocodilean sauria, the Chelonia, Ophidian, and Batrachian reptiles, are equally numerous, and the results of these researches will be laid before the Association at our next meeting. Deeply impressed as we are with the value of this report, we cannot conclude a notice of it without again alluding to its origin, in the words of Professor Owen himself: "I could not," says he, "have ventured to have proposed to myself the British Fossil Reptilia as a subject of continuous and systematic research, without the aid and encouragement which the British Association has liberally granted to me for that purpose."

Mr. Edward Forbes, whose labours in detecting the difference of species and varieties among the existing marine testacea of our shores have been most praiseworthy, has on this occasion given us a report "On the Pulmoniferous Mollusca of the British Isles." The variations in the distribution of the species in this class of animals, are shown by him to depend both upon climate and upon soil, the structure of the country (or geological conditions) having quite as much share in such varied distribution as the greatest diversity of temperature. The Association has to thank the author for valuable tables, which show both the distribution of the pulmoniferous molusca in our islands, and their relations to those of Europe generally.

From zoological researches let us now turn to physical geology. One of the most interesting fruits of modern experimental research, is the knowledge of the fact, that electrical currents are in continual circulation below the surface of the earth. Whether these currents, so powerful in developing magnetical and chemical phenomena, are confined to mineral veins and particular arrangements of metal and rock, or generally capable of detection by refined apparatus well applied, appeared a question of sufficient importance to deserve at least a trial on the part of the Association. Our present volume records the result of such a trial on the ancient and very regularly stratified rocks of Cumberland, consisting of limestone, sandstone, shale, and coal, so superimposed in many repetitions as to resemble not a little the common arrangement of a voltaic pile. Varied experiments, with a galvanometer of considerable delicacy, failed to detect, in these seemingly favourable circumstances, any electrical current.

The extensive and rapidly increasing applications of iron to public and private structures of all kinds in which durability of material is a first requisite, have made it highly desirable to possess accurate information respecting the nature of the chemical forces which effect the destruction of this hard and apparently intractable metal. The preservation of iron from oxidation and corrosion, is indeed an object of paramount importance in civil engineering. The Association was therefore anxious to direct inquiry to this subject, and gladly availed itself of the assistance of Mr. Mallet, a gentleman peculiarly qualified for such investigations, both from his knowledge as a chemist, and from his opportuities of observation as a practical engineer. An extensive series of experiments has accordingly been instituted by him, with the support of the Association, on the action of sea and river water, in different circumstances as to purity and temperature, upon a large number of specimens of both cast and wrought iron of different kinds. These experiments are still in progress, and the effects are observed from time to time. They will afford valuable data for the engineer, and form the principal object of the enquiry, but a period of a few years will be required for its completion. In the meantime, Mr. Mallet has furnished a report on the present state of our knowledge of the subject, drawn from various published sources, and from his own extensive observations. In this report he examines very fully the general conditions of the oxidation of iron, and how this operation is greatly promoted, although modified in its results, by sea-water; also in what manner the tendency to corrosion is affected by the composition, the grain, porosity, and other mechanical properties of the different commercial varieties of iron. The influence of minute quantities of other metals, in imparting durability to iron, is also considered. Mr. Mallet devotes much attention to the consequences of the galvanic association of different metals with iron, a subject of recent interest from the applications of zinc and other metals to protect iron, which